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RECENT ADVANCES IN SCIENCE

PURE MATHEMATICS. By F. PURYER WHITE, M.A., St. John's College, Cambridge.

THE majority of the papers dealt with in this article have appeared in the *Giornale di Matematiche di Battaglini*, which is published in Naples under the direction of E. Pascal. As might be expected, the two chief departments of mathematics dealt with are Geometry and Integral Equations.

Algebra and Analysis.—G. Usai (*Giornale di Mat.*, **58**, 1920, 114–24) obtains values of the minimum and maximum exponents of an element i in the a-part partition of a number s. For $i = 1$, the numbers had been calculated by Amaldi in 1918, but for other values of i the results are claimed to be new, although the author admits that he has not examined all the very extensive and complicated eighteenth-century literature of the subject. He verifies his results for the partitions of the number 12.

The same author (*ibid.*, **59**, 1921, 11–20) investigates the relation between certain symbols introduced by E. Pascal in the theory of differential forms and others used by Amaldi in his work on the successive derivatives of functions of any number of variables.

The publishing in 1829 of Sturm's *rule of signs* for determining the number of positive and of negative roots of an algebraic equation made a great stir among mathematicians of the time and called forth a number of memoirs aiming at more convenient methods of obtaining the functions on which the theorem is based. Among these was one due to Mollame (1883), which is allied to the classical process of Bézout for determining the resultant of two equations, but which attracted little attention, and in fact, as published, could not be applied without mistakes to equations of degree higher than the third. G. Usai (*Giornale di Mat.*, **61**, 1923, 77–91) gives an account of the method, with the necessary emendations and with illustrations of its use.

Cauchy developed a method, known as the *theory of indices*, for determining the number of roots of an algebraic equation

within a simple area, bounded by segments of rational curves. For the case of a circular area the problem can be reduced to Hurwitz's problem of finding the number of roots with negative real part; M. Bosco (*Giornale di Mat.*, **60**, 1922, 199–212) investigates a similar reduction for any area; he shows, in particular, that the number of roots of the equation $\sum_0^n a_\nu z^{n-\nu} = 0$, with real coefficients, which lie within the ellipse $x^2/a^2 + y^2/b^2 = 1$ is equal to the number of roots with negative real part of the equation

$$\sum_0^n [a(1 + t^2) + 2bt]^{n-\nu} a_\nu (1 - t^2)^\nu = 0,$$

less n.

The determination of the real maxima and minima of a rational fraction of degree n,

$$t = (a_0 x^n + a^1 x^{n-1} + \ldots + a_n)/(b_0 x^n + b_1 x^{n-1} + \ldots + b_n),$$

can be reduced to the determination of the proper double points of the linear series g_n^1 formed by the sets G_n or which the function t takes the same value. For $n = 2$ the discriminant is a quadratic, and no difficulties arise; the g_2^1 is an ordinary involution of pairs of points, and it is known that the existence or not of real double points is characterised by the mutual position of any two pairs. For $n = 3$, the discriminant is biquadratic, and there are still known algebraic rules for distinguishing the cases of 4, 2 or 0 real roots. V. Notari (*Giornale di Mat.*, **60**, 1923, 33–46) substitutes for these algebraic rules geometrical considerations based upon the mutual position any two triads of the g_3^1. He shows that if the two triads separate each other there are no double points, that if they can be isolated there are 4, and that if two points of one triad are enclosed between two consecutive points of the other, or if two points of one or of both are conjugate imaginaries there is ambiguity.

In the last case a further investigation is required; this may be performed by transporting the g_3^1 on to a conic.

It is an elementary theorem that a change in order of the terms of a non-absolutely convergent series may alter the sum of the series, or, more precisely, if $u_1 + u_2 + \ldots + u_n + \ldots$ be a series having an infinite number of positive terms a_i, and an infinite number of negative terms $-b_i$, and the general term u_n converges to zero while both series a and b diverge, then the series does not possess the commutative property, and it is always possible with a suitable change in the order of the terms to obtain a series converging to a number arbitrarily assigned (Riemann) or a series diverging to $+\infty$ or to $-\infty$ or oscillating (Dini). O. Mignosi (*Giornale di Mat.*,

68, 1920, 100–13) examines the matter without the restriction that the general term shall tend to zero, showing that it is possible to change the order of the terms so as to obtain both regular and non-regular series. He prefaces his paper with an account, after Cipolla, of the ideas of *regular* sequences or series, which have only *one* limiting value, finite or infinite, and of the commutability of series.

It is well known that the first example of a function which is continuous and yet has no derivative was given by Weierstrass in 1875; it is $f(x) = \sum_0^\infty b^n \cos(\pi a^n x)$, where a is an odd integer and b a positive number less than unity and $ab > \frac{3}{2}\pi + 1$. In 1890 the editor of the *Bulletin des Sciences mathématiques* published a note found among the papers of C. Cellérier (1818–89), not dated but marked "très important, et je crois, nouveau," which gives another such function in the form of $\phi(n) = \sum_0^\infty a^{-n} \sin(a^n x)$, a being a positive integer sufficiently great. This can clearly be obtained as a special case of Weierstrass's function by writing $\pi x = \frac{1}{2}\pi - y$, $b = \pm a^{-1}$, according as a^2 is of the form $4m + 1$ or $4m + 3$. But this makes $|ab| = 1$, and the property of the function in question does not follow from Weierstrass's work. A. Falanga (*Giornale di Mat.*, **59**, 1921, 137–46) gives an account of Cellérier's method of investigation, which differs considerably from that of Weierstrass, and by means of it proves that Weierstrass's more general function has no derivative for $|ab| \geqq 1$.

Differential Equations.—If we take a system of two homogeneous linear differential equations of the first order $\frac{dy}{dx} = ay + bz$, $\frac{dz}{dx} = \alpha y + \beta z$, it is easily seen that the ratio $\xi = y/z$ satisfies a Riccati equation

$$\frac{d\xi}{dx} = b + (a - \beta)\xi - \alpha\xi^2.$$

A natural generalisation is to take a system of $(n + 1)$ such linear equations and to form the equations satisfied by the ratios of n of the unknowns to the $(n + 1)$th. The system arrived at is of the form

$$\frac{dy_r}{dx} = \Gamma_r + \Lambda y_r \; (r = 1, \ldots n),$$

where $\Gamma_1 \ldots \Gamma_n$ and Λ are linear combinations of the y's; such a system may be regarded as an extension of Riccati's equation, and as such it has been studied by G. Andreoli (*Rend. Napoli*, 1919–21; *Giornale di Mat.*, **60**, 1922, 33–64). The same system can be arrived at in another way from the $(n + 1)$ linear equations in the unknowns $z_0, \ldots z_n$. If we wish to

determine n coefficients $h_1, \ldots, h_n$ such that putting $z_r = \Sigma h_s y_s$, and substituting in the given equation the first shall be a consequence of the others, then the h's will have to satisfy precisely the same equation as the y's. Many properties of Riccati's equation can be extended to the system in question; for example, Weyr's theorem on the invariance of the cross-ratio of four particular solutions and that this is a characteristic property of Riccati's equation. Andreoli then investigates under what conditions there may exist some relation, other than the fundamental one, between solutions. He also deduces geometrical properties of the rational normal curve of order $(n + m)$, where m is the number of independent and n of dependent variables. The Riccati systems so obtained are related to a system of equations studied by E. Pascal; if one or more of the unknowns are eliminated, getting equations of higher order, we get what Andreoli calls a *secondary* Riccati system, including as special cases the primary system and, when all but one of the unknown are eliminated, the equations of Pascal. The author shows how any secondary system can be obtained by equating to zero n determinants arbitrarily chosen from the minors of a certain fundamental matrix of $(n + 1)$ columns and an infinite number of rows, but he has not yet succeeded in determining necessary conditions between the coefficients of a system for it to be a Riccati system, nor has he extended the geometrical properties to secondary systems.

Considering a system of linear partial differential equations of the second order:

$$F_{hk} \equiv \frac{\partial^2 f}{\partial x_h \partial x_k} + \sum_{r=1}^{m} \chi_{hkr} \frac{\partial f}{\partial x_{n-m+r}} = 0, \quad (h, k = 1, \ldots n),$$

E. Pascal in 1901 gave a definition of a *complete* system similar to that used by Clebsch for first order equations; he showed how to complete a given system and then, by consideration of the related total differential equations, how to determine the integrals. A. Chiaria (*Giornale di Mat.*, **59**, 1921, 1–10) obtains Pascal's m integrals of such a complete system without the use of the total differential equations.

Integral Equations.—For the integral operator of Volterra $A(\phi) = \int_a^x A(x,y)\phi(y)dy$, where the nucleus $A(x,y)$ is integrable and bounded in the field for which it is defined, the resolving operator $R = \sum_1^\infty k^{n-1}A^n$ is an integral transcendental function of the parameter k which satisfies the relation $R - kAR = A$;

and for such operators we have the theorem that the non-homogeneous equation

$$\phi(x) - k \int_0^x A(x,y)\phi(y)\, dy = f(x)$$

is possible and determinate, having for any value of k a unique solution $\phi(x) = f(x) + k\,\mathrm{R}f(x)$, while the homogeneous equation $\phi(x) - k\int_0^x A(x,y)\phi(y)dy = 0$ has only the solution $\phi(x) = 0$.

In a similar way for the integral operator of Fredholm $A(\phi) = \int_0^1 A(x,y)\phi(y)dy$ the resolving operator is a meromorphic function of k and we have the theorem that for any value of the parameter k which is not a pole of the resolvent, the non-homogeneous equation—

$$\phi(x) - k\int_0^1 A(x,y)\phi(y)dy = f(x)$$

has a unique solution $\phi(x) = f(x) + k\mathrm{R}f(x)$, while the homogeneous equation has only the solution $\phi(x) = 0$, but for a value k_1 which is a pole of the resolvent, the homogeneous equation has a finite number of linearly independent solutions and the associated homogeneous equation $\phi(y) - k_1\int_0^1 A(x,y)\phi(x)dx = 0$ has the same number of solutions. In this case, $k = k_1$, the non-homogeneous equation is impossible or indeterminate, the necessary and sufficient condition for the possibility of a solution being that the function $f(x)$ shall be orthogonal to all the solutions of the associated homogeneous equation; the non-homogeneous equation then has an infinite number of solutions, the difference of any two being a solution of the homogeneous equation.

M. Nanni (*Giornale di Mat.*, **58**, 1920, 125–60) deals with similar questions for the linear equations of order m with constant co-efficients

$$(a_0A^m + a_1A^{m-1} + \ldots + a_m)(\omega) = 0$$
$$\text{and} \quad (a_0A^m + a_1A^{m-1} + \ldots + a_m)(\sigma) = \bar{\sigma},$$

where the A is the symbol of an integral operator, $\bar{\sigma}$ is a given function and ω, σ are the unknown functions. Similar results are obtained, generalising those summarised above. The author makes use of the abstract calculus of linear operations, so that the results are valid, with slight modifications of form, not only for integral equations, but also for a more extended class of linear functional equations. The properties used have regard particularly to the continuity of linear functional operations, defined for a functional space, of which the elements α are functions of a variable x, and two types of

such operations are considered, with the common property of possessing for resolvent analytical functions of the parameter k, whose singularities are independent in position and kind of the element operated upon and of the values of the variable x. These two types are the Volterra type, in which the resolvent is an integral function of k, and the Fredholm type, in which it is a meromorphic function of k.

While the theory of *linear* integral equations has reached the state of being an organic whole, the study of *non-linear* equations is still in its infancy. Contributions have recently been made to the theory by A. Vergerio (*Annali di Mat.*, **31**, 1922, 81–120; *Giornale di Mat.*, **59**, 1921, 175–214). He first studies equation of the type

$$u(s) = h(s) - \lambda\int_a^b k(s,t)f[h(t)]dt,$$

where $u(s)$ and $k(s,t)$ are known functions, $h(s)$ is the unknown function, and f is the symbol of a functional operation, subject to conditions to be specified later. He proves the existence of *characteristic constants* and *characteristic functions* of the nucleus $k(s,t)$ relatively to the functional operation f, defines the *complete system* of such constants and functions, and uses them to obtain the solution. He then passes to the equation

$$u(s) = \psi[h(s)] - \phi[\psi\{g(s)\}] - \lambda\int_{\mu(s)}^{\nu(s)}\sum_1^p k_r(s,t)f_r[h(t)]dt,$$

where the ψ, ϕ, and f_r are symbols of functional operations; this includes most of the types of integral equations which have yet been studied, the equations of Burgatti and the integro-differential equations of Volterra.

By *functional operation* is meant any analytic operation which, applied to a function, generates a new function. Operations which, applied to any finite function, do not introduce singularities are said to be *regular*; otherwise they are *singular*. An operation f is *distributive* if, for any two functions $\phi(s)$ and $\psi(s)$ and for any constant c, we have

$$f[\phi(s) + \psi(s)] = f[\phi(s)] + f[\psi(s)],$$
$$f[c\phi(s)] = cf[\phi(s)].$$

In the paper in the *Annali*, the author considers only regular distributive functional operations; in the *Giornale* he examines certain types of equation in which they may be singular and also not distributive.

Schwarz's inequality for two real continuous functions, $f(x)$ and $\phi(x)$, *viz.* $\left(\int_a^b f(x)\phi(x)dx\right)^2 \leqslant \int_a^b f^2(x)dx \, . \int_a^b \phi^2(x)dx$, has been proved by Schmidt by means of orthogonal functions and

Bessel's identity; L. Tocchi (*Giornale di Mat.*, **59**, 1921, 115–22) gives a much more elementary proof and obtains a generalisation to more than two functions. He then applies his new theorem to the investigation of the roots of the transcendental equation $D(\lambda) = 0$ studied by Fredholm.

Abelian Functions.—The first general systematic investigations of algebraic varieties from the point of view of *reality* were made by Klein (*Riemann'sche Flächen*, 1894) and by Comessatti (1912), the former treating algebraic curves with symmetrical Riemann surfaces, and the second rational surfaces. In this connection the *antibirational* transformations introduced by Comessatti are of great importance. S. Cherubino (*Giornale di Mat.*, **60**, 1922, 65–94; **61**, 1923, 47–68) attacks the question from the point of view of Riemann matrices, a conception due to Scorza (*Rend. Palermo*, **41** (1916)). A *real* Riemann matrix is defined as one admitting of an involutory Riemann anti-substitution; the Abelian variety to which it belongs is then real, *i.e.* admits of an involutory anti-birational transformation into itself. The Abelian varieties which are birationally identical with the same real variety can be divided into one or more real classes—members of the same class being transformable into one another by *real* birational transformations; the author concludes by determining the number of such classes for a non-singular Abelian variety.

Geometry.—G. Pittarelli (*Giornale di Mat.*, **61**, 1923, 92–108) gives an account of the geometrical work of Nicola Trudi (1811–84) and of Achille Sannia (1822–92).

In the polar theory of plane algebraic curves the curve whose equation is

$$\left(y_1\frac{\partial}{\partial x_1} + y_2\frac{\partial}{\partial x_2} + y_3\frac{\partial}{\partial x_3}\right)\left(z_1\frac{\partial}{\partial x_1} + z_2\frac{\partial}{\partial x_2} + z_3\frac{\partial}{\partial x_3}\right)(fx_1, x_2, x_3) = 0$$

is called the *mixed second polar* of the two points y and z with respect to the curve $f(x_1\ x_2, x_3) = 0$. G. B. Zecca (*Giornale di Mat.*, **58**, 1920, 229–32) investigates the particular case in which the curve breaks up into a system of n straight lines and the two points are the circular points. In 1909 he had considered systems of lines for which this polar is indeterminate; he now shows that the polar has all its points at infinity only when $n = 2$ or when $n = 3$ and the lines form an equilateral triangle. He also shows that if the lines form a regular polygon the polar curve consists of $\frac{1}{2}n - 1$ concentric circles, if n is even, and of $\frac{1}{2}(n-3)$ concentric circles together with the line at infinity, if n is odd.

In the theory of invariants there is a well-known principle of translation due to Clebsch by means of which any invariantive property, expressed by the vanishing of an invariant or

by the identical vanishing of a covariant of a binary form may be transferred to the ternary field and gives a projective property of a ternary form (see, for instance, Grace and Young, *Algebra of Invariants*, pp. 265–7). In a foot-note to the *Leçons sur la Géométrie* (t. i, p. 348), Clebsch applies his principle to the inflexional tangents of a cubic curve $a_x^3 = 0$, which are obtained as the common tangents of a system of curves of the fourth class

$$(abu)^2(auv)(buv) = 0,$$

the v_1, v_2, v_3 being parameters. D. Mercogliano (*Giornale di Mat.*, **60**, 1922, 95–102) works out the details of this result, obtaining the equations of the 16 linearly independent curves of the system, and examining the cases in which the cubic curve has a double-point or a cusp. He also obtains another system of curves, of class 6, given by $(abu)^2(cbu)(cvu)^2(avu) = 0$, whose common tangents are the inflexional tangents of the cubic.

The most fundamental method of investigating the properties of a rational plane quintic curve is, of course, to consider it as the projection of a normal quintic curve in space of five dimensions. This is the method which has been used with success by Marletta and more recently by Hjelmman (*Ann. Soc. Fenn.*, A 3, 1913, No. 11) to obtain synthetically properties of the rational twisted quintic in three dimensions. W. Mueller (*Giornale di Mat.*, **60**, 1922, 103–98) applies it systematically from the point of view of invariant theory to connect up and unify various known results for the plane curve due to Berzolari, Stephanos, and Stahl. He first investigates the invariants and covariants of a single binary quintic in their relation to the normal curve, and then goes on to the combinants of pencils and nets of such forms. He thus obtains the most important covariant curves of the rational plane quintic, develops the theory of osculants, due in the first place to Jolles, and of the fundamental involution (Stahl) and proceeds to the study of a net of cubic curves with a common double-point, and its Jacobian, a rational sextic with a 5-ple point, projectively related to the quintic. He concludes with a study of some special forms of the quintic, in particular, the curve with a 4-ple point and the conjugate curve, in connection with Gordon's invariant theory of the binary septimic, and curves with points of hyperosculation.

A polygon inscribed in a space curve and such that the osculating plane at any vertex passes through the next vertex is said to be *a principal polygon* of the curve. In 1887 Zecca determined the principal polygons of a twisted quartic with a double-point. His work was amplified by A. Brambilla in 1898, but without distinguishing between real and imaginary.

P. Locchi (*Giornale di Mat.*, **59**, 1921, 81–92) investigates the question for a general rational twisted quartic by means of the group of the three biaxial involutions whose axes are the three pairs of opposite edges of the principal tetrahedron. He shows that the 8 principal triangles can be divided into two tetrads, each consisting of a triangle and its three transforms by the involutions and that the planes of each tetrad form a tetrahedron desmic with the principal tetrahedron. Similarly, of the 18 quadrangles, 12 are plane and consist of 3 tetrads with similar properties, while the remaining 6 fall into 3 pairs of skew quadrangles of which the diagonals meet respectively the 3 pairs of opposite edges of the principal tetrahedron. Going on to the case of a quartic with a double point, Locchi shows that the line of intersection of the osculating planes at the node is the only real principal chord, while the proper principal polygons are all imaginary. For the case of an isolated point the ordinary parametric representation, due to Weyr, $x : y : z : t = \lambda : \lambda^2 : \lambda^3 : \lambda^4 + 1$, does not give a real tetrahedron of reference, but the author obtains a new representation $x : y : z : t = 1 + \lambda^2 : \lambda^2(1 + \lambda^2) : \lambda(1 + \lambda^2) : h(1 - \lambda^2)^2$ and shows that all the principal polygons are real. He then goes on to consider the *Steiner polygons* of the quartic, for which the sides are alternately coplanar with two fixed chords of the curve. For the case of a node the only real proper polygons of this kind are quadrangles, but for an isolated point Steiner polygons of any even order exist. The paper closes with a note on circumscribed polygons.

The investigation of the curves which lie upon a cubic surface is a familiar question, for which reference may be made to the work of Cremona and Sturm and to papers by Rohn (*Leipzic Ber.*, **46** (1894) and Baker (*Proc. London Math. Soc.*, (2) **11**, 1912, 285–301). In Prof. Baker's treatment, every algebraic curve on the surface is shown to be co-residual with seven elements of the surface, each taken a certain number of times; these may be taken as the six lines $a_1, a_2, \ldots a_6$ of one row of a double six and a twisted cubic u associated therewith, and we may represent our curve by the symbol $\lambda u + \lambda_1 a_1 + \lambda_2 a_2 + \ldots + \lambda_6 a_6$ where $\lambda, \lambda_1, \ldots \lambda_6$ are integers. If m is the order of the curve, then we have $3\lambda + \Sigma\lambda_i = m$, and by considering the genus of the curve we get an inequality $\lambda^2 - \Sigma\lambda_i^2 \geqslant m - 2$. These conditions have to be solved in integers in order to obtain all irreducible curves of order m on the surface, and this can easily be done systematically by a procedure suggested by Prof. Burnside (*l.c.*, foot-note, p. 291). Clearly curves of the same type, though related to different double sixes, will arise separately. A method recently devised by M. Piazzolla-Beloch (*Giornale de Mat.*, **59**,

1921, 47–72), who does not seem to be aware of Prof. Baker's work, does not have this drawback and enables the author to obtain results of a general kind. She defines the curve by the number of points in which it meets the lines of the surface; the line meeting it in the greatest number of points is taken as a_1, and for b_1 the number is greater than, or at most equal to, the numbers for the other a's, b's, and c's. Calling the numbers a, β, γ respectively, a_1 is put equal to $m - a$, and they are all easily expressed in terms of m, a, and γ_{1r} $(r = 2, \ldots 6)$. They also satisfy various inequalities, in particular $a \leqslant \frac{2}{3} n$ and $\sum_{2}^{6} \gamma_{1r} \leqslant {}^5/_2 a$. The curves are then classified according to the value of a. For $a = 1$ we get rational curves, of one type only, the numbers being different according as m is even or odd. For $a = 2$ we have hyperelliptic curves, 4 types when m is even, and 3 types when m is odd. Details are likewise given for $a = 3$, and for the maximum value of a, the greatest integer in $\frac{2}{3}m$. For $m = 3s$ we get the result that every curve of order $3s$ on a cubic surface not having more than s intersections with any line of the surface is of the maximum genus $\frac{1}{6}m(m - 3) + 1$ and meets *every* line in s points; it is clearly a complete intersection with a surface of order s. Results of a similar nature hold for $m = 3s + 1$ and $m = 3s + 2$.

The investigation of birational transformations of space is naturally bound up with that of rational surfaces and their representation upon a plane, and both matters have been the subject of several recent papers in Italian journals. Cremona determined all the homaloidal systems of surfaces to which a given rational surface F belongs by using a plane representation of F and applying the known properties of plane birational transformations. A more direct method was invented by Loria in which use is made only of properties of the surface F and not of extraneous elements, and he applied this to determine all the birational transformations of space to which a general cubic surface gives rise—all but one had previously been found by Cremona. C. Aroldi (*Giornale di Mat.*, **58**, (1920) 175–92) works out the similar problem for the quartic surface with a double conic, and N. Berardi (*ibid.*, **61** (1922), 109) a pupil of Montesano, enumerates results of the same kind for the quintic surface with two skew double straight lines and a triple straight line meeting them both. Another pupil of Montesano, C. Nobile (*ibid.*, **59**, 1921, 147–74) enumerates 32 other birational transformations likewise determined by homaloidal systems of surfaces whose plane sections are of genus 1. Incidentally the author obtains the plane representations of various rational surfaces of orders 6, 8, and 9.

In the opposite direction is the work of A. Tummarello (*ibid.*, **58** (1920), 60–6) who uses birational transformations of space to obtain general types of rational surfaces. Surfaces of order m with an $(m-1)$-ple or an $(m-2)$-ple line are known to be rational; he goes on to consider rational surfaces with an $(m-3)$-ple line, which must have other singularities. He shows that if such a surface has $(m-3)$ ordinary triple points then it can be transformed birationally into a cubic surface, a quartic surface with a triple point, and a line not through this point (Cayley's monoid with transversal) or a quintic surface with a double line and 2 triple points (investigated by Cremona and Montesano), according as m is congruent to 0, 1, 2 to modulus 3. These three surfaces are all rational, hence the same is true of the surface of order m. The author gives the plane representation and discusses the special cases which arise owing to the presence of further nodes. The pencil of planes through the multiple line gives a pencil of elliptic cubics on the surface, so that the investigation is related to two papers by Marletta (*Rend. Palermo*, **41**, 1916; **42**, 1917) on surfaces of order 6 and 7 with such pencils of curves. In a still more recent paper G. Nobile (*Giornale di Mat.*, **60**, 1922, 23–32) generalises this work to some extent by considering rational surfaces of order m with an $m-\nu$-ple line and ν-ple points $(\nu>2)$. The author proves that if there are k such ν-ple points, of general position, then the surface is rational if, a being a positive integer,

$$\begin{aligned}
&(1)\ k=3a+1 \text{ and } m-\nu=\nu a+1,\\
\text{or }&(2)\ k=3a+2 \text{ and } m-\nu=\nu a+2,\\
\text{or }&(3)\ k=3a+3 \text{ and } m-\nu=\nu a+3.
\end{aligned}$$

For $\nu=3$, this gives Tummarello's result. Case 3 arises only for $\nu=3$; in case (1) there are no other multiple curves, but in case (2) there is a $(\nu-2)$-ple curve of order $2a+1$ meeting the multiple line in $2a$ points and passing through $3a+2$ of the ν-ple points.

By means of vector analysis F. Sibirani (*Giornale di Mat.*, **59**, 1921, 31–6) finds all the developables which have a given director-cone and for which every generator lies in one of the planes of an assigned simply infinite system.

M. de Valle de Paz (*ibid.*, 73–80) shows that the real pseudo-spherical congruences with imaginary foci obtained by Bianchi in 1915 exhaust all possible types.

In 1880, while investigating the representations of imaginary quantities by means of geometrical elements, H. Duport arrived at a class of congruences satisfying two special conditions. If $f(x,y)=0$ is the equation of a plane curve in

rectangular Cartesian co-ordinates, expressing that a point $(a + iP, \beta + iQ)$ lies in the curve and equating real and imaginary to zero, we get two equations connecting a, β, P, Q. The line $x = a + Qz$, $y = \beta - Pz$ thus belongs to a congruence, for which it can be shown (1) that the product of the distances of the foci A, A[1] of any ray from a fixed line is constant, and (2) that the traces on a fixed plane of the tangent planes to the focal surfaces from the foci are orthogonal. Moreover, both focal surfaces satisfy the differential equation

$$\frac{\partial^2 z}{\partial x^2}\frac{\partial^2 z}{\partial y^2} - \left(\frac{\partial^2 z}{\partial x \partial y}\right)^2 = \left[\left(\frac{\partial z}{\partial y}\right)^2 + \left(\frac{\partial z}{\partial y}\right)^2\right]^2 \Big/ (z^2 - 1)^2.$$

S. Ricca (*ibid.*, 21–30) investigates by means of moving axes all congruences satisfying conditions (1) and (2) and obtains an inversion of the theorem of Duport.

ASTRONOMY. By W. M. H. Greaves, M.A., Royal Observatory, Greenwich.

Relativity Displacement of Spectral Lines.—Einstein's prediction of the displacement towards the red of the lines in the solar spectrum has been confirmed independently by J. Evershed and C. E. St. John. A résumé of Evershed's work is given by him in *The Observatory*, vol. xlvi, no. 53, pp. 299 304, October 1923, and St. John's results are given in *Monthly Notices*, R.A.S., vol. lxxxiv, no. 2, pp. 93–6, December 1923. The success of the work in each case depends on the fact that pressure shifts are negligible, a fact which was first recognised by Evershed, and afterwards by St. John. The recognition of the smallness of the pressure effect is important, as it has enabled the investigators to use well-defined metallic lines instead of confining their attention to the cyanogen bands which are free from pressure effects, but which are unsuitable on account of their complexity. Both Evershed and St. John find that the Einstein effect is present and that in addition there are other residual effects consisting of radial motions, which, according to St. John, are downward for high levels and upward for low levels, and of an excess of shift at the limb as compared with the centre. Evershed offers no explanation of the limb effect, but St. John explains it as being due to differential scattering in the longer paths traversed through the solar atmosphere by light coming from the limb. Further confirmation of the Einstein effect is furnished by Evershed's measurements of the lines in the spectrum of Venus.

Deflection of Light in the Sun's Gravitational Field.—Further confirmation of the deflection of light in the sun's gravitational field is given by G. F. Dodwell and C. Davidson in *Monthly*

Notices, vol. lxxxiv, no. 3, pp. 150–62, January 1924. The paper deals with the plates obtained by the expedition which under the direction of Dodwell observed the total eclipse of 1922, September 21, at Cordillo Downs, South Australia. The plates were afterwards sent to Greenwich for measurement and reduction under the direction of Davidson. A striking novelty consists of the photographing *during the eclipse* of a check field, a few degrees from the sun, on the plate which has already been exposed to the eclipse field. This is compared with a similar plate taken some months after the eclipse, and the comparison field gives a determination of any change of scale value which is then applied to the eclipse field. In this way a troublesome source of systematic error is eliminated.

The result obtained for the gravitational displacement at the sun's limb is $1''\cdot77 \pm 0''\cdot3$. The predicted value is $1''\cdot74$.

The probable error is thus unfortunately large. This must be attributed to the small size of the instrument and the faintness of the images arising from the low altitude of the sun. It is particularly unfortunate that this should have been so, for, had the instrument used been more sensitive and the conditions good, the determination of scale change in the manner indicated above would have undoubtedly made the result the most satisfactory confirmation of Einstein's prediction that has yet been obtained.

The Absorption of Radiation Inside a Star.—A. S. Eddington (*Monthly Notices*, vol. lxxxiv, no. 3, pp. 104–23, January 1924) adopts a theory recently elaborated by H. A. Kramers which is based on an assumed correspondence between classical and quantum dynamics and which leads to formulæ in close agreement with laboratory experiments. Kramers's theory gives in the first place a formula for the intensity of the continuous spectrum caused by the encounters without capture of electrons with nuclei. In radiative equilibrium the emission so obtained must be balanced by absorption, and Eddington thus obtains a formula for the contribution made to the absorption coefficient in a star by the process considered by Kramers.

Kramers also gives a theory of the encounters resulting in the capture of an electron with the emission of radiation. Again, this must be balanced by absorption. Eddington calculates the corresponding contribution to the absorption coefficient k for the case of the star Capella for which the necessary data are known and finds that its contribution is 4·91 at the centre of the star. The contribution to k due to the previous process is found to be 0·65 and the value of k as derived from observational data is 47·5, so that Kramers's processes only account for an eighth of the astronomical value. An examination as to whether any legitimate changes in the

adopted data would give better agreement leads to a negative result. It appears that we would get exact agreement if the star contained sufficient hydrogen, but the corresponding brightness of such a star would be too low.

Eddington also considers the contributions to the absorption coefficient corresponding to other processes—namely, the encounters of free electrons with one another, and the excitation of electrons from their normal orbits to higher quantum orbits. He finds that these contributions are negligible in the astronomical case, but points out in a postscript that the investigation of the excitation process is inconclusive. In the course of the investigation the state of ionisation in Capella is examined, and it is found that the mean molecular weight must be about 2·2.

In an investigation of this kind it is necessary to be cautious, and although the investigations indicate that the disagreement between the absorption coefficient corresponding to Kramers's theory and that determined from the astronomical data is outside the limits of uncertainty, Eddington is the first to admit that we cannot be quite sure of this. His (Eddington's) previous theory of nuclear capture, in which the effective agent was emission by electron captures resulting from collisions with nuclei, gave very good results, and he feels that he must hesitate in discarding it in favour of a theory for which agreement with astronomical data is poor. At the same time Kramers's theory is in accordance with laboratory experiments. It is possible, of course, to combine the two processes, but Eddington points out that this will lead us to assume that in the cases of capture by nuclear collision the captured electrons fall mainly into high level orbits and not into the inner K and L rings.

A Statistical Study of the Near-by Stars.—W. J. Luyten (*Harvard Annals*, vol. lxxxv, no. 5) gives a statistical study of 104 stars considered to be within 10 parsecs of the sun. Luyten's paper contains a large mass of interesting information, and it is impossible to do it justice in an abstract such as this. We may, however, mention a few of his results.

The solar motion from these stars comes out to be 25 *km/sec* directed towards R.A. 278° and Dec. 36° N. A linear relation is found between the quantities $H = m + 5 + 5 \log \mu$ and $M = m + 5 + 5 \log \pi$, where m = apparent magnitude, π = parallax in seconds of arc and μ = annual proper motion in seconds of arc. Functional relations are established statistically between the absolute magnitude M on the one hand, and on the other the colour index, the linear diameter, and the density. On the assumption that Kapteyn's luminosity and density law hold throughout the stellar system, the total mass of the Kapteyn system is computed to be $1{\cdot}4 \times 10^9$ times the

sun's mass. For details and further results, the reader must be referred to the paper itself.

Absolute Magnitudes and Parallaxes.—R. K. Young and W. E. Harper (*Journal of R.A.S. of Canada*, vol. xviii, nos. 1–2, pp. 9–59, Jan.–Feb. 1924) give an account of some work which has been performed recently at the Dominion Astrophysical Observatory at Victoria. This work consists of the determination of the spectroscopic absolute magnitudes and parallaxes of 1,080 stars, and the complete publication of the material is to be expected shortly. In the meanwhile the authors have presented us with this account of their work. The general programme was planned along the lines set forth at Mount Wilson, but Young and Harper introduce some improvements. Their determinations rest upon more spectral lines than those at Mount Wilson, and they have developed a method whereby the step, this being the unit of intensity ratio, is fairly well defined. Their method consists of the preparation of an artificial spectrum which is used as a standard of reference whereby intensity ratios are estimated. The preparation of the standard consists in the photographing, by several successive exposures on the same plate, in the same position, of an illuminated opening, across which are placed a number of wires of standard size. Between each two exposures one of the wires is removed, and the result is an imitation spectrum with absorption lines of different intensities.

Comparison of the results with those obtained at Mount Wilson shows good agreement for most spectral types, but for the late K type giant stars there is a discordance. For these stars Young and Harper obtain a range in absolute magnitude of almost five magnitudes, whereas the Mount Wilson observers get a range of little over one.

Internal Motions in Spiral Nebulæ and a Proposed New Cosmic Force.—The measurements of Van Maanen of the internal motion of spiral nebulæ form the basis of an interesting paper by J. H. Jeans in *Monthly Notices*, vol. lxxxiv, no. 2, pp. 60–70, Dec. 1923.

It has been shown by v. d. Pahlen that the arms of normal spiral nebulæ are approximately equiangular spirals. Jeans points out that this renders it highly probable that the motions in progress in the arms must be such that these change into new equiangular spirals. Adopting this view as the basis of his investigation, he obtains a formula of the type—

$$\mu_{trans} = a + b \log r,$$

where μ_{trans} is the motion perpendicular to the arms, r is the radius vector from the nucleus, and a and b are constants. He shows that this formula is in accord with Van Maanen's measurements.

Jeans then passes on to inquire what forces can produce the observed motions. A preliminary discussion shows that electro-magnetic forces and forces arising from gas pressure may be ruled out. Two formulæ are obtained connecting F and G, the radial and transverse components of force, and v the velocity along the spiral arm. Van Maanen's measures indicate that v varies as r^n where $n = 0{\cdot}26$ within the errors of observation, and we are led to the conclusion that F and G must vary as $r^{-\frac{1}{2}}$. Jeans shows that it is very improbable that F and G can arise from the ordinary gravitational field of nucleus plus arms, and he is led to conjecture that the new force must depend on the velocity of a particle on which it acts and must be proportional, in part at least, to v^2/r. Furthermore, the resultant new force must not be directed towards the nucleus, and indeed it appears that neither F nor G can be zero.

Systematic Motions of Faint Stars.—W. M. Smart (*Monthly Notices*, vol. lxxxiv, no. 1, pp. 3–14, Nov. 1923, and vol. lxxxiv, no. 3, pp. 123–33, Jan. 1924) gives the results for eight photographic regions which have been the subject of study at the University Observatory, Cambridge. The discussion of the proper motions in these regions shows very clearly the phenomenon of star streaming among stars of magnitude 8 to 12. The results are in good agreement with the results for bright stars, but there is some evidence that the proportion of stars belonging to Stream II is greater in the case of faint stars.

The motions of faint stars have been previously studied by Van Maanen (*Mount Wilson Contributions*, no. 168) and also at the Union Observatory at Johannesburg, which gives some further results in *Monthly Notices*, vol. lxxxiv, no. 3, pp. 190–92, Jan. 1924. These results are illustrated by an interesting diagram.

Accounts of other recent researches are unavoidably held over.

METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

The *Quarterly Journal of the Royal Meteorological Society* for January 1924 contains two papers which are of particular interest to those meteorologists who concern themselves with the dynamics of wind currents and with the causes underlying the phenomena of cyclones. The first, by Sir Napier Shaw, is entitled "Resilience, Cross Currents, and Convection." Many of the ideas put forward in this paper have been outlined already in the numerous books and pamphlets of that writer, but some are quite new. The tendency to stratification

in the earth's atmosphere, that has been proved to exist by the researches into the physical state of the air up to about 30 kilometres' height that have been made during the last twenty years, appears to be a world-wide phenomenon of fundamental importance in all considerations of atmospheric motion, as Shaw has often insisted. Under normal conditions the potential temperature of the air increases with height, in other words a sample of air will in general be found to have a higher temperature than the air near the ground if it be brought down to the level of the ground, and the greater the height from which the air is brought down the greater will be this excess in its temperature. Such a state of affairs causes " resilience," for it follows that a sample of dry air which is raised to a higher level will in general find itself colder than its surroundings and will tend as a result of its greater density to descend again, just as it will tend on account of its higher temperature and consequently lower density to reascend after being brought down to a lower level. The stratification of the air up to a height of about 10 kilometres, i.e. in the troposphere, is on the average very marked. At higher levels even the fall of actual temperature ceases and in the " stratosphere," from the " tropopause " (which generally occurs at about 10 km. in temperate latitudes) up to a height of 30 km. at least, the increase of potential temperature with height is rapid, and the resilience in consequence is very great. It follows from all this that vertical movement of dry air is not normally facilitated by the vertical distribution of temperature, and in those cases where mechanical forces are available for the production of such movements strong forces of restitution are brought into play.

If the imaginary sample of air which we have been considering is not dry, however, but nearly saturated, the state of affairs as regards possible upward movements in the " troposphere " is different, for the ascent of such air will be accompanied by the condensation of water-vapour and a liberation of " latent heat." This heat may warm the sample sufficiently to enable it to maintain itself at a higher temperature than the surrounding air, and so long as this state of affairs continues there will be no force of restitution. As higher levels are reached, however, the general fall of temperature of both sample and environment will, owing to the diminished capacity of air for holding water-vapour or low temperatures, result in an approximation to dry conditions. The sample of air will seldom reach the " tropopause," and even should it do so, the very high potential temperatures of the stratosphere will completely arrest the motion. These considerations lead Shaw to say that " we come to look upon

the successive layers even of the troposphere as being in a sense separate 'aerospheres,' and further to regard water-vapour as the real agency which enables the air of one layer to penetrate the layers above it, temperature alone being insufficient to cause any considerable elevation. . . . Layers of various cloud-forms covering a considerable portion of the sky, afford evidence of this kind of thermal stratification." The atmosphere must therefore be regarded as being made up of a series of "aerospheres" which are thermally and dynamically distinct, and in which any motion may be regarded as very nearly two-dimensional. For example, if a mass of air is removed from one layer and is not replaced by air of the same pressure, vortical motion will tend to be set up and the differences of potential temperature can be balanced by a suitable adjustment of pressure and velocity such as is found in actual cyclonic depressions.

Although the "aerospheres" are nearly independent, some turbulence and mixing as a result of the relative motion between consecutive layers do occur. Shaw deals in this paper with the nature of this kind of motion. His treatment is in some respects novel, although based upon the researches into eddy-motion carried out during recent years by other investigators. It has been shown that "in the case of the turbulent motion at the surface the relation of the wind velocity to height follows approximately a logarithmic law, and when the disturbance of the steady velocity is the turbulence due to the ground, when also there is no change of gradient or of coefficient of eddy-viscosity with height, the logarithmic relation can be formally represented by the addition of a vector to the line representing the geostrophic wind such that the point of the added vector moves on an equiangular spiral of 45°, making equal steps of rotation for equal steps of descent from the undisturbed wind to the surface. In the limiting case, zero velocity is reached at the surface, when the angular deviation from the geostrophic wind is 45°, and the relation of the wind V at any level to the geostrophic wind G can be computed from the position of the level with reference to the spiral." Shaw then shows how analogous reasoning can be applied to the case of two wind currents in adjacent "aerospheres" that are equal in velocity and opposite in direction. Between such currents there must be a layer of "zero velocity," where the motion is entirely turbulent; this layer corresponds with the ground in the case already referred to, and the distribution of horizontal velocity in the zone separating the two undisturbed currents can be sketched. (An example is given.) Since every change of velocity with height is summed up as a transition from one

vector to another by the gradual addition of a third, this is equivalent to the reversal of a component. Every example of cross-currents or of sudden change of wind velocity may therefore be treated in the manner just described, for the results as regards turbulence are unaffected by the general drift caused by the unchanged component of the motion. Two interesting examples of actual reversals of wind close the discussion of eddy-motion; in one case a wind of 10 metres per second, geostrophic at all levels, is reversed gradually between the surface and a height of 8 km., and in the other case a wind of the same strength is reversed between the levels of 2 and 5 km., while the gradient wind is reversed between 3 and 4 km. This latter example serves to illustrate the method, which, as Shaw points out, is only approximate, and is put forward tentatively as a possible way of reducing the great variety of wind structures actually observed to some semblance of order. It should be noted that the effect of the turbulence due to the relative motion of consecutive aerospheres will be to change the lapse rate towards the condition of convective equilibrium, and often to lead to cloud formation.

The third subject dealt with in this paper is convection. Shaw divides convection into two kinds: that which may be compared with the ascent of a bubble through a liquid, and is never brought about by the mere heating of a portion of the atmosphere unless that portion is nearly saturated, and another kind analogous to the convection of water in a tank which is of the form of a steady ascent caused by the accumulation of colder fluid underneath the ascending portion. Two theorems relative to the dynamics of the sea given by J. W. Sandström in a recent work apply almost equally well to the atmosphere because the atmosphere is stratified in a way analogous to the stratification in the sea, and these theorems help the study of atmospheric convection. They are:

(1) "In a fluid stratified like the water of the sea, external forces such as those of the wind or of the rotation of the earth cannot produce an exchange of particles between different layers. The surface of contact between the strata is impenetrable for particles which move under the influence of such external forces.

(2) "If in the sea there is in any locality a physical change such that the density is altered there, that volume of water will leave its own stratum and place itself in the stratum which corresponds with its new density."

Potential temperature in the air corresponds with density in sea water, and any portion of the atmosphere tends to find its own level according to its potential temperature irrespec-

tive of external forces such as those due to wind, general gravity, or the earth's rotation. This is the first type of convection. The analogy between the local disturbances caused by the formation or melting of ice in the sea and the disturbances due to condensation or evaporation of water-vapour and water in the atmosphere should be noted. In the second type of convection the upward movement of the air takes place over a much larger area. Examples of such movements are furnished by the "rain areas" of cyclonic depressions. In accordance with the modern view that all rain is caused by dynamical cooling of moist air, the presence of areas of continuous heavy rain many hundreds of square miles in extent implies that the air often has an upward component of motion over a large area. As has already been pointed out, the stratification of the air does not favour such movements, and it appears as though this second type of convection is not thermal in character but dynamical, and is a forced movement carried out in spite of unfavourable thermal conditions. The case of eddy-motion shows very clearly that on a small scale cold air can be carried upwards and warm air downwards by the application of external force. On this view "a cyclone must be regarded as a dynamical system for which some dynamical explanation must be found." It is this problem which is occupying the attention of so many meteorologists at the present time, the Norwegian "Polar Front Theory" being one of the most striking suggestions for its solution that have been put forward. At the same time, Shaw points out that the exact nature of the more local "thermal" type of convection is not fully understood, and presents an almost equally urgent problem for solution.

The second of the two papers under discussion is by Dr. H. Jeffreys, and is entitled "The Cause of Cyclones." For nearly a hundred years two theories purporting to explain the origin of both temperate and tropical cyclones have been in vogue. They may be referred to as the "local heating" and "millpond-eddy" theories.

The first of these two theories is so generally understood that it is unnecessary to describe it in detail. As Jeffreys' has shown in another paper (*Q.J.R. Met. Soc.*, **48**, 1922, p. 29), this theory does satisfactorily explain the formation of a great depression over Asia every summer, the local heating being more than sufficient to cause the observed variation of pressure. It has been objected that this explanation requires an ascending current in the centre which has not been observed and would cause torrential rainfall if it were to occur. When it is considered, however, that such an ascending current would be distributed over the whole of the vast area that is heated,

it is seen that it must necessarily be too small to be easily observed; moreover, once the area of low pressure is established no ascent of air is necessary apart from the ascending current required to remove the very small amount of air that creeps towards the centre in the lowest layers of the atmosphere as a result of friction with the ground.

When, however, this theory is applied to the formation of moving cyclones, which nearly always originate over the sea, whether in temperate or in tropical regions, there arises the insuperable objection that nowhere does the sea become heated above its surroundings to an extent sufficient to account for the differences of pressure actually observed.

Another theory quite different from the theory just described, yet also dependent upon local heating, was worked out a few years ago by Lord Rayleigh. It is based upon a consideration of the type of motion likely to result from the formation of innumerable eddies due to heating of the lower layers of a column of air which has an unstable lapse rate of temperature. While this type of motion doubtless exists in the atmosphere, the scale of the phenomenon is far too small for the theory to be applied to cyclonic depressions. It is, however, comparable with the size of thunderstorms and small secondary depressions. This theory has not been developed to the point of making quantitative estimates of the fall of pressure to be expected under different conditions.

In the millpond-eddy theory, cyclones are regarded as analogous to the eddies formed when a mass of water moves rapidly through other water which is nearly stagnant. These eddies form along the surface of separation of the two supplies of water. Helmholtz investigated the possibility of such motion in the atmosphere, but his investigation appeared to show that vortices formed along a surface of discontinuity in the atmosphere would tend always to die out quickly for want of the means for maintaining a continuous state of instability. Closely allied to the millpond-eddy theory is the theory of Bjerknes that temperate cyclones are of the nature of wave-motion on the "polar front." This theory is not generally accepted, but a discussion of the reasons for this cannot be entered into here in detail. Jeffreys' principal objection is that the calculations of Helmholtz lead to wave-lengths far too small for the production of cyclonic depressions.

Helmholtz also investigated the phenomena to be expected when the two distinct supplies of air analogous to the moving and stagnant waters in the mill-pond mix, and showed that the mixed air must necessarily tend to rise, just as it would if heated. Jeffreys considers that this may possibly be the true cause of all cyclonic phenomena, but that a more searching

investigation is desirable before the theory can be pronounced to be adequate. Some idea, for instance, of the rapidity with which mixing can proceed in a horizontal direction is necessary, and also of course whether a reduction of pressure equal to that which occurs in cyclones can be brought about as a result of mixing.

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BIOCHEMISTRY. By R. KEITH CANNAN, M.Sc., University College, London.

Carbohydrate Metabolism.—Diligent search continues for the particular function in carbohydrate metabolism with which insulin is concerned. The problem is intriguing in the extreme. In the diabetic animal injection of insulin reduces the concentration of sugar in the blood and allows the replenishment of the glycogen stores. So far the picture is not difficult. Insulin stimulates the tissues to convert glucose into glycogen. But the question becomes obscured when one turns to consider the effect upon the normal animal, or—what comes to the same thing—of excessive doses on the diabetic. In such cases, not only is the blood sugar reduced below the normal level, but the glycogen stores are mobilised in a vain attempt to restore this level and the experimenter is faced with a loss of carbohydrate, the fate of which has not yet been discovered. Whereas there is, as we have seen in a former review, some evidence that insulin stimulates the oxidation of glucose by the tissues (Hepburn and Latchford, *Amer. Jour. Physiol.*, 1922,

68, 177), metabolic and respiratory experiments give little support to the view that the sugar which disappears can be accounted for by its oxidation (Dudley, Trevan and others, *Jour. Physiol.*, 1923, **57**, xlvii). A considerable conversion of sugar into fat is an attractive possibility, but, again, the evidence up to the present is entirely negative (Dudley and Marrian, *Biochem. Jour.*, 1923, **17**, 435). A third hypothesis —that the lost sugar has been converted into some form which escapes detection by the prevailing methods of carbohydrate analysis—has excited ardent, if sometimes careless, search for a hidden store of carbohydrate. The observation of Winter and Smith that injection of insulin in the normal rabbit leads to a reduction of the concentration of the inorganic phosphates of the blood (*Jour. Physiol.*, 1923, **57**, 447) has revived interest in the view that hexose phosphoric esters play an important part in carbohydrate metabolism, though it is to be remarked that such esters as have yet been isolated from yeast fermentations reduce alkaline copper solutions almost as readily as glucose itself ; so that it is improbable that insulin merely converts the sugar of the blood into such a form. Briggs, Koechig, Doisy, and Weber have confirmed the fall in blood inorganic phosphates under insulin and have found a concurrent rise in the concentration of lactic acid (*Biol. Chem.*, 1923, **58**, 721). We are reminded that Meyerhof obtained some evidence that a hexose phosphoric ester is the precursor of lactic acid in the muscle. On the other hand, Dudley has communicated to a recent meeting of the Biochemical Society experiments in which were recorded an actual decrease in the lactic acid of the muscles of animals in insulin hypoglycæmia below the normal concentration. It is to be remembered that in the hypoglycæmic condition is exhibited a very abnormal respiratory condition associated with irregular muscle activity, either of which may lead to considerable fluctuations in the lactic acid concentration of the blood (Anrep and Cannan, *Jour. Physiol.*, 1923, **58**, 244). It would seem that a more suitable subject for study than the hypoglycæmic animal would be an animal maintained for long periods under the influence of large doses of insulin with the simultaneous administration of glucose in amount sufficient to maintain the normal level of blood sugar. Such an animal might be expected to show to an exaggerated extent any redistribution of carbohydrate brought about by the presence of excess of the pancreatic hormone. Before leaving the subject of lactic acid production we must record that Azuma and Hartree (*Biochem. Jour.*, 1923, **17**, 875) have found that insulin is without effect on the heat production of the contracting isolated frog's muscle.

Winter and Smith (*Physiol.*, 1923, **62**, 100) recently revived the suggestion of Hewitt and Pryde (*Biochem. Jour.*, 1920, **14**, 395) that certain tissues have a specific capacity to convert glucose and fructose into their reactive γ isomerides. The work of Hewitt and Pryde has been carefully repeated by Stiven and Reid (*Biochem. Jour.*, 1923, **17**, 556), and by Creveld (*Biochem. Jour.*, 1923, 17) who have been quite unable to obtain any evidence that under the conditions of the experiments there is any such activation of sugar. This has been followed by Eadie (*Brit. Med. Jour.*, 1923, July 14) and others who have failed to confirm the contention of Winter and Smith that there is in the liver an enzyme which after activation by insulin converts glucose into the ethylene oxide (γ glucose) isomer. γ glucose is at the moment little in favour with physiologists. It seems difficult, on the other hand, to avoid the conviction that in some way the pancreatic hormone does influence the reaction of carbohydrate to the oxidative mechanisms of the cell and, in this connection, the observation of Ahlgren (*Skand. Arch. f. Physiol.*, 1923, **44**, 167) is of interest that the tissues of diabetic dogs show little power to reduce methylene blue *in vacuo* in the presence of glucose until insulin is added. Of information as to the part played by insulin in the hydrogen transport systems of the cell there is none.

There has been up to the present little disposition to criticise the conclusion that insulin preparations really do exhibit the specific character of the pancreatic hormone in respect to carbohydrate metabolism. The evidence for this view rests largely on the fact that insulin does undoubtedly allay the recognised symptoms of pancreatic diabetes, and upon the preparation of active extracts from just those cells of the pancreas which have long been held to be the seat of manufacture of the hormone. We may yet be given cause to revise this conclusion in so far as we hold that the properties of insulin are specific to the pancreas. It appears that a substance having much in common with insulin has a wide distribution in nature. Collip (*Jour. Biol. Chem.*, 1923, **57**, **67**, and **58**, 163) has prepared from yeast, lettuce, onions, and other sources extracts which not only reduce the blood sugar of normal animals and cause the familiar hypoglycæmic convulsions, but also are able to maintain alive for long periods completely depancreatised dogs. Hutchinson, Smith, and Winter (*Biochem. Jour.*, 1923, **17**, 583, 764) confirm this result with yeast and extend it to an extract of a coli-form bacillus. Of greater significance is the report of Best and Scott (*Jour. Amer. Med. Assoc.*, 1923, **81**, 382) that they have prepared from the submaxillary gland, the thymus, thyroid, and the liver of dogs a preparation in yield equal to that from the pancreas which

gave typical insulin effects when injected into normal or depancreatised dogs.

Improved methods of preparation of insulin are reported in a number of journals with preliminary attempts to investigate the chemical nature of the specific agent. The general conclusion at the moment, though there are dissentients, is that insulin is or is associated with material of a proteose nature.

Fat Synthesis by Yeast.—A valuable paper by Mrs. Smedley McLean has appeared on this question (*Biochem Jour.*, 1923, **17**, 720). It was found that yeast forms fat from alcohol, sodium acetate, sodium lactate, and sodium pyruvate, probably with the intermediate production of sugar. Oxygenation of a suspension of yeast in glucose solution increased the fat content of the yeast 100 per cent. above that attained in the absence of oxygenation, whilst the addition of phosphates still further increased the fat synthesis. The suggestion is advanced that storage of carbohydrate is in part effected as hexosephosphoric esters and that fat synthesis is facilitated by the oxidation of the latter compounds involving a simultaneous reduction to fatty acids. Since it was found that maltose promoted a more extensive storage of glycogen than did sucrose or the hexoses it was concluded that maltose was capable of polymerisation to glycogen without fission into glucose, a conclusion supported by Irvine's view that this polysaccharide has a maltose unit (*Jour. Chem. Coc.*, 1923, 898).

The Chemistry of the Proteins.—Following the appreciation of the significance of the nitroprusside reaction of tissues, which led to the isolation of glutathione, attention has been directed to the nature of the sulphur contained in proteins. There is evidence (*Proc. Roy. Soc.*, 1923, **94** B, 436) of the presence of an unidentified sulphur linkage and that, whereas in serum albumin 86 per cent. of the sulphur is in the form of cystine, in egg albumin only 14 per cent. is in that condition. The nitroprusside reaction is only given by proteins after denaturation, suggesting that this process involves the hydrolysis of, or a keto-enol change in, a thio-peptide linkage. Mueller (*Jour. Biol. Chem.*, 1923, **55**, *Proc.* xv) confirms the existence of sulphur in proteins in a form which does not blacken lead or give the nitroprusside reaction. In a later paper (*Jour. Biol. Chem.*, 1923, **56**, 157) he claims to have isolated a new sulphur-containing amino-acid from several proteins having the composition $C_5H_{11}O_2NS$. It is not yet excluded that the sulphur may have been introduced into the molecule during the processes of isolation.

Knaggs, Manning, and Schryver (*Biochem. Jour.*, 1923,

17, 473) report an interesting study of the purification of gelatin and from the same laboratory has been published a valuable addition to our methods for the isolation of the products of hydrolysis of protein, involving the separation of the barium carbamates of the amino-acids (*Biochem. Jour.*, **15**, 636). Studies of the dissociation constants of the amino-acids have enabled the elaboration of a titrimetric method for the determination of the NH_2 and COOH groups in amino-acids and polypeptides, which should find useful application to studies of the enzymic hydrolysis of proteins (L. J. Harris, *Proc. Roy. Soc.*, 1923, **95** B, 440).

Zelinski and Sadikov (*Biochem. Zeit.*, 1923, **88**, 156) have brought forward evidence in support of the view that proteins are built up largely of diketopiperazine rings united by long methylene chains, a suggestion independently made by Marston (*Biochem. Jour.*, 1923, **17**, 851) on quite different reasons. The former authors are criticised by Brigl (Ber [B] 1923, **56**, 1887), who considers that the diketopipersazines isolated were due to polymerisation subsequent to the hydrolysis of the protein, since glycine under similar conditions yielded 40 per cent. of the condensed product.

The Chemical Nature of the Vitamins.—Takahashi (*J. Chem. Soc., Japan*, 1923, **44**, 580) has isolated from cod-liver oil a semi-crystalline substance of which 0·08 milligrammes per day for ten days completely cured a mouse in extreme deficiency of vitamin A. The substance contained no nitrogen, was aldehydic, unstable to light and oxygen, gave the lipochrome reactions, and was soluble in the usual fat solvents. The daily press has recently reported the isolation of vitamin B by the American worker, Dr. Eddy, but no details have yet been vouchsafed to the scientific world. The chemistry of the anti-scorbutic factor has progressed under the investigations of S. S. Zilva (*Biochem. Jour.*, 1924, **18**, 182) and we may be hopeful of further light on this member of the vitamins. It may be noted with approbation that there is an increasing tendency to regard the accessory food factors rather as necessary nutritional elements, whose requirement is probably small just as the requirement of the animal for iodine is small but imperative, than as specific agents of a catalytic or stimulative function.

PHYSICAL CHEMISTRY. By W. E. Garner, M.Sc., University College, London.

The Law of Mass Action.—Deviations from the law of mass action in chemical processes were formerly ascribed to the complexity of the chemical reactions, it being nearly always possible to discover a mechanism which fitted the experimental

results. During the last decade, with the introduction of the conception of activity, a change of view has taken place which has led to numerous attempts to replace the concentrations in the equations of the law of mass action by the activities of the reacting substances. At first, the abstract nature of the thermodynamic conception militated against its rapid acceptance, but now it is replacing the concentration hypothesis in many expressions for systems in equilibrium. It is, however, being applied somewhat indiscriminately to all of the apparent deviations from the older mass action law, and there is considerable danger that a true knowledge of the mechanism underlying chemical reaction may be lost sight of, if the new weapon be allowed to completely replace the old. As Bjerrum has pointed out (*Z. Phys. Chem.*, 1924, **108**, 82), the conception of activity should be used only with the utmost caution if the system is far removed from the equilibrium state. Also, the concentration hypothesis when logically used is as free from thermodynamic objection as is the conception of activity.

The catalysis of the inversion of sucrose by hydrogen ions has been very closely examined recently by numerous workers. It is found that the velocity constant of this *unimolecular* reaction increases with increase in sucrose concentration and an explanation of this is being sought in terms of the changes in the activity of the hydrogen ion which are found to occur. The assumption that the rate of reaction is proportional to the activity (Lewis and Jones, *J. Chem. Soc.*, 1920, **117**, 1120; Lewis and Moran, *ibid.*, 1922, **121**, 1613) is open to criticism. The activity of the hydrogen ions in a sucrose solution undergoing inversion may readily be determined by means of the hydrogen electrode, and the assumption is made by (Lewis, *loc. cit.*, and Scratchard, *J.A.C.S.*, 1921, **43**, 2398) that the activity of a hydrogen ion in catalysis is the same as that measured by the hydrogen electrode. On this assumption, complex equations are derived for the chemical mechanism of the inversion. Lewis and Moran (*loc. cit.*) have shown that the velocity constant on the concentration hypothesis does not increase so rapidly with increase in sucrose concentration as the activity of the hydrogen ion, and there is no reason why this should be the case even if, as is usually assumed, the reaction be of the first order. The effect of changes in the degree of hydration of $H^{\cdot}$ with increasing sucrose concentration has been overlooked. Any change in its degree of hydration will modify its activity as a catalyst. The water envelope may be regarded as a protective covering, hindering the catalytic action and decreasing the fraction of collisions which are fruitful of chemical change. The relationship between the degree of hydration and the catalytic activity of the ion is undoubtedly very

complex, and, at present, is beyond our means of calculation; but there is no *a priori* reason why its activity as a catalyst should run *pari passu* with that of its activity in the thermodynamic sense.

Brönsted, in a new theory of reaction velocity which deals principally with the neutral salt effect (*Z. Phys. Chem.*, 1922, **102**, 169), very largely confines his attention to solutions so dilute that changes in hydration are almost negligible and, under these conditions, the employment of activities instead of concentrations is not open to the same objections. In ionic solutions, the concentration is not of great service, especially if the theory of complete dissociation be correct. The activities of the ions, on the other hand, are very largely determined by the electrical forces between them and are a rough measure of the effective concentrations.

As an example of his theory, taking the reaction (1) $Fe^{+++} + y^{-} \rightarrow Fe^{++} + y$, he assumes that the velocity is determined by the production of a molecular complex $(Fe\ y)^{++}$, according to the reaction (2), $Fe^{+++} + y^{-} \rightarrow (Fe\ y)^{++}$, with a velocity $h = k\, C_{Fe^{+++}} C_{y^{-}} \ldots f_1 f_3 / f_2$ where f_1, f_2, and f_3 are activity coefficients of the uni, bi, and trivalent ions respectively. Thus the velocity constant obtained from concentrations must be multiplied by a factor containing the ratios of the activities. The general equation is $h = k\, c_1, c_2 \ldots F$, where F is an activity factor dependent on the nature of the ions, and on the total equivalent concentrations. These equations have been derived more accurately by Bjerrum, by treating (2) as an equilibrium process, to which activities may be applied. It is possible to divide ionic reactions into types according to the nature of F. For reactions between ions and neutral molecules, *e.g.* sugar inversion, $F = 1$, and is practically independent of the concentration, providing that the total ionic concentration, reacting ion and neutral salt, is low. In these cases, the concentration hypothesis holds with considerable accuracy. Reactions between ions, on the other hand, are abnormal, in some cases the velocity increasing with rise in concentration of neutral salt, and in others decreasing. In reactions between ions of the same signs, the salt effect is positive and the velocity is accelerated. On the other hand, the addition of salts reduces the velocity where the reaction is between ions of opposite sign. These considerations have great value in interpreting the vagaries of the neutral salt effect; but, as Bjerrum points out, there is no necessity to depart from the concentration hypothesis to derive Brönsted's equations.

Activity Coefficients of Strong Electrolytes.—During the past year the theory of *complete* dissociation of strong electrolytes has been extended both from theoretical and practical points

of view. This theory which seeks to explain the deviations from the simple gas laws solely in terms of the electrical forces between the ions has been elaborated by Milner and Ghosh (SCIENCE PROGRESS, 1922, **66**, 195) and, more recently, by Debye and Hückel, and has received considerable experimental support from G. N. Lewis and Brönsted. J. N. Brönsted and V. K. la Mer (*J.A.C.S.*, 1924, **46**, 555), in supporting this theory, incline to the opinion that there are three distinct regions of dilution for dilute solutions of electrolytes: (1) extreme dilutions where the gas laws are applicable within the limits of experimental error, (2) intermediate dilutions at which the deviations from the gas laws depend only on the valence type, and (3) a region of higher concentration where the deviations are occasioned, in part, by the size and chemical nature of the ions. Brönsted, who has interested himself in the intermediate region, has found that the activity coefficient is given by the following empirical equation: $\log_e f = 3\,a\sqrt{c}$, where f = activity coefficient, $a = 0{\cdot}32$, and c = concentration. This formula shows that even at $c = 10^{-5}$ M the deviation from the simple gas laws is 0·1 per cent. It is unlikely that such a simple equation would hold if the changes in the activity coefficient were in part caused by a displacement of an electrolytic equilibrium.

Influence of the Valency of the Ion.—A. A. Noyes and his collaborators showed that a rough proportionality could be obtained between a, the degree of ionisation and the valency product of the ions. Bjerrum (*Z. Electrochem.*, 1918, **24**, 321) suggested that the activity coefficient was directly proportional to the square of the valence, and Brönsted has confirmed this by measurements of the solubility of slightly soluble electrolytes in solutions of electrolytes of different valence types. Adopting G. N. Lewis's convention of ionic strength, $\mu = \frac{1}{2}(c_1 z_1 + c_2 z_2 \ldots)$ where $c_1, c_2 \ldots$ are equivalent concentrations of ions with valencies $z_1, z_2 \ldots$ respectively, Brönsted shows that $-\log_e f = 3\,a\,z_1\,z_2\sqrt{\mu}$ for a salt composed of two ions. This equation is in agreement with that deduced by Debye and Hückel (*Physikal. Z.*, 1923, **24**, 185; SCIENCE PROGRESS, 1924, **71**, 471) for the activity coefficient of a salt composed of two ions, *e.g.* $\log_e f = e^2\kappa/2\mathrm{D}k\mathrm{T}\,(z_1 z_2) = -1{\cdot}14\,z_1 z_2\sqrt{\mu}$, where e is the charge on a univalent ion, k the Boltzmann constant, D the dielectric constant, T the temperature, and κ a characteristic distance. The Debye and Hückel equation gives $a = 0{\cdot}38$, which is quite close to the experimental value, $a = 0{\cdot}32$.

For higher concentrations, the size of the ion, and their degree of hydration become important, for the ions can no longer be regarded as point charges. For these cases $-\log_e f = 3\,a\sqrt{c} + 2\beta c$,

where β is a factor dependent on the diameter of the ions. This equation also agrees with that furnished by the Debye and Hückel theory. The establishment of the general validity of the above formulæ would be a strong indication of the correctness of the assumption of the complete dissociation of strong electrolytes.

Isotopes.—From the Bohr theory it was predicted that the line spectra of the elements should show evidence of the presence of isotopes. The expected shifts in the wave-lengths due to the different masses of the nuclei were, however, almost too small to measure. The smallness of the isotope shift is due to the fact that one of the particles concerned in the vibration is the electron itself, whose mass is minute compared with that of the nucleus. Shifts of a much larger magnitude are to be expected if the vibration causing the light is due to two atoms or nuclei in combination. Imes (*Astrophys. J.*, 1919, **50**, 251) observed that the infra-red spectra of hydrochloric acid were accompanied by faint satellites. These satellites Loomis (*Nature*, 1920, 179) attributed to the isotopes of chlorine. The shift calculated on this assumption agreed with the observed value.

More recently, Mulliken (*Science*, 1923, **58**, 164; *Nature*, 1924, **113**, 423, and 489) and Nagoaka, Sugiura, and Mishima (*Nature*, 1924, **113**, 459, and 532) have found that a large number of non-series lines of many elements may be attributed to vibrations of the atoms in pairs. Thus neon can vibrate in the pairs (20, 20) (22, 22) and (20, 22). The difference in wave-length of the spectra due to the two symmetrical pairs of isotopes may be represented by $\delta\lambda = (1 - \sqrt{m_1/m_2})\lambda_2$. Similar formulæ may be derived for the differences between symmetrical and unsymmetrical pairs. Neon, chlorine, argon, copper, zinc, lithium, copper, bromine, and rubidium all give spectral lines due to the existence of such pairs. Also, pairs like BO, SiN, and CuH give band spectra with satellites in the positions predicted from the mass of the isotopes. In all cases agreement is found with the results of the positive ray method. Nagoaka, and Sugiura conclude that calcium contains a trace of isotope 44, sodium of 21, and that the isotopes of barium are 135, 137, 138, and 139.

For elements with atomic numbers 22 to 28 the spectral lines are so numerous that it is possible to conceive of the existence of many isotopes in these elements. Aston, on the other hand, finds that they are simple with exception of 28. It is suggested that these lines are due to nuclear agitations, and may be of enormous importance in elucidating the structure of the nucleus.

Colloidal Electrolytes.—In order to reconcile the conductivity,

mosotic pressure, and viscosity data of solutions of soap, protein, and dye solutions the assumption was made by McBain that an ionic micelle is present in concentrated solutions with a mobility higher than that of the ions aggregated to form it. This view has been established by McBain and Downey (*Trans.*, 1923, **123**, 2417) by migration measurements with potassium oleate and laurate. In dilute solution, where the soap is crystalloidal, the ordinary migration number is observed, and, in all other cases, where the soap is a colloidal electrolyte, the migration of the fatty acid radicle is several times greater than that of the simple ion. The results show that the neutral colloid is not contained in the ionic micelle. Colloidal electrolytes also occur in the extracts from some seaweeds. Harwood (*Trans.* 1923, **123**, 2254) has shown that the calcium salt extracted from carrageen is a colloidal electrolyte which is ionised to the same degree as the salt of any bivalent acid. The colloidal ion of carrageen extract possesses a mobility very similar to that of the sulphate ion.

GEOLOGY. By G. W. Tyrrell, F.G.S., A.R.C.Sc., Ph.D., University, Glasgow.

Orogeny, Diastrophism, and Geosynclines.—The first chapter of F. Heritsch's *Die Grundlagen der Alpinen Tektonik* (Berlin, 1923, pp. 259) deals with geosynclines, orogeny, epeirogeny, and foredeeps. Heritsch regards geosynclines as long, narrow, labile strips of the earth's crust, which do not sink because they get loaded with sediment, but which are filled with sediment because they tend to sink and form hollows. Heritsch makes great play with subaqueous slips and landslides of great masses of sediment on the mobile slopes of the geosynclinal hollows, which produce peculiar brecciation and distortion in otherwise undisturbed strata. These phenomena are often taken as signs of folding and thrusting. They are abundant in the Alps, and this is one of the reasons why Heritsch tends to be very sceptical regarding the more extreme developments of the *decken* theory as applied to the Alps. He also accounts for the great thicknesses of clay in geosynclinals by subaqueous flow of sediment.

Orogeny and epeirogeny work in opposing senses. The first produces new mountains which are joined to the continents; and epeirogenic movements break them down again. Heritsch criticises Suess's view of foredeeps. According to him foredeeps are geosynclines of the second order, which set in with the folding, and expire with it. After interesting chapters on folding and mylonisation, Heritsch passes on to the main topic of Alpine structures. He argues strongly against the *decken* theory in what he calls its present extrava-

gant form; but the *Verschluckungtheorie*, which he puts in its place, following Ampferer and Schwinner, seems to be no less far-fetched. The insucking or swallowing-up theory is founded upon isostatic considerations in the first instance; but for sub-crustal mass translation it adopts a meteorological analogy of cyclonic and anticyclonic movements. A centrifugal streaming of material in orogenic zones creates a diminution of mass beneath the passive tectonosphere which results in the latter becoming too large for its foundations. Thus Heritsch would explain the origin of the fold and imbricate structures of mountain regions. It would take too long to follow out even a few of the implications of this theory; but, to the writer, the possibility of any mass movements in sub-crustal regions at all analogous to cyclonic or anticyclonic whirls, seems very remote.

L. A. Cotton has written a long and elaborate paper on some fundamental problems of diastrophism and their geological corollaries, with special reference to the theory of polar wandering (*Amer. Jour. Sci.* (5), 6, 1923, pp. 453–503). G. H. Darwin's mathematical analysis set limits to polar wanderings which were based on the assumptions that only surface rock masses were involved in displacements, and that the earth as a whole behaved as a perfectly rigid body. He concluded, however, that quite extensive polar wandering was possible if the earth were considered capable of yielding like a viscous solid for stresses of secular duration. The whole question thus depends on the primary assumptions that are made as to the nature and constitution of the interior of the earth. Cotton's inquiry shows that mathematical theory, and geodetic, seismological, and geological evidence, all support the conclusion that the earth as a whole behaves like a highly viscous solid, and hence that polar wandering is not only possible but probable. The theory of polar wandering may then take its rightful place in helping to explain many obscure geological and palæoclimatological problems—explanations previously barred by the mathematical opposition to the theory.

According to Sonder ("Die Erdgeschichtlichen Diastrophismen im Lichte der Kontraktionslehre," *Geol. Rundschau*, xiii, 1922, 217–72) the complete cycle of geological history accomplished in an era includes a well-nigh universal orogenic period followed by a long continental stage, and a period of increasing oceanic transgression, after which the cycle recurs with renewed orogeny. This is believed to result directly as a consequence of continuous earth contraction. The usual assumption that the earth's crust is a completely rigid body in which shrinkage can only take place by inbreak or folding is incorrect, as the crust must be elastic. A review of experi-

mental determinations of the elastic moduli of rocks shows that the greater part of the shrinkage due to contraction can be met by elastic shortening of the crust. The freed contraction energy is stored as a waxing crustal stress proportional to the shortening. Finally it passes the elastic limit and folding ensues in the weaker parts of the crust. By this mechanism the continuous process of contraction may develop a periodic folding.

The inhomogeneity of the crust involves variations in the moduli of elasticity, whereby stress-differences originate in the shell of compression. With great stresses aniostatic crustal deformation occurs, which is comparable to a slight wave motion of great wave-length. The lower parts of the continents thereby sink below sea-level, and transgressions are caused which are the more marked the greater the tangential stress. Folding reduces the crustal stress, and, if sufficiently extensive, will bring the deformation and transgression to an end. Thus the interplay of waxing crustal stress and folding results in the above postulated course of the geological cycle.

In the second part of his paper Sonder discusses geological history and attempts to apply his views thereto. He assumes the essential permanence of the continents and ocean basins. The geological record from the Cambrian to the present is divided into three major cycles, each of which is subdivided into a continental stage, a submergence stage, an alternation (*Wechsel*) stage, and an emergence stage. The first of these is a period of diastrophic quietude, represented in the three major cycles respectively by the Eo-Cambrian (Torridonian, Sparagmite, etc.), the Lower Old Red Sandstone, the Triassic, and the Present, which is regarded as the continental stage of a fourth cycle. The submergence stage is the period of geosynclinal deposition, and is represented by the Cambrian, Devonian, and Jurassic periods respectively in the three cycles. The alternation stage is the period of maximum transgression of shallow seas upon the continental pedestals, and is represented by Ordovician and Early Silurian, the Lower Carboniferous, and the Cretaceous in the three major cycles. Finally, the emergence stage is that of maximum orogeny, represented in the successive cycles by the Late Silurian, the Upper Carboniferous and Permian, and the Cainozoic. In this scheme the Cainozoic is therefore regarded as merely the concluding stage of the Mesozoic Era, and the Present is the opening stage of a new cycle. The climax of igneous activity coincides with the climax of orogeny. The great glaciations and desert formations occur immediately after orogenic paroxysms, *i.e.* at the beginning of the continental stages.

F. P. Shepard questions the theory of world-wide periodicity

of orogenic movements (*Jour. Geol.*, **31**, 1923, 599–613) on the ground that geological history shows a record of almost continuous diastrophism from the Ordovician to the Present. Further, the apparent concentration of diastrophism at special epochs may be due to the loss of record by erosion, to unrecorded sub-oceanic orogeny, and the possibility of the shortening of the crust by minor flexures, which are important in their totality, but leave no record of their time. Shepard thinks that the earth's crust cannot accumulate large stresses, and is able to support only a fraction of its own weight. Hence it would be more or less continuously folded in order to adjust itself to a shrinking interior.

In a second paper on the bearing of normal faults on the hypothesis of a shrinking earth (*Amer. Jour. Sci.*, vii, 1924, 146–54) Shepard asks why elongation or tension faults, which imply an expanding interior, are spoken of as "normal" faults, whilst compression faults, which should accompany a shrinking interior, are regarded as "reverse" faults. He further questions the alleged relative abundance of normal faults as opposed to reverse faults. Vulcanicity is suggested as a possible cause of normal faulting, without necessitating the cessation of compression arising from earth shrinkage. Increase of folding with depth may also cause normal faulting towards the surface in regions where vulcanism cannot be the originating cause.

PLANT PHYSIOLOGY. By R. C. Knight, D.Sc., Imperial College of Science and Technology and East Malling Research Station (Plant Physiology Committee).

The Carbohydrate/Nitrogen Ratio.—The gap which exists between plant physiology and horticultural practice has not yet been bridged at more than a few points. The reason may be that the gap is rather wide and the bridging material scanty, or it may be that in so young a science as physiology the worker prefers to turn his attention to academic rather than to applied problems. Whatever may be the merits of the case, it is undisputed that horticultural practice is almost entirely empirical, and such operations as pruning and manuring are carried out by rule-of-thumb methods which are largely dictated by tradition. The inevitable result of applying rigid methods to a variety of subjects under a variety of conditions has been divergence of results, and a consequent conflict of opinion, which serves to emphasise the necessity for the application of critical methods of research to the problems involved. The necessity for physiological analysis of results hardly requires emphasis, but it is only in recent years that efforts

have been made to arrive at physiological interpretations of the facts involved in horticultural practice.

The development of the theory of the carbohydrate/nitrogen ratio is at present the outstanding example of the attempt to investigate the problems of horticulture from the physiological viewpoint. There is general recognition that heavy nitrogen manuring may induce strong vegetative growth, that certain types of pruning practice generally result in certain modifications of the form of growth of a tree, but no comprehensive attempt at analysis of these phenomena had been made until American physiologists turned their attention to the carbohydrate and nitrogen nutrition of horticultural plants. For some years Americans have carried out systematic studies of the chemistry of plants with the object of investigating the correlation which must exist between the internal chemical condition and the phenomena of growth.

The work of Kraus and Kraybill ("Vegetation and Reproduction, with Special Reference to the Tomato," *Oregon Agric. Coll. Exp. Sta. Bull.*, **149**, 1918) constitutes the first co-ordinated work on this subject, and this bulletin may be said to have been the starting-point of a series of researches on the relation of carbohydrates and nitrogen to growth and reproduction.

Previous to 1918 a good deal of work had, of course, been carried out on carbohydrates and nitrogen in crop plants, but Kraus and Kraybill were the first to attempt any co-ordination of the known facts of growth with the results of their carbohydrate and nitrogen investigations. Thus their enunciation of the theory of the importance of the carbohydrate/nitrogen ratio may be regarded as a fresh starting-point for physiological research in horticulture, more particularly in America. At the present time many horticulturists are planning and conducting field experiments with this theory as a basis, and the physiologists who are dealing with horticultural problems are also investigating the theory from many points of view.

The main point of the theory of Kraus and Kraybill is that the relation between the amount of vegetative growth and the amount of reproductive growth made by a plant, is determined by the relative amounts of carbohydrates and nitrogenous substances which are available within it. A plant which is liberally supplied with nitrogen produces much vegetative growth but little fruit. A plant with a deficient nitrogen supply produces little vegetative growth and little fruit. In the intermediate position with a moderate supply of nitrogen, the plant produces an intermediate quantity of vegetative growth and the maximum quantity of fruit. "Fruitfulness is associated neither with highest nitrates nor highest carbo-

hydrates, but with a condition of balance between them." Thus with the magnitude of the carbohydrate/nitrogen ratio is associated a definite type of growth. A low ratio results in vegetative growth, and a very high ratio reduces vegetative growth, without inducing fruitfulness. It is when the ratio has an intermediate value that a vigorous and fruitful plant is obtained. There are many corollaries to this theory, which Kraus and Kraybill were able to prove. For example, a vigorously vegetative, unfruitful plant can be made to fruit by a decrease in the nitrogen supply, with the consequent increase in the carbohydrate/nitrogen ratio. Again, if nitrogen is supplied to a plant which has been deficient in nitrogen, the result depends upon the quantity of available carbohydrate. If the latter is low, considerable vegetative growth and little fruit production follows nitrogen addition, whilst if the carbohydrate is high, an increase in nitrogen tends to induce fruit production. Such considerations may explain the various effects which, under different conditions, may follow the application of nitrogenous manures.

It was immediately recognised that the implications of this work were of great importance to horticulture, and particularly perhaps to the culture of perennial plants. Some of the most important cultural practices are of a nature which must influence the carbohydrate/nitrogen ratio. For example, nitrogen manuring, whether effected by direct application, or by cultivation of leguminous crops, naturally influences the quantity of nitrogen available for the plant. Pruning, again, must affect the carbohydrates as well as nitrogen ; winter pruning by removing stored materials, and summer pruning by restricting assimilatory activity in addition. Kraus and Kraybill were careful to point out that they did not consider that the carbohydrate/nitrogen ratio alone was of importance in determining the type of growth produced. Other environmental conditions and hereditary factors cannot be neglected, but in view of the fact that even pollination and fertilisation may not result in fruit production unless nutrition conditions warrant it, the value of the theory as a working basis cannot be questioned. Thus it is that physiologists and horticulturists have extended and amplified the observations of Kraus and Kraybill with respect to other plants and other conditions.

The work of Woo ("Chemical Constituents of *Amaranthus retroflexus*," *Bot. Gaz.*, 1919, **68**, 313–45), following immediately upon Kraus and Kraybill's work, provided an excellent indication of the limitations of the theory of the carbohydrate/nitrogen ratio. Woo found that *Amaranthus* is a plant which has an apparently exceptional capacity for the absorption and retention of nitrates. Large quantities of nitrates may be

present and the plant may still not be "forced out of reproduction," although the ratio is low. It is suggested that this nitrate-absorbing capacity may account for the "success" of the plant as a weed, and for its bad effects upon cultivated crops. From this work it appears that a different range of carbohydrate/nitrogen ratio is required to produce the same range of effects in different plants. This is a reasonable conclusion, and later work has demonstrated its soundness. Gurjar ("Carbon/Nitrogen Ratio in Relation to Plant Metabolism," *Science*, 1920, **51**, 351–2), working with tomato, found that the ratio, which is normally influenced chiefly by the nitrogen supply, may vary between 2 and 19, but fruiting occurs only when the ratio is between 4 and 6. It may be mentioned here that Harvey and Murneek, in an investigation of the apple, recorded values between 22 and 40 for the ratio in fruiting spurs. (Murneek, in his Table *X*, designates his ratio as "c/n $\frac{\text{total nitrogen}}{\text{total carbohydrates}}$" but this obviously should read "c/n $\frac{\text{total carbohydrates}}{\text{total nitrogen}}$".) This work is discussed later. Thus the range of the ratio varies in different plants, and probably for different parts of a plant, as Woo concluded it must. Gurjar found that there was also a correlation between the ratio and the rate at which photosynthesis and respiration are carried on, but this aspect of the problem must be exceedingly complex, and extreme difficulty is likely to be encountered in distinguishing between cause and effect.

The application of Kraus and Kraybill's methods to apples soon followed. A certain amount of work had already been carried out on the chemistry of apple-trees (see Butler, Smith and Curry: "Physiology of the Apple: Distribution of Food Materials in the Tree at Different Periods of Vegetation," *New Hampsh. Coll. Agric. Exp. Sta. Bull.*, **13**, 1917), but this had been chiefly of a general nature, rather than with particular reference to carbohydrates or nitrogen. The problem in the apple-tree is obviously of a character quite different from that in a herbaceous plant of the type of the tomato. In the latter, the fruiting function passes progressively from the base to the apex, and different parts of the same plant are in different stages at the same time. With the apple, every spur bearing fruit is in approximately the same stage at the same time. Thus it would appear that, by taking samples consisting only of fruit spurs, a more reliable indication of the relation of chemical composition to fruiting should be obtained than if the sample included many other parts of the plant, as in the previous investigations. On the other

hand, it is not certain to what extent the spur may be regarded as a unit. Considerable emphasis has been laid on the "individuality" of the spur, but later work has shown that the spur may draw on reserves in other portions of the tree. The use of the spur as a unit is very convenient, however, and renders it possible to carry out many analyses, using only a few trees. The uniformity of results is naturally greater under these circumstances than if many different sources of material must be employed. The habit of biennial bearing in apples has made it possible to distinguish between two phases of fruit-production. The fruiting spur of a tree may be regarded as producing fruit-buds in one year and developing flowers and fruit in the next year. Thus the cycle of changes extends over two seasons. Hooker ("Seasonal Changes in the Chemical Composition of Apple Spurs," *Univ. of Missouri Agric. Exp. Sta. Bull.*, **40**, 1920) carried out serial analyses of apple spurs over a two years' cycle, and correlated the carbohydrate-nitrogen relations with the type of growth taking place. He found that at the beginning of the non-blooming year, the nitrogen is low and carbohydrate relatively high. When growth begins, there is a reduction in the quantity of both substances, which proceeds until the cessation of growth and the formation of terminal buds. From this stage, the photosynthetic activity of the now mature leaves results in an accumulation of carbohydrates in the spur, but the nitrogen content still remains low. During this period, fruit-buds are differentiated. Accumulation of both carbohydrates and nitrogen in the spur continues until the end of the season, at which period both of these materials are at their highest concentrations. In the following spring, the process of flowering is accompanied by a rapid depletion of the spur of carbohydrates, but nitrogenous materials are not removed until the development of fruit and seeds takes place. At this stage the spur is at its lowest efficiency as a storage organ, with the result that little vegetative growth takes place, and few fruit-buds are formed.

These analyses show that in the non-fruiting year the carbohydrate/nitrogen ratio is at first low, and much vegetative growth results. There follows, in the summer, an increase in the ratio and the formation of fruit-buds. In the following year, the production of flowers reduces the ratio to a low value by the time fruit is setting, and during the summer the low value is maintained until nitrogen is used in seed production. This work confirms the earlier results in showing that a decrease in the ratio is followed by the production of vegetative growth, whilst the formation of fruit primordia accompanies a high ratio. In addition, a low

ratio is found to be characteristic of the fruit-setting period. Hooker supplemented this work by field trials of the effect of nitrogenous manures on fruit production ("Certain Responses of Apple Trees to Nitrogen Applications of Different Kinds and at Different Seasons," *Univ. of Missouri Agric. Exp. Sta. Bull.*, **50**, 1922). He was able to show that addition of nitrogen to the soil in spring increased the nitrogen content of the spurs at the period of fruit-setting, and there was a consequent increase in the set of fruit. Hooker points out, however, that this work indicates the importance of considering the condition of the tree in determining the nature and time of application of nitrogen. For example, application of nitrogen to a tree in which the ratio is already too low will obviously not tend to increase the production of fruit-buds. Harvey and Murneek ("The Relation of Carbohydrates and Nitrogen to the Behaviour of Apple Spurs," *Oregon Agric. Coll. Exp. Sta. Bull.*, **176**, 1921) also worked with samples of apple spurs, and altered the carbohydrate/nitrogen ratio by defoliation. Spurs were defoliated in mid-June, just before the period in which fruit-buds are differentiated, and this was followed by a reduction of 48 per cent. in the number of fruit-buds produced. Samples of spurs were analysed at intervals after defoliation, and it was found that whilst little change took place in the carbohydrates, nitrogen was more abundant in defoliated than in normal spurs. Thus the ratio, which normally increased by over 50 per cent. in six weeks, increased by less than 20 per cent. in defoliated spurs during this period. In defoliated spurs, the ratio at the last analysis was 18 per cent. below that in normal spurs. Harvey comments upon the difference between this 18 per cent. decrease in ratio and the 48 per cent. decrease in fruit-bud formation, apparently expecting that if the ratio were of paramount importance in determining fruit-bud development, the decrease observed would have been numerically identical in the two cases. It appears, however, that no numerical parity could possibly be expected between such an arbitrary ratio and an almost equally arbitrary measure of fruitfulness.

Harvey and Murneek also studied the influence of spring defoliation upon fruit-setting. Defoliation in April reduced the quantity of fruit set almost in proportion to the extent of defoliation, so that the number of fruits set per spur varied directly with the number of leaves remaining. Analyses showed that April defoliation increased the carbohydrate/nitrogen ratio by both increasing carbohydrate and decreasing nitrogen. This decrease of fruit set accompanying increase of the ratio is quite in accordance with Hooker's work, which showed that setting normally occurs when the ratio is low.

Kraus and Kraybill also recorded the opinion that the conditions accompanying fruit-setting were probably different from those favouring fruit-bud formation, a theory which has been amply justified by Hooker and by Harvey and Murneek. The work on defoliation raises several interesting questions as to the reasons for the radical differences in the influence of defoliation at different seasons upon the contents of the spur. Probably defoliation alters the extent to which the spur is dependent upon other tissues. Further detailed analyses of the type carried out by Butler, Smith, and Curry (above) would be invaluable in this connection.

In a later publication Harvey ("A Study of Growth in Summer Shoots of the Apple, with Special Consideration of the Role of Carbohydrates and Nitrogen," *Oregon Agric. Exp. Sta. Bull.*, **200**, 1923) records the effects upon growth rate of defoliation and ringing at different seasons of the year. Defoliation retards growth considerably if effected during the period of maximum growth rate. Defoliation earlier in the season has a much smaller retardating effect. Defoliation late in the season retards growth very little or may even result in acceleration. The effect of ringing shoots is in general the opposite of that following defoliation. Analyses of shoots were made at intervals and the results were correlated with the growth phenomena.

Gardner ("Studies in the Nutrition of the Strawberry," *Univ. of Missouri Agric. Exp. Sta. Bull.*, **57**, 1923) found that the yield was influenced to a large extent by the nutritive conditions in the autumn at the time of formation of fruit-buds. The maximum number of flower clusters were obtained when the conditions during the previous autumn were such as to produce a high carbohydrate content. His tables show that high carbohydrate content also involved a high carbohydrate/nitrogen ratio, since the nitrogen content was almost constant. Roberts ("Nitrogen Reserve in Apple Trees," *Proc. Amer. Soc. Hort. Sci.*, 1921, 143–5) presents evidence that apple-trees may store a reserve of nitrogen in one season, which may be utilised in a later season, when the external supply is low. Thus, trees transferred from a medium high in nitrogen to a medium lacking nitrogen made nearly as much growth as trees still provided with abundant nitrogen. The nitrogen content of the branches of the former, however, decreased by nearly 70 per cent. as compared with a 9 per cent. loss by trees not deprived of external supply. Conversely, trees which have been starved of nitrogen make good growth when nitrogen is supplied, and in addition increase their nitrogen content by 40 per cent. Roberts also records the influence of carbohydrate and nitrogen content upon fruit-bud formation. A very high

c/n ratio, accompanied by low absolute nitrogen-content, resulted in very little growth and the complete suppression of fruit-bud formation. A very low ratio, with high nitrogen, produced much vegetative growth but very few fruit-buds. When the ratio had an intermediate value, with both carbohydrates and nitrogen present in intermediate quantities, the trees were fairly vigorously vegetative and produced abundant fruit-buds.

The work of Garner and Allard (" Effect of the Relative Length of Day and Night and other Factors of the Environment on Growth and Reproduction in Plants," *Jour. Agric. Res.*, 1920, **18**, 553–606, etc.) must be considered in relation to the carbohydrate/nitrogen ratio. Garner and Allard showed that the type of growth of a plant, whether vegetative or reproductive, can be determined by regulating the daily period during which the plant is exposed to light. Some plants require a " long day " to enable them to flower, whilst others flower only if the daily period of exposure to light is short. It is reasonable to suppose that the difference of exposure results in a difference of carbohydrate accumulation with a consequent change in the c/n ratio. Garner and Allard found, however, that apparently it was not the total quantity of radiation received which produced the observed effects, but rather the actual length of the exposure period. Plants exposed for long daily periods to sunlight of one-quarter of full intensity behaved similarly to plants exposed to full intensity for the same period, and not in the same manner as plants exposed to full intensity for one-quarter of the length of time. This might be interpreted to mean that the total amount of carbohydrate produced is *not* an important factor in flower production, but there is no evidence to show whether the differences in total radiation were, or were not, accompanied by differences in the photosynthetic activity. It may be that in the experiments light was never a limiting factor, and that other conditions regulated carbohydrate production, so that the change of intensity had no effect upon photosynthesis. Correlation of this work with chemical analyses is obviously desirable. Nightingale (" Light in Relation to the Growth and Chemical Composition of some Horticultural Plants," *Proc. Amer. Soc. Hort. Sci.*, 1922, 18–29) has investigated the possibilities of influencing the carbohydrate/nitrogen ratio in tomatoes, salvias, buckwheat, radishes, and soybeans, by nitrogen manuring, and by regulating the period of daily exposure to sunlight. Apparently, under greenhouse conditions in the latitude of Wisconsin, light was definitely a limiting factor in carbohydrate production, since Nightingale was able to alter the ratio by altering the carbohydrate content, as the

result of varying the daily period of exposure. The results following the alteration of the ratio were in agreement with those formerly obtained by Kraus and Kraybill, but Nightingale in addition obtained evidence that carbohydrates are used in the manufacture of proteins from nitrates. The nitrogen content of the plants was separated into an insoluble portion (chiefly proteins) and a soluble fraction (the simpler nitrogenous substances). If carbohydrate formation is decreased, nitrates accumulate, and the formation of insoluble nitrogenous substances ceases. Conversely, if a plant which is low in carbohydrates is transferred to conditions favouring photosynthesis, the resulting increase in carbohydrates is accompanied by a reduction of nitrate-content and an increase in insoluble nitrogen. Further investigations along such lines appear to be promising, but the inevitable first effect of progress has been to demonstrate the complexity of the problem. There is primarily the difficulty of locating the region in which changes in the carbohydrate-nitrogen balance are important. The stem is naturally suggested in the case of deciduous perennials, but a somewhat greater discrimination is necessary in dealing with such plants as these. Again, there is the question of cumulative and reciprocal influences. The excessive vegetative growth induced by a low c/n ratio must react upon the ratio by virtue of the increased assimilating area. In such a case as the apple, it may be deduced from the work of Hooker and of Harvey and Murneek that such factors as the length of the growing season, as influenced by weather, will also be a factor of importance. Finally, it may be necessary in many cases to consider not only accumulated reserves, but also the relative rates of nitrogen absorption and of carbohydrate synthesis at critical periods of growth.

ENTOMOLOGY. By J. Davidson, D.Sc., Rothamsted Experimental Station, Harpenden.

General Entomology.—A. Berlese (*Redia*, **15**, 115–75) discusses certain considerations (adaptability, reproduction, variation, etc.), in the evolution of the organism with reference to insects. This is a general summary of the author's views which will be more fully expanded in the next fascicle of *Gli Insetti*. Criticism is invited from students interested in the subject. An interesting account of the effective use of an aeroplane distributing an insecticide is given by B. R. Coad, E. Johnson, and C. L. McNeil (*U.S. Dept. Agric. Bull.*, 1924). Calcium arsenate was distributed as a dust over a cotton field attacked by the cotton leaf-worm, *Alabama argillacea*. From 400 to 500 acres can be dusted per hour, the insecticide being used at the

rate of 9 lb. per acre. G. C. Crampton, in continuation of his morphological studies, discusses the labium in certain holometabolous insects from the standpoint of phylogeny (*Proc. Entom. Soc. Washington*, **25**, 171–80). The acid and alkaline conditions obtaining within the different regions of the digestive tract of the larvæ of *Psychoda* and *Chironomus*, expressed as hydrogen-ion concentration values, have been investigated by W. J. Crozier (*Journ. Gen. Physiology*, **6**, 289–93). The larvæ were kept in solutions of appropriate non-toxic indicators.

L. O. Howard (*Proc. Entom. Soc. Washington*, **26**, 25–46) gives a very interesting and comprehensive survey of the position regarding the biological control of economic insects by insect parasites. The same author shows that one must not always expect speedy results from introduced parasites (*Proc. Nat. Acad. Sci. Washington*, **10**, 16–19). The parasite or predator may take a long time to establish itself in the new area, as is seen in the case of *Scutellista cyanea* and *Pleurotropis epigonus*. The former species introduced into Louisiana in 1898 was not recovered until 1922. The latter introduced as a parasite of the Hessian fly in 1894 was not recovered until twenty-two years later ; it has now become a common parasite of the Hessian fly in certain states. Stephen Kopec contributes two interesting papers on the influence of starvation on development and the duration of life in insects (*Biol. Bull.*, **46**, 1–21, 22–34). Experiments were carried out with *Lymantria dispar L.*, the caterpillars being totally deprived of food during certain days and fed freely during the remaining days. Intermittent starvation produced prolongation of the larval life and an abbreviation of the pupal period, but did not influence the life of the imago. Age is a factor affecting the results. Caterpillars starved from about the seventh day after the final moult showed retarded pupation, whereas pupation was accelerated by starvation from the tenth day after the last moult. The author is of the opinion that insect metamorphosis is checked by means of secretions from the larval brain. When females derived from starved caterpillars were mated with control males the offspring were not appreciably affected, but injurious effects were produced in the offspring by inanition of the male, this presumably being due to the different metabolism of the sexes.

A. D. MacGillivray has written a book entitled *External Insect Anatomy* (Urbana, Illinois, Scarab Co.). The work aims at being a guide to the study of insect types in the laboratory. The lack of illustrations and the wealth of new technical terms introduced are noticeable disadvantages in what is otherwise a useful guide to the study of the external anatomy of insects. E. Séguy has compiled a reference book on the insect parasites

of man and domestic animals (Paris, Paul Lechevalier, price 30 frs.). The conclusions to be drawn from the 122 species of upper Triassic insects collected at Ipswich, Queensland, are discussed by R. J. Tillyard (*Proc. Linn. Soc. N.S. Wales*, **48**, 481–98); 63 genera, 32 families, and 10 orders are represented.

Orthoptera.—Further studies on the Dermaptera and Orthoptera of Columbia are given by H. Morgan (*Trans. Amer. Ent. Soc.*, **49**, 165–313). This paper deals with the family Acrididæ and 61 general with 96 species are dealt with, of which 9 genera and 47 species are new.

Coleoptera.—G. C. Champion describes two new genera and species of Clavicorn beetles of a new sub-family, the Thaumastodinæ, *Thaumastodus fusiformis* from Philippines and *Acontosceles hydroporoides* from India (*Entom. Mo. Mag.*, **59**, 25–9). Some interesting notes on the biology of *Tenebroides mauritanicus* L., a common insect pest in stored grain, are given by R. T. Cotton (*Jour. Agric. Res.*, **26**, 61–8). In Columbia the beetle overwinters in the adult and larval stages. Eggs are laid in spring in flour, etc., in batches of 10 to 40 eggs. They hatch out in 7 to 10 days and the larvæ may be active from 39 days upwards and then burrow into the soft woods of floors, bins, etc., where they pupate. The adults emerge in from 10 to 15 days. A paper on the characters and biology of Cerambycid larvæ of North America by F. C. Craighead will prove useful to systematists of that group (*Dom. of Canada Dept. Agric. Bull.*, **27**, 1–150). The author indicates the correlation between form and modification of the larvæ and their environment. A short account of the habits of the adults is given and a general discussion of the characters of the larva in each case, with biological notes on food and habits. In continuation of the revision of the Amycterides, E. W. Ferguson deals with the Eumoides (*Proc. Linn. Soc. N.S. Wales*, **48**, 381–435). Nineteen genera are dealt with, of which two are new. An interesting note on the breeding of *Ptinus sexpunctatus* Panz. from the mud cells of the bee *Osmia rufa* L. at Oxford is given by A. H. Ham (*Entom. Mo. Mag.*, **59**, 29–31). The statement that *Ptinus* is found in or near houses is thus explained by the nesting habits of this bee. The *Galerucides*, a sub-family of the Australian Chrysomelidæ, form the subject of a paper by A. M. Lea (*Proc. Linn. Soc. N.S. Wales*, **48**). The genus *Monolepta* and the allied forms of *Candesea* are dealt with and many new species described. H. W. Miles gives a detailed account of the biology of *Anthonomus pomorum* in England (*Ann. Appl. Biol.*, **10**, 348–69). All stages in the life-cycle are described and the influence of food-plants and climate on distribution is discussed. The second volume on the beetles of the Spruce in Finland by U. Saalas has now appeared (*Ann.*

Acad. Scientiarum Fennicæ Helsingfors, Ser. A., **22**, 1–746). It includes keys for identification; 367 species are referred to, of which 311 have been observed on spruce. With a view to establishing a more natural classification of the Coleoptera, F. S. Stickney has studied the comparative morphology of the head capsule in this order (*Illinois Biol. Monographs*, **8**, 1–51). Starting with hypothetical types of head capsule of a generalised insect, an adult and larval beetle, the salient features of the various structures of the head capsule in representatives of 105 families have been studied.

Lepidoptera.—The question of colours of Lepidoptera are of great interest and there are many problems which remain unsolved. H. A. Baylis (*The Entomologist*, **57**, 1, 29, 52, 78) discusses the main conclusions of Onslow and others. Colour-producing factors may be pigmentary or structural. Blues and metallic green are usually due to interference, the scales possessing a special structure; a background of a dark pigment, in addition, results in an intense brilliant colour. Scales whose colour by reflected light is due to structure, very frequently show the complementary colour by transmitted light. Little is known about the chemistry of the pigments. An interesting case of seasonal adaptation in the case of *Melitara junctoliniella* Hulst., one of the insects introduced into Australia with a view to controlling the prickly pear, is noted by J. C. Hamlin. This insect is indigenous in North America. In South Texas it produces two generations annually. When introduced into Australia three generations occurred within sixteen months and observations indicate that when the species ultimately adapts itself to the seasons it will have three broods annually (*Jour. Econ. Entom.*, **16**, 420–3). F. Silvestri makes a contribution to our knowledge of the Tortricidæ of the oak (*Boll. Lab. Zool. R. Scuola sup. Agric.*, **17**, 41–107). *Tortrix viridana* L. and *T. loeflingiana* L. have been studied. The former species appears in May and June and eggs are laid on the bark of twigs being covered with debris. The damage it causes is more or less of a temporary nature, and after three or more years the insect is generally kept under control by the increase of natural enemies and other factors. With the latter species, which occurs throughout Europe and Asia Minor, the eggs which are also laid on the bark of twigs are not covered with debris. The author records certain parasites of this moth which have not hitherto been recorded from this species.

Diptera.—R. S. Bagnall and H. Harrison continue their studies on New British Cecidomyidæ (*Entom. Rec.*, **36**, 36–8). Twenty-two species new to the British fauna are given. Some useful critical notes, being chiefly additions and corrections to Surcouf's valuable systematic work on the Tabanidæ, are given

by J. Bequaert (*Psyche*, **31**, 24–40). A discussion of the modifications of the antenna in Diptera, particularly with regard to the so-called fissicorn condition, is the subject of a paper by M. Bezzi (*Proc. Linn. Soc. N.S. Wales*, **48**, 647–59). The author deals especially with fissicorn Tachinidæ, descriptions of new forms from Australia and South America being given. The fissicorn condition in Tachinids is strictly sexual, being confined to the male, and the author gives a table of distinctions of the known fissicorn Tachinid genera based on the character of the male antenna. J. G. H. Frew gives an account of the larval anatomy of *Chlorops tæniopus* Meigen, and two related forms *Meromyza nigriventris* and *Balioptera combinata* (*Proc. Zool. Soc. London*, 1923, 783–821). The same author deals with the external and internal anatomy of the larva of *Forcipomyia piceus* Winn. (*Ann. Appl. Biol.*, **10**, 409–41). The pupa is also described. In the larva of this insect the malpighian tubules open into the posterior end of the mid gut; the salivary glands discharge their secretion by means of intracellular ducts; stigmata are absent, the tracheal system being entirely closed, and the author is of the opinion that gaseous exchanges probably take place through the thin chitin of the body wall, the body fluid acting as an oxygen carrier. A further paper by this author deals with the morphology of the head capsule and mouth-parts in *Chlorops tæniopus* Meigen (*Jour. Linn. Soc. Lond. Zool.*, **35**, 400–10). Certain observations in this paper are in disagreement with those of Peterson. The correlation between food and longevity and reproduction in flies is shown by R. W. Glaser, who experimented with *Musca domestica*, *Stomoxys calcitrans*, and *Lyperosia irritans* (*Jour. Exp. Zool.*, **38**, 383–412). Some interesting new observations on *Hypoderma lineatum* are given by S. Hadwen and J. S. Fulton. These authors show that once the larvæ enter the integument of the animal there is a rapid migration to the œsophagus, first stage larvæ having been found in that region and the course of the migration observed. Young stock are attacked and older animals become immune to warble larvæ attack (*Parasitology*, **16**, 98–106). R. G. Harris has investigated the factors which result in the production of the two types of *Miastor* larvæ, namely, the pædogenetic larvæ and those which pupate (*Proc. Nat. Acad. Sciences, Washington*, **9**, 407). From preliminary experiments the author concludes that light and temperature are not the important factors. The conditions which induce the appearance of pupa-larvæ are those resulting from overcrowding. What appears to be a case of accidental parasitism is recorded by E. Hindle (*Parasitology*, **16**, 111) who found a living Tabanid larva in a sac-like elongation of the right lung in *Bufo regularis*. The presumed

case of pædogenesis in *Calliphora erythrocephala* recorded by Parker is shown by D. Keilin to be incorrect, Parker's results being due to faulty experiments (*Parasitology*, **16**, 239–47). Two papers by D. Keilin (*Parasitology*, **16**, 113–26, 150–9) give an account of the life-history and structure of the larvæ of *Neottiophilum præustum* Meigen, *Anthomyia procellaris* Rond., and *A. pluvialis* L., which inhabit the nests of birds. In the case of the first species the larvæ of which live as intermittent blood-sucking parasites upon birds, the early larval stages are described for the first time. The larvæ of the second species is saprophagous, and only lives in the nests of birds, thus exhibiting a saprophagous specificity. J. R. Malloch gives some useful notes on some species of Australian Diptera, which are dealt with under four families, Muscaridæ, Drosophilidæ, Chloropidæ, and Agromyzidæ (*Proc. Linn. N.S. Wales Soc.*, **49**, 601–22). A very full account of the biology and anatomy of *Forcipomyia* is given by L. G. Saunders (*Parasitology*, **16**, 164–212). The interesting observation is made that the malpighian tubules may be either two or three in number, the latter condition being unique in insects. Irwin Smith (*Proc. Linn. Soc. N.S. Wales*, **48**, 49–81) has a paper on the respiratory system of the larva of the Stratiomyid *Metoponia rubriceps*. An important paper entitled " Dipteres Anthomyides " by E. Séguy has appeared in the series (*Faune de France*, XI, 393 pp., 813 text-figs.). An account of the biology of *Trichopoda pennipes* Fab., a tachinid parasite of *Anasa tristis* D.G., is given by H. N. Worthley (*Psyche*, **31**, 7–16). Collections of squash-bugs made showed a parasitism of 80 per cent. in nature. The interesting Psychodid *Telmatoscopus meridionalis* Eaton, which frequents particularly the African shores of the Mediterranean, has been found in great numbers in Sardinia (Cagliari) and an account of its morphology and ætiology is given by Ed. Zavattari (*Redia*, **15**, 191–231). Fertilised eggs are deposited on water, and the larvæ live near the surface on the decomposing vegetable matter. There appear to be at least three generations in the year. In the female the contents of the spermathecæ are to a large extent used up as material nutrition.

Hymenoptera.—A description of Termites from the islands of Simalur and Pulu Babi (Sumatra) by Masomitsu Oshima includes 8 new species (*Capita Zoologica*, **2**, Afl. 3), and H. F. Dietz has collected the records regarding the biology of the termites of the Panama canal zone (*Jour. Agric. Res.*, **26**, 280–302). O. E. Plath discusses the theories put forward regarding the so-called " trumpeters " in humble-bee colonies and concludes that their true function is that of ventilating the nest (*Psyche*, **30**, 146–54). F. Picard gives a detailed account of the anatomy and biology of the interesting species *Melittobia*

acasta Walk., *Bull. Biol. France et Belgique*, **57**, 469–507. The Ichneumonid *Limnerium* (*Eulimneria*) *crassifemur Thoms.* is recorded by A. Paillot from *Neurotoma nemoralis* L., being the first record of a hymenopterous host of this parasite (*C. R. Soc. Biol.*, **89**, 1045–48). The biology of *Thrinax mixta* Kl. and *T. macula* Kl. (*Tenthredinoidea*) recorded from Durham, and the features which differentiate the two types of larvæ are given by A. D. Peacock (*Proc. Univ. Durham Philosoph. Soc.*, **6**, 365–74). The ant genera *Myopias* and *Acanthoponera* are revised by W. M. Wheeler (*Pscyhe*, **30**, 175–92). A key to proposed sub-genera of the genus *Andrena*, together with description of several new species, is the subject of a paper by H. L. Viereck (*Can. Entom.*, **56**, 19, 28).

Hemiptera.—J. C. Chamberlin has an important monograph on the Tachardiinæ, or lac insects, some members of which produce lac or shellac of commerce. Sub-family rank is given to the group, although the author considers it should more properly represent a family of the super-family Coccidoidea. Forty-four species are redescribed and figured. With a view to putting the systematic study on a morphological basis, the morphological characters of the adult females have been specially noted. The group is divided into four genera: *Tachardia* Blanch., *Tachardiella* Ckl., *Tachardina* Ckl., and *Austrotachardia* gen. nov. The three former genera are subdivided each into two sub-genera (*Bull. Entom. Res.*, **14**, 147–213). Another paper on lac insects by S. Mahdihassan aims at classifying these insects from a physiological standpoint. This author differentiates between true lac insects for which the genus *Lakshadia* is erected, and pseudo-lac insects which are grouped under the old generic name *Tachardia*. The biological notes are interesting, but due regard does not appear to have been paid to the rules of nomenclature (*Maharajah's College, Vizianagaram, S. India, Journ. Sci. Assoc.*, 1923, 47–99). Interesting observations on the relationship of leaf-hoppers to certain plant associations are given by D. M. De Long (*Ann. Ent. Soc. Amer.*, **16**, 363–71). Ten new species of Coccidæ are described by E. E. Green and F. Laing (*Bull. Entom. Res.*, **14**, 123–31). A description of all stages in the life-history of *Ericerus pela* Chav., the coccid which forms Chinese white-wax, is given by Inokichi Kuwana (*Philippine Journ. Sci.*, **22**, 393–405). There is an interesting note by S. Marcovitch (*Science*, **58**, 537) on the influence of "length of day" on the appearance of winged migrants and sexual forms in aphids and F. M. Wadley (*Ann. Ent. Soc. Amer.*, **16**, 277–303) discusses the influence of parentage, temperature, and nutrition on the alate and apterous condition in the same family. Ryoichi Takahashi (*Dept. Agric. Govt. Res. Instit. Formosa, Rept.*, **4**, 173 pp.), has published

Part 2 of the Aphididæ of Formosa, which includes descriptions of 12 new species. New Aphididæ from Egypt by F. V. Theobald (*Bull. Soc. Roy. Entom. d'Egypte*, 1922) includes 14 species new to science. H. Osborn and F. H. Lathrop have reviewed the North American species of the genus *Phlepsius* (*Ann. Entom. Soc. America*, **16**, 310–350). G. Teodero describes an organ on the elytra of Heteroptera which is evidently associated with the support of the wings during flight (*Redia*, **15**, 79–95). A detailed study of the mouth parts, salivary pump, and salivary glands in *Cimex lectularius* is given by J. M. Puri (*Parasitology*, **16**, 84–97). The systematist will find the critical observations of L. B. Woodruff (*Jour. N.Y. Entom. Soc.*, **32**, 1–62) on the Membracid genus *Cyrtolobus* of interest. Part 4 of the Insects of Connecticut deals with the Hemiptera (*Connecticut Geol. Nat. Hist. Survey Bull.*, **34**, 805 pp.); 45 families and 1,646 species are dealt with.

Other Orders.—K. Friederichs (*Capita Zoologica*, **2**, Afl. 1) has some observations on the Embiidæ, with interesting notes on their habitat. *Ptilocerembia roepkei* gen. et sp. nov. from Java is described. With a view to reorganising the Mallophagan family Menoponidæ and establishing the relationship of these insects to their hosts, G. F. Ferris in a preliminary paper deals with the types of five genera (*Parasitology*, **16**, 55–66). The systematist will find the notes on the Apterygota by J. R. Dennis (*Ann. Soc. Entom. France*, **92**, 209–46) of interest. P. Esben-Petersen in Parts 4 and 5 continues the studies of the Australian Neuroptera (*Proc. Linn. Soc. N.S. Wales*, **48**, 576–600).

ARTICLES

ANCIENT EGYPTIAN MATHEMATICS

By WARREN R. DAWSON.

The "wisdom of the Egyptians" is proverbial, and in the extensive remains of their great civilisation which time and vandalism have spared is sufficient proof of their advanced knowledge and abilities. The large and varied literature which has come down to us on papyri and ostraca written in the cursive script known as *hieratic* discloses the Egyptian to us as a man capable at once of great achievements and yet subject to considerable limitations. Whilst we have a great abundance of literary texts—stories, poems, and the like—of philosophical and didactic works, of legal and business documents, we have comparatively little in the way of scientific works. A group of medical papyri in which magic predominates over an undoubted modicum of real medical knowledge has recently been supplemented by the *Edwin Smith Papyrus*, which is a practical treatise on surgery applied to wounds. Of mathematical works we have still less. Our reconstruction of Egyptian mathematical knowledge is necessarily centred on the *Rhind Mathematical Papyrus* and a number of lesser fragments on associated subjects. The Rhind Papyrus was edited half a century ago in German by Prof. Eisenlohr, but this edition is now completely out of date, for not only has our knowledge of the language, and especially the highly technical phraseology employed in a scientific work, increased by leaps and bounds, but additional material has come to light which must necessarily be brought into focus.

In 1898 the authorities of the British Museum published a fine folio facsimile of the papyrus, but, as this was unaccompanied by a translation, the document has long been before our eyes tantalisingly inaccessible to all save the few who can read the hieratic script. Our difficulties are now, however, at an end, for an admirable edition of the papyrus has just appeared, containing not only the Egyptian text, but a full translation and commentary, and a valuable introduction on

Egyptian mathematics generally.[1] From this edition it will be interesting to glance briefly over some of the fundamental notions of Egyptian mathematics as understood in the second millennium before Christ.

On the general character of Egyptian mathematics we cannot do better than quote Prof. Peet's opening statement (p. 10):

"The outstanding feature of Egyptian mathematics is its intensely practical character. This is not peculiar to mathematics, for it is typical of all the sciences in Egypt. As Plato alone of the Greeks seems to have realised (*Republic*, iv, 436), the Egyptians were essentially 'a nation of shop-keepers,' and interest in or speculation concerning a subject for its own sake was totally foreign to their minds."

The papyrus itself affords ample evidence of this, for everything is expressed in concrete terms. The Egyptian does not think of 8 as an abstract number, he thinks of 8 loaves, or 8 sheep. Similarly, he enters into calculations on the angle of slope in a pyramid in order to direct his masons how to cut the blocks, not because he is interested in angles or slopes. There is an almost total absence of general principles or formulæ; each problem has to be worked out on its merits. Thus Egyptian mathematics stagnated. No progress was made once a system had been devised to meet the ordinary requirements of daily life—to measure a field, to distribute commodities, etc.

Egyptian notation is a decimal system. The units are written by a stroke |, the tens by ∩, hundreds, thousands, tens of thousands, hundreds of thousands each having its appropriate sign, which cannot be here reproduced, as hieroglyphic type is necessary. Thus 3 is written |||, nine is |||
|||
|||, for 13 we have the notation ∩|||, for 56 ∩∩∩|||
∩∩||| and so on. It will easily be seen that high numbers require the use of a great many ciphers, so that, in the cursive or hieratic script, graphic abbreviations were used. There is evidence that this decimal system was originally quinary and based on finger-counting, but as early as the 1st dynasty (*circa* 3400 B.C.) the decimal notation up to 1,000,000 has been found. The evidence for the quinary method is extremely interesting, but is an excursus into which we cannot wander now.

[1] *The Rhind Mathematical Papyrus—British Museum, 10057 and 10058.* Introduction, Transcription, Translation, and Commentary by T. Eric Peet, Brunner Professor of Egyptology in the University of Liverpool, etc. (Liverpool University Press and Hodder & Stoughton, Ld. 1923. Folio.)

When we come to review the simple arithmetical processes, we have to reckon with certain basic facts, which, although known to all of us, we scarcely ever pause to consider. We will quote Prof. Peet's remarks again (p. 11):

"There is only one truly fundamental process in arithmetic, that of counting. What we are accustomed to call the four simple rules are not fundamental, but are results of counting committed to memory. Thus, when I say 8 and 7 make 15 I am not performing a basic process, I am merely repeating a fact which I know from memory, and the child who says 8 and 7 are 14 is not making an error of calculation, but merely one of memory. If I wish to prove that 8 and 7 really do make 15, I must count out 8 objects, then 7 more. I must then count both lots together and I shall get 15."

The same holds good of subtraction, and multiplication and division are acts of memory too. Ability to make rapid calculations is therefore dependent largely on memory equipment, and the individual who carries all the multiplication tables in his head from 2 *times* to 19 *times* is obviously better equipped than one whose memory does not extend beyond 12 *times* 12. The Egyptian had a great advantage in addition, as the nature of the decimal notation enabled him to dispense with a great deal of memory-work, for he had actually to count his ciphers as he wrote them, except that memory had to be employed when writing the hieratic abbreviations for groups of ciphers. Thus 7 written by hieroglyphic notation is $\overset{||||}{|||}$, but in hieratic an arbitrary ligature very like our figure 7 was used. Similarly 8 is contracted in hieratic from $\overset{||||}{||||}$ to $=$, and so on.

Addition was often expressed by a preposition meaning "in addition to." Thus we have "a number whose fourth part is added to it." Sometimes the verb "put" is also employed: "You are to put 100 in addition to it," i.e. "add 100 to it." Subtraction is expressed by a verb the primitive meaning of which is "to break."

With regard to multiplication, the Egyptian never multiplied, by sheer act of memory, by any numbers except 2 and 10. Here again in the case of multiplication by 10 it was simply a case of altering the decimal notation, *i.e.* turning unit-signs into ten-signs and so on. By reversing the process, division by 10 was similarly accomplished. Multiplication by 2 was, however, a simple process of memory. To multiply by a higher number than 10 was a question of manipulating the powers of 2 and 10. Thus 12 *times* a number was obtained by adding

2 times to 10 *times*. To multiply 15 by 13 the following process was used :

$$
\begin{array}{rl}
 & /1 \times 15 = 15 \\
 & 2 \times 15 = 30 \\
 & /4 \times 15 = 60 \\
 & /8 \times 15 = 120 \\
\text{Total} & 13 \times 15 = 195.
\end{array}
$$

Here the Egyptian merely kept on doubling. The multipliers 1, 4, and 8 add up to 13, and consequently their products must amount to 13 times 15. These numbers were ticked off, and they and their products totalled at the foot of the sum. Division was a simple reversing of the same process; thus $77 \div 7$ was done as follows :

$$
\begin{array}{rl}
 & /1 \times 7 = 7 \\
 & /2 \times 7 = 14 \\
 & 4 \times 7 = 28 \\
 & /8 \times 7 = 56 \\
\text{Total} & 11 \times 7 = 77.
\end{array}
$$

In this case the products of the ticked numbers 7, 14, and 56 add up to 77, and the corresponding numbers 1, 2, and 8 make 11, which is the answer. The processes of multiplication and division were expressed by a compound word which means literally to " incline the head," and, as Prof. Peet suggests with great probability, " nodding the head " may have been a primitive operation in counting, and so perhaps acquired the meaning " to count," the head being nodded to mark off every five or ten counted off on the fingers. Thus " count with 4, 5 times " means " multiply 4 by 5." Division was also expressed with the addition of the verb " to find." Thus " divide 77 by 7 " would be expressed " count with 7 to find 77." The result of multiplication or division was usually expressed by the verb " to become." Thus 4×5, " it becomes 20."

Fractions were extensively used. We find their practical application in many account papyri which have survived and in specifying the quantities of drugs used in the prescriptions of the medical papyri. The fractional system used by the Egyptians, however, was a simple one, because with the single exception $\frac{2}{3}$ no fraction was ever employed with a numerator greater than unity, and consequently all Egyptian fractions were aliquot parts. For writing fractions it may be mentioned that a special notation was employed. The fractions $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, and $\frac{2}{3}$ had special hieratic signs, but all other fractions were expressed by writing the numeral underneath a sign which in

hieroglyphic characters is the *mouth.* This sign was replaced in the hieratic script by a dot. Thus $\frac{1}{5}$ was written $\overset{\bullet}{\substack{\text{III}\\ \text{II}}}$, or $\frac{1}{13}$, $\overset{\bullet}{\cap\text{III}}$ (the denominator likewise having its hieratic equivalent). The idea of fractional expression on philological grounds is a series of " breakings " or " halvings." We thus have frequently in the medical papyri $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$, $\frac{1}{32}$, $\frac{1}{64}$, etc. But side by side with these dimidiated fractions there was another series which evidently had its origin in dividing into *thirds.* This perhaps will explain the unique position occupied by the fraction $\frac{2}{3}$ in Egyptian mathematics. The special sign for $\frac{2}{3}$ conceived a part which we may call a " two-thirds." It is clear that this conception was a fundamental one, for one-third of a number was invariably found by first obtaining two-thirds and then halving it. Although the Egyptians had no notation for fractions with a numerator greater than 1 (with the single exception of $\frac{2}{3}$) there is abundant evidence that such fractions were conceived. For this Prof. Peet adduces clear evidence on p. 16 of his book. Speaking of fractions generally (p. 17) he says :

" In order to appreciate the powers and limitations of the Egyptian mathematician in dealing with fractions, it is essential to bear this notation in mind throughout. What enabled him to keep such a simple apparatus undeveloped throughout ages was the fact that he never learned to multiply directly by any other number than 2. The result was that in his processes he was rarely likely to be threatened with worse fractions than doubled aliquot parts, *e.g.* " twice the thirteenth part." He had discovered that all such quantities could be resolved into the sum of two or more aliquot parts, and had actually worked out tables for such resolutions, running from twice a fifth-part to twice a 101st-part. This simple apparatus, a copy of which begins our Rhind Papyrus, saved him the trouble of evolving a more complicated fractional notation."

Before proceeding to consider the contents of the Rhind papyrus, it may be mentioned that the principles of the Square and Square Root were known to the Egyptians, as examples in the Moscow, Berlin, and Kahun papyri show. Solution of equations by a simple method of trial is exemplified in the Rhind Papyrus, and likewise arithmetical and geometrical progressions. With regard to Geometry the same severely practical character of its application is manifest. The areas of squares, oblongs, and circles are estimated, and the volumes of cubic figures worked out.

For setting out sums a regular set of technical phrases and

terms are employed. Each sum consists of a title, statement of the problem, the working out, and the proof. A long section of the introduction is devoted by Prof. Peet to the philological and methodical aspects of the problems (pp. 21 ff.). Egyptian weights and measures, of length, area, and capacity are likewise discussed. The metric system is a study in itself, and must not detain us now.

We must now briefly consider the actual contents of the Rhind Papyrus itself. Like almost all Egyptian books it opens with a grandiloquent title, followed by the date in the reign of the king under which it was drawn up, and the name of the scribe who copied it. The title is translated by Prof. Peet as follows (p. 33):

"Rules for inquiring into nature, and for knowing all that exists, [every] mystery, . . . every secret."

A considerable space is then devoted to a table of each of the various fractions whose numerator is 2 and whose denominator is one of the odd numbers from 3 to 101 as the sum of two or more aliquot parts, *i.e.* fractions whose numerator is unity. Thus:

$$\tfrac{2}{19} = \tfrac{1}{12} + \tfrac{1}{76} + \tfrac{1}{114}.$$

As mentioned above, the Egyptian did not use any fractions other than aliquot parts (except $\tfrac{2}{3}$) and in order to deal with fractions whose numerator was 2 he drew up a table for "the division of 2," *i.e.* a table for breaking up such fractions into aliquot parts.

"By what process did the Egyptian arrive at his results? This may best be understood by examining a typical specimen of a resolution, namely, that of $\tfrac{2}{7}$, which may be paraphrased in modern terms as follows:

"*Problem*: express $2 \div 7$ as the sum of a series of aliquot parts.

"*Answer*: $1\tfrac{1}{2} + \tfrac{1}{4} = \tfrac{1}{4}$th of 7; $\tfrac{1}{4} = \tfrac{1}{28}$th of 7. *i.e.* $\tfrac{2}{7} = \tfrac{1}{4} + \tfrac{1}{28}$.

"*Proof*:

	1	7
	$\tfrac{1}{2}$	$3\tfrac{1}{2}$
	/$\tfrac{1}{4}$	$1\tfrac{1}{2} + \tfrac{1}{4}$
/4	$\tfrac{1}{28}$	$\tfrac{1}{4}$

(Proof of this step:)

1	7
2	14
/4	28.

"There is no doubt as to what takes place here. The 2 is broken up into two parts, namely $(1\tfrac{1}{2} + \tfrac{1}{4})$ and $\tfrac{1}{4}$. The first

of these is then shown to be $\frac{1}{4}$ of 7 by the simple process of dividing 7 by 2 and then by 2 again, while the second is shown to be $\frac{1}{28}$ of 7 by multiplying 7 by 4 and obtaining 28. Thus $2 \div 7 = \frac{1}{4} + \frac{1}{28}$.

"As a proof this is satisfactory, but it does not throw the slightest light on the one feature of interest in these problems, namely, the manner in which the Egyptian obtained his answer, which, be it noted, is not worked out at all, but merely assumed and then proved. To arrive at the method by which the answer was obtained it is necessary to examine the whole series of resolutions from $\frac{2}{5}$ to $\frac{2}{101}$, and to try to discern in them any signs of the employment of a general formula." (Peet, *op. cit.*, p. 34.)

This problem is tackled at length by Prof. Peet, who finds the solution and gives a table of the whole series from $\frac{2}{5}$ to $\frac{2}{101}$. The Egyptian naturally omitted any treatment of $\frac{2}{3}$, as this fraction stood quite alone, as previously intimated.

After these lengthy calculations, there follows a series of sums showing the division amongst 10 men of various numbers of loaves, *viz.* 1, 2, 6, 7, 8, and 9. The method of all these sums is the same. The first line states the problem, the second gives the answer as usual without any working out, and the remaining lines show the working of the proof. Thus No. 1 reads as follows (p. 51):

"Example of dividing 1 loaf among 10 men.
"You are to multiply $\frac{1}{10}$ by 10.
"The doing as it occurs:

/2	$\frac{1}{5}$
4	$\frac{1}{3} + \frac{1}{15}$
/8	$\frac{2}{3} + \frac{1}{10} + \frac{1}{30}$.

"Total 1. This is the number in question."

No. 3.

"To divide 6 loaves among 10 men.
"You are to multiply $\frac{1}{2} + \frac{1}{10}$ by 10.
"The doing as it occurs:

1	$\frac{1}{2} + \frac{1}{10}$
/2	$1\frac{1}{5}$
4	$2\frac{1}{3} + \frac{1}{15}$
/8	$4\frac{2}{3} + \frac{1}{10} + \frac{1}{30}$.

"Total 6. This is the number in question."

In the multiplication by four $\frac{2}{5}$ is broken up into $\frac{1}{3} + \frac{1}{15}$ by the table at the beginning of the papyrus, and in the next line $\frac{2}{15}$ is resolved into $\frac{1}{10} + \frac{1}{30}$.

We may explain the working of the above two examples in the same manner as in the cases of simple multiplication and division stated earlier in this paper. In No. 1 the ticked numbers 2 and 8 add up to 10, and their fractions $\frac{1}{5} + \frac{2}{3} + \frac{1}{10} + \frac{1}{30} = \frac{30}{30} = 1$, and in No. 3, $1\frac{1}{5} + 4\frac{2}{3} + \frac{1}{10} + \frac{1}{30} = 5\frac{6+20+3+1}{30} = 5\frac{30}{30} = 6$.

Following upon these divisions of loaves, the papyrus proceeds to two sets of calculations called "completions." It is difficult to see what practical purpose these calculations can have had. Prof. Peet has taken a typical specimen (No. 13) which he sets out in modern notation as follows :

$$\begin{aligned} a &= \tfrac{1}{16} = \tfrac{1}{112} \\ \tfrac{1}{2}a &= \tfrac{1}{32} = \tfrac{1}{224} \\ \tfrac{1}{4}a &= \tfrac{1}{64} = \tfrac{1}{448} \\ \text{Total } a(1 + \tfrac{1}{2} + \tfrac{1}{4}) &= \tfrac{1}{8}. \end{aligned}$$

The sums may, as Prof. Peet suggests, show us some part of the experimental working which is taken for granted in so many of the problems. We have here, not definite problems to solve, but the discovery by trial of useful facts for future use. The second set of "completions" is quite different from the first and involves the solution of definite problems. Thus we have "What completes $\frac{2}{3} + \frac{1}{15}$ to 1 ?" and the answer is $\frac{1}{5} + \frac{1}{15}$, or as we should say $\frac{4}{15}$; so that the problem is simply "subtract $\frac{2}{3} + \frac{1}{15}$ from 1, expressing the answer in aliquot parts."

The method of working involves the principle of the common denominator, which in this case is 15, the values of the fractions in fifteenths being written in red below them. All that is then required is to express the result in aliquot parts, the proof being supplied again with the common denominator of the fractions.

No. 21 (*op. cit.*, p. 58): [the underlined figures are written in red in the papyrus].

" It is said to you ' what completes $\underset{\underline{10}}{\frac{2}{3}} + \underset{\underline{1}}{\frac{1}{15}}$ into 1 ? ' Total 11. Remainder 4.

" Reckon with 15 to find 4.

1	15
$\frac{1}{10}$	$1\frac{1}{2}$
/ $\frac{1}{5}$	3
$\frac{1}{15}$	1

" Total 4 : then $\frac{1}{5} + \frac{1}{15}$ is what must be added to it.

" Therefore $\underset{\underline{10}}{\frac{2}{3}} + \underset{\underline{3}}{\frac{1}{5}} + \underset{\underline{1}}{\frac{1}{15}} + \underset{\underline{1}}{\frac{1}{15}}$ is complete up to 1."

We now come to a long series of calculations which are the solutions by trial of equations of the first degree. One

of the problems is headed " a quantity whose seventh part is added to it becomes 19." (No. 24, *op. cit.*, p. 61.) This equation, expressed in modern terms, is: "If $x + \frac{1}{7}x = 19$, find x." This sum is worked out by the cumbrous Egyptian method which is summarised as follows (*op. cit.*, p. 61):

" Step 1. The trial number 7 is set down, and one-seventh is added to it, giving 8.

" Step 2. The 8 is operated on in the usual fashion to produce 19, or, as we put it, 19 is divided by 8. The result as shown by the ticks is $2 + \frac{1}{4} + \frac{1}{8}$.

" Step 3. This last quantity is multiplied by 7, giving $16\frac{1}{2} + \frac{1}{8}$.

" Proof: One-seventh of this quantity is taken and added to it, the result being the required 19. The working of this proof is omitted."

Two problems follow which deal with division of loaves in unequal proportions, and then we pass on to a different series of problems involving mensuration and beginning with volumes and cubic capacity (Nos. 41–7). We have a number of circular containers, the height and diameter of which are given, and the answer is worked out in capacity in *ḥekat*, or bushels. The results on the whole are fair approximations, but not accurate calculations. No. 44 deals with the volume of a rectangular container, and No. 45 is to find reverse, *i.e.* given the quantity of bushels, find the dimensions of the container.

A considerable collection of problems (Nos. 48–55) deals with calculation of areas, and involves the use of the Egyptian land-measures. We have pieces of land specified in various shapes, circular, triangular, etc. Some of the problems are accompanied by linear figures, after the manner of Euclid.

Problems Nos. 56–60, likewise accompanied by figures, are to find the angle of slope, or " batter " of the sides of pyramids, a matter of great practical importance in the Middle Kingdom, at the time the book was composed, as pyramids were then still in fashion.

All the above groups of problems can be classified, but the next series, Nos. 61–84, deal with a great variety of miscellaneous matters. Thus we have proportionate values of precious metals, division of barley into shares in arithmetical progression, reckoning of live-stock, barter values of grain, bread, and beer, geometrical progression, food estimates of poultry and cattle, etc.

With these problems we reach the end of the mathematical book. A short free space of papyrus at the end has been used, as was so often the case, for miscellaneous jottings which have

no connection with the subject-matter of the roll. Here we have an unintelligible group of signs, perhaps a puzzle, a fragment of accounts, and some calendrical jottings from which very hazardous chronological conclusions have been drawn. These are written on another scrap of papyrus which has been pasted on to the Rhind Papyrus as a patch to cover a tear.

The above paragraphs will, it is hoped, serve to point out some of the interesting features of the Rhind Papyrus. To do justice to Prof. Peet's publication far more space would be required. The whole undertaking is a model of how a scientific publication should be produced. The subject-matter is not subordinated to philology, as is often the case in editions of Egyptian texts. The Egyptologist has the palæographical, philological, and archæological matter to interest him, the mathematician has a book which is entirely comprehensible to him without the necessity of knowing a single word of Egyptian, and, to the student of the development of human thought, papyrus is full of appeal, for it raises many pertinent questions as to the relation of language to thought and of language to writing. From the historian's point of view the chapters on the comparison of Egyptian with Babylonian mathematics and on the Greek views on Egyptian mathematics are both enlightening and important.

Egyptology should not be confined to Egypt, it touches the whole development of human thought and action.

PROBLEMS OF SATURN'S RINGS.

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Part I

No other planet has received anything like the attention that has been bestowed by astronomers on Saturn. It would also be correct to say that no other planet has so amply repaid that attention. A rich harvest has been gathered which more than justifies the labour spent in cultivating the field.

Why Saturn has been so favoured with attention will be apparent from a glance at the drawing (Fig. 1). This shows the appearance of Saturn in a powerful telescope. The remarkable ring-system that surrounds the planet is, so far as we know, a feature that has no parallel elsewhere in the universe.

It is not surprising, therefore, that most of the work of astronomers in connection with Saturn has been directed towards the elucidation of the problem—What is the nature of Saturn's ring? The last word has not been said even yet, and at the moment of writing a theory has been put forward which threatens to upset all our previous notions respecting the composition of this curious formation.

The systematic study of Saturn really began when Galileo turned the newly-invented telescope on the planet about the year 1610. His telescope was a very small and imperfect affair, magnifying only thirty diameters, and his view of Saturn through it merely served to mystify him.

What he saw was this. Instead of a simple round body, he saw a central globe accompanied by two smaller globes on each side of it. And, instead of publishing his discovery in plain terms and simple language, Galileo resorted to a dodge which was very common in those days, which was done in this way:

When a person was not sure of a discovery, and yet wanted to be the first to publish it, he generally wrote down the details, and then jumbled the letters up, so that it would be impossible for anyone to read it who had not the proper key.

This was what Galileo did. He wrote a letter announcing

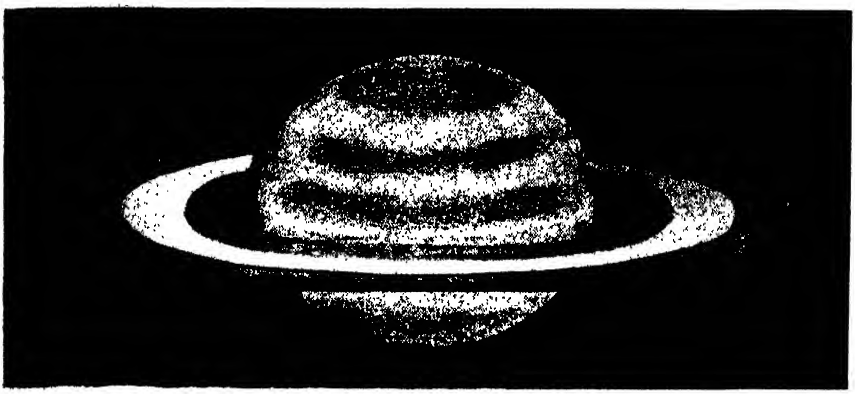


FIG. 1.—Saturn, showing the Crape Ring, Cassini's division, Encke's division, etc
Drawn by J. A. Lloyd.

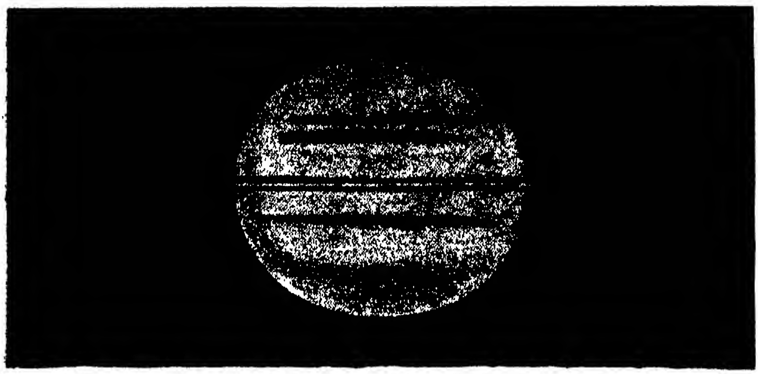


FIG. 3.—Saturn with edge of Ring presented to the earth.
From a drawing by J. A. Lloyd.

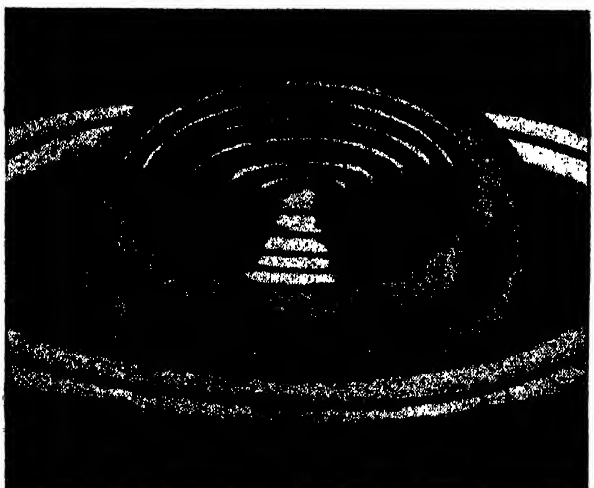


FIG. 6.—Picture diagram to explain the modern conception of the internal constitution of Saturn.
From a drawing by J. A. Lloyd.

his discovery to the astronomer Kepler, but which the latter failed to read, just as Galileo intended he should. Later, the sentence was found to mean "I have observed the planet Saturn to be triple."

As soon, however, as Galileo was assured of the correctness of his observations, he had no hesitation about announcing it without equivocation. "When I observe Saturn," he wrote, "the central star appears the largest; two others, one situated to the east, the other to the west, and on a line which does not coincide with the direction of the Zodiac, seem to touch it. They are like two servants who help old Saturn on his way, and always remain at his side. With a smaller telescope, the star appears lengthened, and of the form of an olive."

It is necessary, in order to understand what follows, to say that the ring of Saturn is not always presented to our view at the same angle. Its appearance varies from time to time. This happens because the plane of the ring does not coincide with the plane of the Earth's orbit. So that, as the Earth and Saturn both travel round the Sun, we find ourselves alternately above and below the ring in space, while it periodically happens that the ring disappears completely, because we can only see its extremely thin edge at those times.

Unfortunately, this condition was fast approaching when Galileo began to observe, but as he knew nothing of it he was very much puzzled to account for what he saw taking place. The two attendants, as he called them, gradually dwindled down, becoming smaller and smaller, till at last they became invisible altogether to him. He was very perplexed and disappointed. He doubted the evidence of his eyes, and blamed his telescope for having deceived him. He was so shaken by the occurrence that, strangely enough, instead of following up his observations and trying to fathom the mystery, as we should have expected him to do, he gave Saturn a wide berth for the remainder of his life.

Half a century went by, and then the murder was out. Christian Huyghens, a Dutch scientist, had turned his attention to making glasses for telescopes, and, after a while, he succeeded in constructing a very good instrument, far more powerful than that employed by Galileo.

Huyghens took up the problem of Saturn, and at last was able to say that the appearances which had so puzzled Galileo and the earlier observers were caused by a thin flat ring surrounding the planet, which we saw from different directions, depending on the relative positions of Saturn and the Earth. Consequently, when Saturn "swallowed his children," as Galileo had declared in one of his letters, what happened was that we were looking at the thin edge of the ring instead of

viewing it full on, as it were. Then the ring disappeared from view because of its extreme thinness and the lack of power of the telescope.

Huyghens published his important discovery in the form of a cipher, as Galileo had previously done. But in 1659 he felt sufficiently sure of himself to announce to the world that "Saturn is surrounded by a thin and flat ring, not touching the planet at any point, and inclined to the ecliptic." Fig. 2 shows how Galileo saw Saturn with his baby telescope; and in the lower portion, the improved view obtained by Huyghens with his superior instrument.

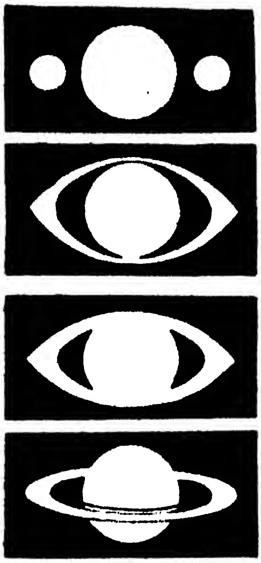


FIG. 2.—Saturn as seen by Galileo and (bottom) Huyghens.

From a diagram by J. A. Lloyd.

Next a man named Cassini, an Italian who lived at Paris, began to work at the problem; and in 1675 he was rewarded by an important discovery. For he found that the ring was really double, being divided by a dark gap, which is now called "Cassini's Division." The feature is shown as a heavy black line running all round the ring in Fig. 1. However, as often happens, this division had been independently discovered by an Englishman named William Ball, who now shares the credit with the Italian.

Cassini also called attention to the fact that the two rings are not of equal brightness, and he compared the inner one to burnished silver, and the exterior to dull silver.

So matters stood till the time of Sir William Herschel. Sir William refused at first to believe that Cassini's division was a veritable gap in the ring. He insisted that it was a mere shading on the surface. He subjected it to a long course of careful scrutiny, but failed to detect any changes in it.

But when, in 1790, he got a chance to see the other face of the rings, and perceived that what he called the "broad black mark" was visible on that side as well, he was forced to admit that the Cassini division was a true gap in the ring.

Coming nearer to our own time, the next discovery with regard to the rings was made in 1837 by Encke, who noticed that another division existed in the outer ring. But this was a very fine and delicate line, visible only in the largest instruments. It was situated rather more than half-way between the Cassini Division and the outer edge of the ring (see Fig. 1).

On the inner portions of the rings discoveries were also made

during this period. Galle in 1838 called attention to a gradual shading off of the inner ring towards the planet, and then in 1850 this was proved to be still another ring, far less luminous than the others. It is now known as the "Crape Ring," or gauze ring, because it is partly transparent, allowing us to see the body of the planet through it (Fig. 1).

Now, what about the rings when they are presented edge on to us? Well, in 1861 the rings were seen edgewise, and an observation by a man named Wray was made, which is of considerable interest. "A prolongation of very faint light stretched on either side of the dark shade of the ball, overlapping the fine line of light formed by the edge of the ring to the extent of about one-third of its length, and so as to give the impression that it was the dark ring and seen edgewise, projected on the sky." The drawing (Fig. 3) will serve to explain the foregoing remarks.

We are able to gather from the above observation that the rings are really most remarkably thin. Estimates of their thickness vary, but if we provisionally adopt about fifty miles, we shall probably not be very far out.

The next thing to be determined was whether the rings are circular or not. The earlier observers thought they were, but grave doubts were entertained by some later astronomers, chief of whom was perhaps the late Father Secchi of Rome. He spent considerable time in making careful measurements of the rings, and he decided that they were not circular but very slightly elliptical. Another interesting fact that was disclosed is, that the globe of Saturn is not in the exact centre of the rings, but just a little to one side of the centre. Here the mathematician steps in and asserts that such a condition of eccentricity is necessary to the stability of the ring-system, so we have here both theory and observation going hand in hand.

There was another curious fact came to light from the measurements made on the rings; it was this. They were gradually widening towards the planet, so that, if this extension were to continue, the rings would at last come to touch the surface of Saturn. What would happen then is a nice question; it is probable that the rings would go to pieces. Flammarion has even predicted the year 2150 as the date when this will happen. But it is premature to make such predictions, for the later measures by Barnard and others do not show the expected increase in width. It is quite likely that the very nature of the rings makes it possible for them to expand and contract as they are pulled this way and that by the gravitational influence of Saturn's moons.

We now approach the very heart of the mystery. What exactly are these wonderful rings? What is the material

from which they are fashioned? This is the problem that has been perplexing astronomers ever since they began to observe Saturn at all.

The earlier observers naturally thought they were solid flat sheets of the same material as the planet itself; and, until the question is examined carefully in the light of modern science, this seems reasonable enough.

It has, however, been definitely proved that the rings cannot possibly be solid, because no material that we can imagine would be able to stand the enormous strains and stresses to which they must be subjected from the attracting force of the planet. The late Mr. R. A. Proctor likened the ring to a vast arch or bridge across an abyss. "The strains and pressures upon the various parts of the system would exceed thousands of times those which even the strongest materials built into their shape could resist. The system would no more be able to resist such strains and pressures than an arch of iron spanning the Atlantic would be able to sustain its own weight against the attraction of the earth."

If the rings be assumed to rotate, as, indeed they actually do, the theory becomes even more hopeless. Our friends the mathematicians tell us that the pull of Saturn, combined with the centrifugal tendency of the rings themselves, would instantly burst them asunder. This effectually disposes of the solid theory.

How about rings composed of some liquid—water, for example? We find ourselves in rather worse case than before. The mathematicians again tell us that the waves which would be inevitably set up in the rings would immediately destroy them. Therefore, once more, they cannot be liquid.

What remains? The question was taken up by a brilliant young scientist, Clerk-Maxwell, in 1857. He proved beyond all reasonable doubt that the rings can only be composed of mighty swarms of separate lumps of matter, meteorites as we term them, each one revolving on its own path about the equator of Saturn, in periods of time which depend on their distances from Saturn's centre.

Thus was the mystery of Saturn's rings solved, not by observations with the telescope, but by calculations on paper.

Part II

There is no reason to doubt the validity of Clerk-Maxwell's deductions. On the contrary they have been confirmed in a number of different ways. One confirmation is obtained from measurements of the brilliancy of the rings under different

angles of tilting. The fact of the rings retaining the same surface brilliancy no matter how they are slanted cannot be explained in any other way than by supposing the rings to be composed of separate particles.

Another method of testing the question is a beautiful one devised by Prof. Keeler, using the instrument known as a spectroscope. The method is so ingenious that I feel tempted to explain how Prof. Keeler applied it.

If the rings were solid, it is clear that the outer edge would rotate faster than the inner edge. But if the rings are made up of separate particles, the inner edge would go the faster, because the nearer any body is to a controlling mass, the faster it must travel in order to preserve an orbit. Otherwise it would fall on to the attracting body.

Now in the spectroscope we have a means of testing this. By means of a prism the light from any luminous body can be broken up into its constituent parts, being spread out into a multi-coloured ribbon called the spectrum. By noting the position of certain lines that cross the spectrum we can tell of what elements the luminous body is composed. And these lines have a fixed position in the spectrum provided the source of light is at rest.

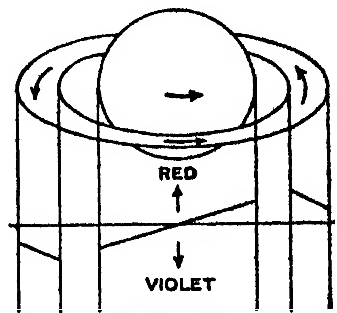


FIG 4 —Professor Keeler's Method of testing the rotation of Saturn's Rings

From a diagram by J. A. Lloyd.

Now suppose the luminous body begins to move towards us. Then all the lines in the spectrum undergo a shift of position towards the violet end of the spectrum. If the body is moving away from us, the shift is in the opposite direction or towards the red end. The amount of displacement of the lines depends on the speed at which the luminous body is travelling.

This, then, gives us a means of testing the rotation-periods of the various parts of Saturn's ring. Fig. 4 explains how the result is obtained. By placing the slit of the spectroscope across the ball of Saturn, we obtain three different spectra, the broad middle one due to Saturn itself, and the two narrow ones to the parts of the ring on each side of the planet. If we take the straight central line to represent any particular line common to all three spectra, it would represent that line when Saturn was at rest.

But Saturn is not at rest. It is turning on its axis in the direction shown by the arrows, and consequently the line is

broken and tilted as shown by the short segments. The line on the left is tilted with its inner end towards the violet, showing clearly that side of the ring is coming towards us and that its inner edge is moving faster than its outer.

On the other side we have the reverse effect, showing that side to be moving away from us. The amount by which the short lines are displaced at any point of their length from the normal line will give us the speed in miles per second of the corresponding portion of the ring. In this way the rotation-period of Saturn has been determined; and it proved beyond doubt that the ring was composed of independent units.

But how did such a ring come into existence at all? There have been several explanations, some of them more or less plausible. A mathematician named Roche proved in 1848 that no solid body could exist near Saturn if its distance from the planet were 2·38 radii of the latter, because of the gravitational effect of the planet. It would be quickly disrupted. Sir George Darwin suggested that the rings are due to the shattering of such a satellite by the gravitational influence of Saturn. At any rate the rings agree fairly well with the position which such a body would have occupied.

Turning now to the study of the gaps or divisions in the rings, perhaps the work of the late Prof. Percival Lowell ranks foremost amongst what has been accomplished in this direction. It has already been mentioned that two gaps are visible in the rings—Cassini's and Encke's. These gaps are proved to be due to the disturbing influence of Saturn's moons on the particles composing the rings. But Prof. Lowell and Dr. Slipher at Flagstaff Observatory, Arizona, discovered that there existed several other divisions in addition to the two already described.

Now, there is no question about the reality of these markings, although we are touching on very recent work here, because they were not only seen, but their positions in the rings were measured. Moreover, the measurements made at different times agreed with each other very closely indeed.

To explain these divisions, suppose we have one body revolving about another larger one. Then its path will be an ellipse, the larger body occupying one focus. The rate of speed of the lesser body, or the time it takes to complete a revolution, will depend on the mass of the greater attracting body and on the distance between the two.

But now, let us suppose another body to commence revolving about the same central mass. Each of these two secondary bodies, or satellites, will now interfere with the motion of the other, because every particle of matter in the universe is attracting every other particle, and this we call gravitation

If, in addition, there is a commensurability of period, or, say that one goes round in half the time taken by the other, or that one makes three revolutions while the other makes five, or any other simple relation like that, then these mutual perturbations will occur over and over again at the same points of their orbits, and the end of it will be that one or other of these bodies will be forced to abandon its original path and take up another where the periods will not be commensurable. Commensurability of period is always an unstable condition. The bodies will henceforth not overtake each other at the same point time after time, but always in a different position.

Saturn has ten moons revolving round it, and of these, two, which are nearest to the planet, named Mimas and Enceladus, have a markedly disturbing effect on the rings.

So, when Lowell came to compare the periodic times of any supposititious particles revolving round Saturn in the positions indicated by the gaps in the rings, with the periodic times of

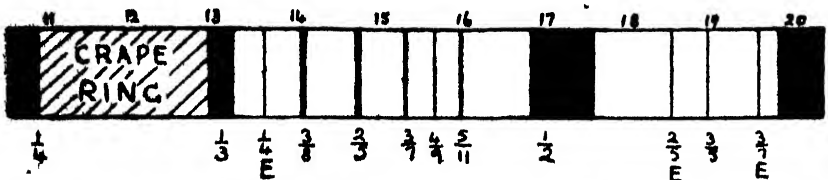


FIG. 5.—Gaps in Saturn's Rings showing distances at which rotation periods are commensurable with those of the Satellites, Mimas and Enceladus

Drawn by J. A. Lloyd, based on Professor Lowell's researches.

Mimas and Enceladus, he found that such a ratio existed. The diagram Fig. 5, based on Lowell's researches, shows this at a glance. Here we have a section as it were cut off from the rings, Saturn itself being to the left. The figures along the top do not concern us now, for they merely give the distances in seconds of arc of various parts of the ring-system from Saturn's centre.

The very wide black band is Cassini's division, and below it is the fraction ½, which means that any particle revolving round Saturn in that position must do so in half the time occupied by Mimas. Consequently the perturbating effect of Mimas on such bodies would tend to sweep them out of that position, and so leave a blank space in the ring. And so for each of the other divisions. Those distinguished by the letter E are due to Enceladus, and represent positions in the ring where the periods would be submultiples of the period of that satellite.

Of course, if the gaps in the rings had corresponded exactly with the positions assigned to them by theory, all would have

been well. But they did not quite correspond. Theory was perfect, the measures were satisfactory, yet something was wrong. What was to be done about it?

Astronomers had always regarded Saturn as rotating in one solid mass. But it was found that the gaps in the rings were all slightly too far out from Saturn's centre to be compatible with the solid theory. There was only one thing to do, and that was to assume that Saturn was built up of a series of concentric shells, as imagined in the picture-diagram, Fig. 6, the inner ones rotating faster than the other, and then to re-calculate the positions of the gaps on this assumption. When this was done, measurement and theory agreed perfectly. Thus, by trifling errors in the positions of the gaps in the rings, the astronomers were able to deduce the internal condition of Saturn.

The gravitational influence of Saturn's moons on the rings is shown in another way. When the rings are seen edge on, some of the largest telescopes show certain knots or swellings in the line which represents the ring at those times. In other words, the line is not equally thick all along its length. The knots are seen at the outer edge of each ring, the matter being more closely gathered together at those points than at the inner edges.

Prof. Lowell suggests that the swellings are due to collisions taking place among the particles of the ring, the effect being to drive some of them out of the plane of the ring and so increase its thickness at those points. According to Lowell, the swellings appear "exactly inside the points where the Satellites' disturbing action would be greatest, or, in other words, in precisely their theoretic place."

Coming now to some very recent work on Saturn, it was suggested in 1914 by Hepburn that the exterior ring was transparent. This view he put forward after an examination of a photograph by Barnard, which shows the ball of Saturn very clearly through the ring. This observation came in for some pretty strong criticism, but in 1917 Hepburn was proved to be right.

In that year Messrs. Ainslie and Knight were fortunate enough to see Saturn pass between us and a small star. Now when first seen the star was on the extreme edge of the inner bright ring, and as Saturn moved along, the star, of course, being fixed, some very curious appearances took place. The star passed all along Cassini's division, and was just as bright as when Saturn was not there, showing that Cassini's division was a real gap in the ring and not a mere shading on the face of it.

But as the star passed behind the outer ring it was very

considerably dulled, but was still easily visible. During its passage behind the ring it flickered twice, brightening suddenly for two short periods, the first lasting ten to fifteen seconds, and the second five seconds. What caused the brightening? The first was due no doubt to the star being seen through Encke's division, and the second to an unknown gap outside Encke's. Moreover, the star was not quite so bright when seen through these gaps as it was after Saturn had completely passed from before it.

Subsequent discussion of the observation brought to light several curious facts about which we need not here concern ourselves. But I might call attention to one thing, and that is: it was possible to get a very tolerable estimate of the size of the particles composing the ring.

The star was very small, and there was no perceptible twinkling of its light as it passed along. Taking into consideration the magnitude of the star, it would seem to require fragments considerably over two hundred yards in diameter to cause it to twinkle; therefore, we may safely say that the separate units of Saturn's rings cannot possibly be larger than this, and probably very much less.

The observation seemed also to show that there are only two gaps in the outer ring and not some half-dozen or so as Lowell shows. It is, however, probable that these smaller gaps are not permanent features at all, but open and close from time to time. Even Encke's division is not always visible, though it can sometimes be seen with quite moderate-sized telescopes.

Changes of this sort seem to be continually going on in the rings of Saturn. It is said that changes are also taking place in the Crape Ring; it varies in colour and brightness from time to time, and occasionally some well-marked irregularities have been perceived in its edges.

Very recently indeed it has been suggested that Saturn possesses another Crape Ring outside the bright ring-system. Such a ring seems to be required by mathematical theory. I believe that one or two distinguished observers claim to have seen it with the telescope, but more confirmation is required before the matter can be regarded as definitely settled.

That is where the problem of Saturn's rings stands at present.

THE CHEMISTRY OF EMBRYONIC DEVELOPMENT.

By JOSEPH NEEDHAM, B A.

Benn W Levy Student in Biochemistry, University of Cambridge

"I remember that Doctor Harvey ceme several times to Trin Coll: to one George Bathhurst B.D. who had a hen in his chamber to hatch eggs, which they dayly opened to discerne the way and progress of Generation."

John Aubrey: 1636.

When in the future the historian of Biological Chemistry looks back on the events of the last twenty years of the nineteenth, and the first twenty of the twentieth century, his attention will assuredly be drawn to at least one outstanding point. He will observe that some parts of biochemistry—standing though this subject does at the centre of the biological sciences—remained for years altogether neglected, while on parallel lines research proceeded with signal success. Indeed, this inequality of progression is well known to all who have had occasion to survey the history of science as a whole. On a large scale, it may be seen, for example, in the three well-marked periods into which progress in Anatomy has fallen—the first, from 1500 to 1560, associated with the names of Vesalius and Fabricius, the second, from 1600 to 1690, with those of Harvey and Malpighi ; and, thirdly, that later period beginning about 1770, and containing Bichat and John Hunter.

More interesting speculation will be afforded him, however, if he tries to find out the probable causes for this unequal advance. Doubtless, before 1850, the factors were more numerous, but since then, such factors as the varying facilities for research in different countries, the degree of encouragement on the financial side, the availability of material, and, above all, the position of experimental methods at the time—have all played their part. Probably of late years, and especially in biochemistry, the question of technique has been the most important. As Prof. F. G. Hopkins wrote in 1919, "In the growth of any branch of knowledge, there are, indeed, periods when the development of technique becomes the most pressing of needs, and its success the best measure of progress." The lack of accurate quantitative methods has certainly often

proved an insurmountable barrier to progress in certain directions.

But, to turn now from the general to the particular; what reasons, we may ask, will be found for the very small amount of work, relative to the importance of the matter, which has been done on the metabolism of the developing egg? Here the difficulty of experimental methods cannot entirely be held responsible. Methods which can be applied to blood can usually be applied also to the egg without much difficulty. Yet it is certainly true that an ignorance of the properties and constitution of many of the most important bodies in the egg, proved a stumbling-block to the earlier investigators—and may still do so, as our knowledge of them is fragmentary even now.

Another difficulty inherent in the problem is that of material. The eggs of insects and of fishes are usually too small to deal with except in very large numbers, and then the impossibility of telling how far development has proceeded is an exceedingly grave drawback. On the other hand, for some purposes, avian eggs, such as those of the hen, are too large. However, in most cases this has been the one chosen, and it is this one which seems to hold out most hope for future work. The difficulty of inequality of development is one that can only be overcome statistically. Everyone who has dealt at all with developing chick embryos knows that they never all begin and end together. Some seem to have the start of their companions by about twenty-four hours, and on the twentieth day a few begin to emerge, though others may remain inside their shells till the twenty-second day of incubation. Accordingly it is impossible to be sure that eggs withdrawn for analysis on the tenth day contain true tenth-day embryos; some will be late ninth-day ones, and others early eleventh day. However, by using a sufficient number of eggs, an average result can be obtained, and, considering all the inaccuracies inherent in most of the best estimation methods, this error can probably be made by no means excessive. But in the case of the very small eggs, and especially those whose incubation-period consumes months, it is quite impossible to be certain as to this point.

A more subtle cause of error, though one which has done probably more damage, is the indefiniteness of morphological boundaries. Are the membranes of the embryo, for example, to be counted as belonging to the embryo or to the rest of the egg? When the yolk-sac is being withdrawn within the body of the chick from the eighteenth day onwards, where does the "yolk" end, and where does "intestinal contents" begin? It is impossible to say; and analytical data may be

rendered very misleading and difficult to interpret by inaccuracies such as this. For them there is no help, except the faithful adherence to well thought-out arbitrary boundaries. But the correlation of chemical with morphological facts is no easy matter, as shall appear in due course.

We have now expatiated somewhat on the difficulties of "Embryochemie" (as it was termed by Liebermann in 1888), and supplied several speculations to the historian of the future, should he happen to read the old files of this journal. It is necessary, therefore, to see whether the results gained in such studies are at all commensurate with the difficulties of the work. That they really are is evident without much thinking. In the hen's egg, for example, we have a completely closed system in which for twenty-one days the most remarkable chemical transformations are occurring with very little loss or gain. Water and CO_2 are lost, but no N_2O_2 is taken in. Unlike the adult organism, no special feeding has to be done, no analyses of excreta have to be made, and bacteria, though certainly present in the interior of the egg (Pennington, 1910), can hardly be expected to possess the activity which is associated with the flora of the intestinal tract. They do not "line every approach to the body," as they do in the adult animal. We shall return to the special benefits obtained from the study of such a closed system later on. But who can fail to be impressed with the synthesis of the most complex substances, such as Hæmoglobin and the feather-pigments, from the simple constituents of the yolk and the white?

We might discuss now the different ways in which researches on the metabolism of the developing egg could be carried on, and afterwards detail the experimental results obtained. But instead, they shall come first; since facts should precede theories, and their exact significance shall be dealt with afterwards. At the outset there are several closely related branches of work which do not come into consideration. Much work has been done on the physical and chemical changes associated with the act of fertilisation; especially in America, but this is really a distinct phenomenon from the development of the resulting embryo, and will be disregarded. Another considerable portion of the literature deals with the gaseous exchange of the incubating egg, and for this we are chiefly indebted to the Dutch and Scandinavian physiologists. Still a third group of papers, by Tangl and his associates, deals with the "Entwicklungsarbeit," and the energy-changes bound up with it. But what we are here to consider is the series of actual chemical changes taking place as the embryo develops—a line of work which has been very insufficiently studied relatively to the others.

In the first place, among the innumerable chemical problems which attract our attention, the shell with which avian eggs are surrounded presents points of great interest. That we should take this first is incidentally justifiable on historic grounds, for in 1822 William Prout published an article in the *Philosophical Transactions of the Royal Society*, entitled "Some Experiments on the Changes which take place in the Fixed Principles of the Egg during Incubation." In this he contended that as the skeleton of the hatched chick contained more lime than could be accounted for by that present in the interior of the egg at the beginning, some of it must come from the shell. Out of this paper there grew a vigorous little controversy. In 1877 Voit decided that Prout had been wrong, for he got lower figures for the skeleton Calcium than Prout, and considered that all of it could be obtained from the interior of the egg. His average for the Ca in the embryo was ·0234 g., while the average yolk alone contained ·0347 g. On the other hand, in the following year Vaughan and Bills came to definite conclusions of an exactly opposite nature. Their average figures were as follows :

Ca in chick at end of incubation	·382 g.
Ca in interior of egg at beginning . . .	·070 g.
Ca acquired by chick from some other source	·312 g.

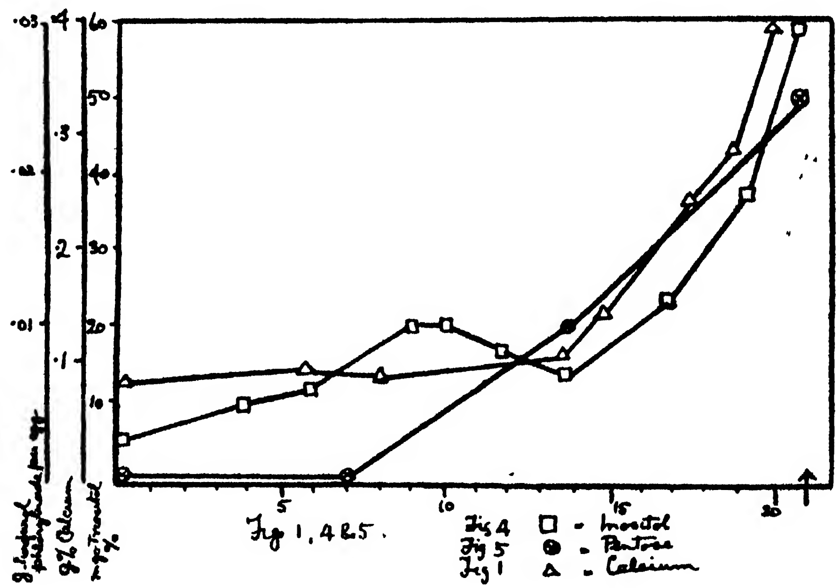
Ca in shell at beginning .	5·856 g.
Ca in shell at end . .	5·362 g.
Ca lost by shell	0·323 g.

Thus the gain in the embryo was just counterbalanced by the loss from the shell. In 1882 Pott and Preyer, in the course of analyses of large numbers of eggs, returned to Voit's conclusions. They considered that since Vaughan and Bills had only used from sixty to seventy eggs, their figures were not numerous enough to draw conclusions from. Individual variations might be so great as to negative these. The appraisement of these papers is complicated by the fact that the methods for Calcium estimation then in use were not remarkable for their accuracy. And in connection with this, it is amusing to note that Voit and Pott and Preyer all give their analytical results to four, five, or six places of decimals !

In 1918 Carpiaux reopened the question. With much improved technique he succeeded in finally confirming Prout, after a century of dispute. Fig. 1 shows the curves he obtained, and it will be seen that they place the matter in a definite position at last.

Mention of the process of Calcification or Ossification leads us directly to consider another aspect of the same phenomenon.

In addition to Calcium, Phosphorus is necessary for it, and it had been the speculation of the earlier writers as to how this was obtained. After some ineffective attempts to estimate the changes which Phosphorus compounds might undergo during incubation, the subject was put on a firm basis by the excellent paper of Plimmer and Scott (1909). Fig. 2 shows the changes which take place in the various fractions during development, calculated for the whole egg. A glance at these shows that among the most marked changes is the inorganic P. It increases from nil at the beginning to 60 per cent. of the total P at the end; and arises, as is seen from the diminishing quantities of other fractions, from the lipoid P, the Water-



soluble Organic P, and the Vitellin P. Moreover, it could be correlated well with the ossification process, which, beginning at a few centres as early as the eighth day, does not get into its stride till the eighteenth day, by which time the inorganic P curve is well advanced. A comparison of the Calcium curve of Carpiaux with the inorganic P curve of Plimmer and Scott is most interesting, for they almost exactly coincide.

But now we are to see how important a piece of work it was that Plimmer and Scott carried out. The decrease in the lipoid fraction (Ether-soluble P) was very marked indeed, and could only mean that the lipoids were being broken down into free fatty acids and inorganic phosphorus. Eaves (1910) subsequently showed that the free fatty acids of the chick

increased at the expense of the yolk, though not so as to account for all. And excellent evidence was available from the work of Bohr that fat was used as energy-source during almost the whole of development. A Respiratory Quotient of ·71 was normal. At this point the question is linked up with the study of Cholesterol. Tichomiroff, Mendel and Leavenworth, and Parke, had all, though using very inaccurate methods, found a gradual decrease of free cholesterol as incubation proceeded. They contented themselves with suggesting the highly improbable explanation that cholesterol might serve as an energy-source, and they left the problem at that. Gardner

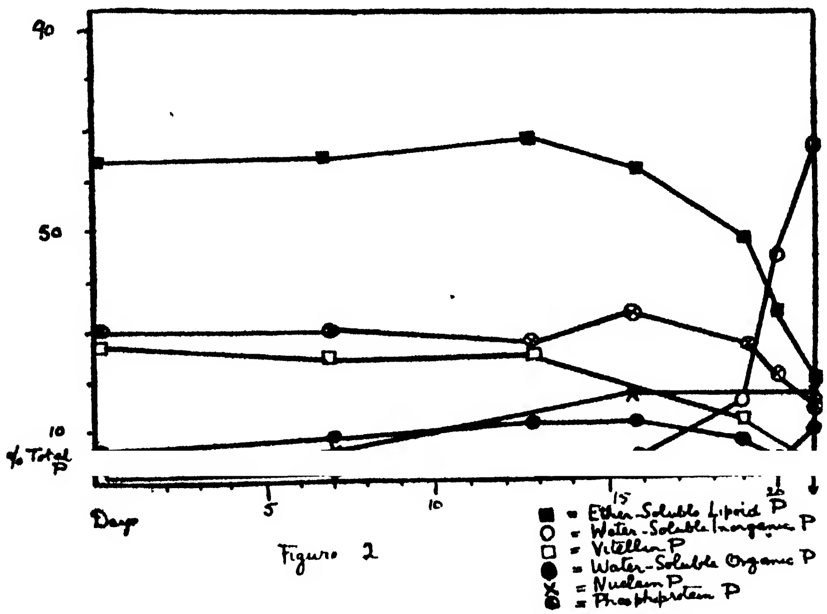


Figure 2

(1909) had shown that it was certainly not synthesised during development. More interesting still was the work of Hanes (1912), who, in the course of histological studies on the developing chick, observed that on the fourteenth or fifteenth day the liver became suddenly loaded with anisotropic doubly-refracting globules. By microchemical tests he proved that these consisted of cholesterol esters, and went so far as to associate them in some way with the process of ossification. It was reserved for Mueller (1916) to bring all these facts into correlation. His figures are set forth in Fig. 3, wherein it will be seen that the cholesterol remains practically constant in amount till the thirteenth day. After that time the free cholesterol diminishes very markedly, and the esters of

cholesterol increase in a compensatory manner.[1] Now, the Phosphorus figure of Plimmer shows that the lipoids do not begin to break down till the thirteenth day, so that Mueller's supposition that the fatty acids so formed combine partially with the free cholesterol can hardly be called a far-fetched hypothesis. On the contrary, it would seem certain that this is what actually happens. The amount of free cholesterol present would not, however, serve to fix more than 25 per cent. of them at most, and presumably oxidation accounts for the rest. Possibly this is a true detoxication-reaction. Now the breakdown of the lipoids as the egg develops might be supposed to lead also to an increase of free choline. Sharpe

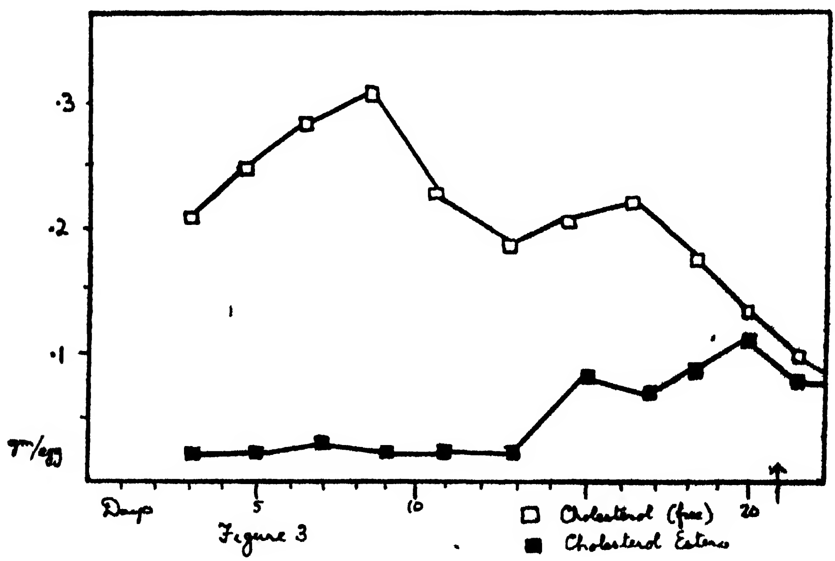


Figure 3

(1923) has recently investigated this point, and finds—though he only gives a few figures—that it decreases. It is only fair to say that he expected it would, since Burns had shown in 1916 that free guanidine increases, and choline was believed to be the precursor of the latter. On this subject there will be more to say later.

Another question raised by the work of Plimmer and Scott is the behaviour of the water-soluble organic phosphate. The chart shows a slow rise till the sixteenth day, followed by a rapid decrease. Plimmer explained the rise by suggesting that in the conversion of vitellin P to inorganic P there was

[1] A possible explanation for the decrease in free cholesterol is that since the lecithin is diminishing in quantity, the cholesterol must also diminish to keep the cholesterol–lecithin ratio constant.

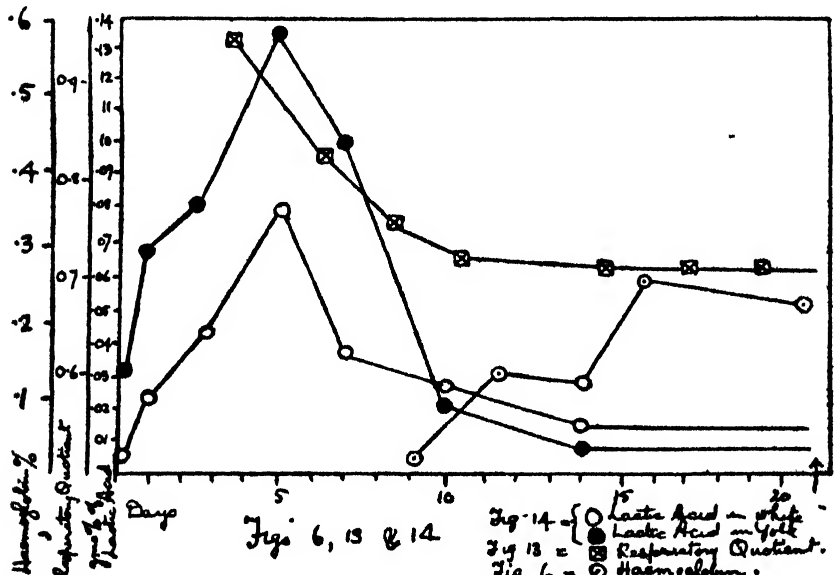
an intermediate phase when a water-soluble organic compound was formed. However this may be, the bodies which this fraction would include are glycerophosphoric acid, any Phosphorus in carbohydrate combination, and lastly, phytic acid, if indeed it is present in the egg. It is this latter substance which is in all probability the precursor of the cyclose of the body, *i*-Inositol; so at first sight it seemed possible that this could be correlated with the curve obtained for Inositol in the egg (Needham, 1923). This is shown in Fig. 4. The big fall after the fifteenth day is unexplained at present, but the general effect is a rise from 6 to 60 mgms. per cent. Calculation shows, however, that about ten times as much organic phosphorus as actually exists at the beginning would be needed to account for all the Inositol produced. A synthesis of it, therefore, is probably what takes place.

Yet another important fact established by Plimmer and Scott is that the Nuclein Phosphorus increases also as incubation proceeds. This is exactly what one would expect would happen as the tissues of the embryo grew and more nuclei were wanted. Although animal nucleic acid is now not considered to contain a pentose, it is interesting to see that Mendel and Leavenworth (1908) found a steady rise in pentose content during development. This needs confirmation, as they used but few eggs; their figures are shown in Fig. 5. All one can deduce from this is that nuclear metabolism is probably more intense towards the end of incubation.

Again, in the experiments of Plimmer and Scott, it will be noticed that the Vitellin phosphorus falls from 30 per cent. of the total P to practically nothing. From the work of Hammarsten and Bunge (1882), we have evidence that the compounds specially responsible for hæmoglobin-formation are vitellin and its near relatives such as Bunge's "Hæmatogen." The nature and properties of this substance have received so much attention that they form a small literature entirely on their own, yet even now little is known about them for certain. On the other hand, Liebermann in 1888 and later on Frankel estimated the hæmoglobin-content of the embryo at different stages. Their combined figures are given in Fig. 6. The curve is, indeed, much as one might expect, but it is interesting in that it does not begin its rise till the tenth day, while the breakdown of vitellin, as seen from Plimmer's chart, does not begin appreciably before then either. It is surely to these chronological correlations that we must look if we want a clear conception of the cogwheels of the egg's metabolism.

Before leaving the question of the transformations which happen to the fats while development proceeds, it is interesting to note that some work has been done to estimate how their

degree of saturation varies during the process. Mottram in 1915 found a peak on the eighth day, but he did not differentiate between the fat in the chick and that in the remainder of the egg, so this result is not very illuminating. Eaves (1910) had previously gone deeper into the matter. Her curves are given in Fig. 7. Obtained from a considerable number of eggs, they clearly show a primary absorption by the embryo of the less saturated fatty acids, followed later by a more equally distributed absorption after the seventeenth day. Now, as has been found by Dakin and others, the first step in the utilisation of fatty acids is undoubtedly their desaturation—a process which appears to be performed in the

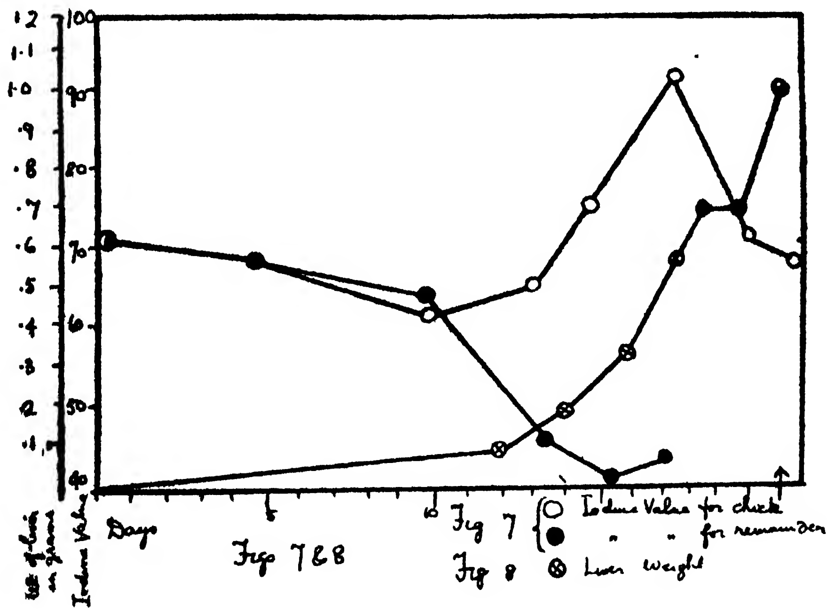


liver. And it was later shown by Mellanby (Fig. 8), in quite another connection, that about this time the liver weight is most rapidly increasing. We can hardly be accused of undue speculation if we assume that here the limiting factor in fat assimilation is the liver tissue, and the highly saturated fats are left outside the chick till a time when it can better utilise them. At any rate, here is a working hypothesis.

Less work, on the whole, has been done with regard to nitrogenous metabolism. Gortner in 1913 investigated that of the eggs of the brook-trout, *Savellinus fontinalis*, N. He was much impressed with the importance of embryonic metabolism, and considered the Nitrogen-changes to be the key to the understanding of the whole. He wrote: "Do the raw

materials of the non-embryonic part of the egg go in to the embryo unchanged, or are synthetic changes also taking place? If not, the simple ovoalbumin and ovomucoid must contain all the amino-acids necessary for the complete animal, and, furthermore, in the right proportions."

His final chart shows a number of changes, but none which are very easy of interpretation. He himself considered that the most marked characteristic of the whole was the constant trend from non-basic Nitrogen to basic Nitrogen, and that 63 per cent. of the energy of development must arise from protein substances; the greater part of this



contribution coming from the monoamino-acids. Later on he repeated this work on the eggs of the giant salamander, *Cryptobranchus alleghiensis*, and obtained almost exactly similar results. During incubation, free Tyrosine was found to diminish in amount, but other amino-acids were not followed. Tomita afterwards found that the Reststickstoff also diminished in amount, but not till after the tenth day. Bywaters in 1913 discovered the interesting fact that the ratio between the ovomucoid and the ovoalbumin in the remainder of the egg was a constant throughout the whole twenty-one days. This result is important, as it shows that at no time does the embryo preferentially absorb ovomucoid. Whether this is broken down into its protein and carbohydrate constituents before absorption is a more difficult question to answer. As we shall

see later, the total concentration of glucose in the incubating egg sinks gradually, and the curve for it probably hides several separate ones. It might be possible that instead of a constant equal absorption of ovomucoid and ovoalbumin, the latter only was absorbed, and was formed from the former. This would certainly lead, however, to an increase of free sugar—which has never been observed.

The only observer who has dealt with the purines in the egg is Tichomiroff. He studied in 1882 the purine-content of the eggs of the silkworm, *Bombyx mori*, and found ·02 g. per cent. at the beginning; ·23 per cent. at the end. The significance of this result is obscure, for we do not know the intervening points on the curve, and we do not know either how directly comparable the eggs of the silkworm are to those of the hen. All we can say is that like the pentose figures a more vigorous nuclein metabolism is perhaps present at the end than at the beginning. This is only what would be expected.

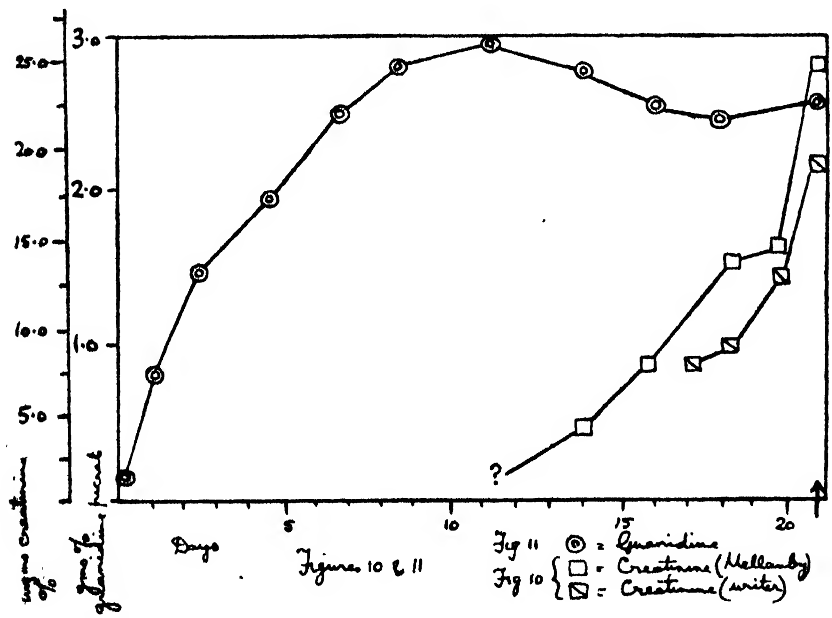
As regards Creatine and Creatinine, the only paper on this subject is that of Mellanby (1907). In the unincubated egg he could find no trace of either (though both Kojo and Salkoffski had previously done so), and it apparently did not increase till the fourteenth day. The present writer has been able to confirm the latter part of Mellanby's curve, though the values came just a little lower. The ascending curve (Fig. 10) Mellanby correlated with the increasing weight of the liver (already seen in Fig. 8) and also of the body *in toto*. But since the inorganic P, the Calcium, and numerous other substances are all ascending rapidly during the last five days of incubation, this is surely somewhat rash. There seems little justification for it. Not much light, unfortunately, was thrown on the matter by Burns's work (1916). In order to ascertain whether guanidine could possibly be held responsible for creatinine formation, he very carefully estimated the total guanidine in the egg at different periods of incubation. His curve is shown in Fig. 11. At first sight, it fits in well with Mellanby's creatinine curve, for the creatinine does not begin to increase till the twelfth day, and here we see the guanidine coming down from its maximum about the same time. Unfortunately, the method of estimation of guanidine includes creatinine in its values, so the total guanidine curve should not fall, it should continue to rise without intermission. Three possible explanations exist. After the twelfth day the protein complex which supplies the guanidine may become more resistant to oxidation, and so escape the method; or the precursor of guanidine, whatever it may be, may be destroyed by enzyme action somehow. Thirdly, the guanidine may, after the twelfth day, not only form creatinine but also

get locked up in very stable ring combination, and so result in an apparent decrease. But there is no doubt as to the accuracy of the figures.

In spite of the obvious interest of the egg's carbohydrate metabolism, only two significant substances have been systematically estimated. All workers are agreed, however, that the following figures are accurate for the unincubated egg :

Glucose in white . .	·74 per cent.
Glucose in yolk . .	·14 ,, ,,
Total percentage . .	·45 ,, ,,

Greene in 1921 showed that for the eggs of the king-salmon,



Oxyrhynchus, the usual amount of glucose was always present, no matter what the animal had been feeding on, or even whether it had been starving. Similar results are seen in the classical papers of Miescher.

Tomita in 1921 made the most exhaustive experiments on this subject. During incubation the glucose sinks to a trace in the white, but remains almost constant in the yolk. (The only evidence for the existence of a polysaccharide in the egg is the work of Bierry, who in 1910 found more glucose after treating the egg with hydrofluoric acid than before.) Bonnanni and Anno had previously found that lactic acid was present in the egg, but it was left for Tomita to follow its concentration

during development (see Fig. 14). The peak on the fifth day is very interesting, for it may mean that the enzyme which splits lactic acid to CO_2 and water is not present in the egg till after the fifth day. Also it stands in marked opposition to the majority of the other curves. The usual behaviour of a metabolite is to remain quiescent till about the twelfth day, and then to move violently up or down. A possible hypothesis is that the substances which are apparently inert or non-measurable for the first fortnight are really not so at all, but also have peaks at this period; much too small, however, to be observable by our present methods of analysis. And it cannot be denied that a knowledge of the chemical events proceeding in intimate association with the formation of the primitive groove, the medullary folds, and the laying down of the three germinal layers, would be of the highest interest. Embryologically the fifth day is chiefly remarkable for the appearance of knee and elbow, and for a large growth in the size of the Central Nervous System. However, to return to lactic acid, Tomita found that he could materially raise the lactic acid maximum by *injecting* glucose into the egg on the first day with the aid of a sterile technique. Amino-acids, such as Alanine, had no effect, and he concluded that he had proof of the carbohydrate origin of the lactic acid in the egg. If this be really the case, it is extraordinarily interesting that in the first week of incubation glucose is utilised and lactic acid formed. For the Respiratory Quotient of the egg does not attain its steady figure till the end of the first week; before then it is about ·9, but afterwards it is constant at ·71. Thus, after the first week fat is being used as the energy-source, but during the first week, apparently some carbohydrate too. The Respiratory Quotient, as given by Bohr in 1903, is plotted in Fig. 13.

The sulphur metabolism of the egg has hardly been worked on at all. Liebermann was impressed by the formation of feathers, and estimated the S in them during the last ten days of development; the curve so produced, however, shows extraordinary peaks, and in view of his methods, can mean but little. Since the discovery of Glutathione by Hopkins in 1921, the amino-acid cyctine has attained so important a position in relation to tissue oxidations, that more data as to the distribution of sulphur in the developing egg are badly needed.

We have now reviewed the literature which exists at the present time on the metabolism of the developing egg. It will be seen that it is quite wide, but somewhat shallow: in only a few places have really deep foundations been dug.

More surprising than this is the fact that all these researches

have not been previously brought together. The reason for this, however, is perfectly simple and illustrates a change in tendency which has been influencing biochemistry more than is generally realised. The earlier workers—indeed, most pre-war workers—approached the egg each from his own standpoint, asking, "What can it tell us about creatinine; or hæmoglobin synthesis; or some other *special* subject?" The conception of the egg as an end in itself does not seem to have held the imagination of more than a few workers; the majority never thought at all of the egg *quâ* egg. It is in just the same way that we are now realising that the bacillus is an organism, and not a convenient box of enzymes; we are thinking of the bacillus *quâ* bacillus (cf. Stephenson and Whetham, 1922). Hence in a paper on glucose in the egg, for example, there will be many references to previous work on glucose, and but few to previous work on eggs. Thus it has come about that when the present writer set out to think about this subject, he did not foresee how many most interesting correlations already worked out would present themselves. And in future, we must go to the egg, not because we are interested in glucose, but because we are interested in the part that glucose plays in the chemistry of Embryology.

From the review which has been given, it will have been seen how small is our understanding of the general problem of the dynamic chemistry of the embryo. At a moment's glance there spring to the mind any number of possible correlations which need investigation. How does the embryo accomplish the synthesis of its blood and feather pigments? How does its Sulphur metabolism provide for such diverse things as glutathione and keratin, from the simplicity of ovoalbumin? How does the purine metabolism of the egg end?

As a matter of fact, the egg is, in a word, the place *par excellence* for the study of *Precursor problems*. *A priori*, if the curve for a substance X goes down just at the same time as that for Y is coming up, then, other things being equal, there is ground for a belief in their interdependence. Yet the search for correlations must proceed with the greatest circumspection; for here we have a field covered with pitfalls. Moreover, one curve may altogether mask several others, and this fact must especially be borne in mind when a substance is present in apparently the same concentration throughout. A curve may rise, but small amounts of a substance reckoned in with it may really be descending. Again, straight lines may sometimes be the resultants of equally vigorous anabolic and katabolic processes. Yet we must hope that in time it will be possible to unravel all such obscure relations.

Perhaps it is somewhat difficult to say how universal in application are results drawn from the incubating egg. To say that because a certain synthesis goes on in the egg, it probably also goes on in the adult animal, is an assertion possibly a little rash. When it is remembered that the eggs of warm-blooded animals behave during incubation as if they were cold-blooded, we are reminded that great differences do exist between the two states, the egg and the adult.

The injection method of Tomita promises most interesting results. That eggs which have been injected with various substances will still go on developing normally, is in itself a discovery of no small interest.

And in the effects of the injection of metabolites on the shapes of the curves of others, we have indeed a wide field of investigation. Probably work of this sort will throw much light on what, when, and how, the embryo absorbs.

The incubating egg can, of course, be considered from the morphological, the physiological, and the biochemical standpoints. It was the morphologists who were first in the field, and the results of their labours cannot be too highly praised. Yet it must be admitted that their data are not so useful to the "Embryochemist" of to-day as they might be, because the embryologists were all histologically minded, and, with few exceptions, disinclined to use other instruments than the microscope. The embryo ceased to interest them after the tenth day. Once the primary form of the animal was laid down, its subsequent development was a merely anatomical question. But what the chemist would like to know would be the weights of all the organs from the tenth day onwards, since he has found that the most marked chemical changes take place towards the end of incubation.

When the embryologist declined and fell, the physiologist took over the egg. The work of Tangl, Bohr, Hasselbalch, and Krogh on the gaseous exchange and heat production of the egg has certainly proved most useful. But though this domain is by no means nearly exhausted, it is time that the chemist came into his own. At least, in the writer's opinion, the future in this matter lies with the biochemist only, and with biochemical methods of investigation. Nor is this a unique phenomenon, for if, as many believe, the whole domain of Medicine itself is bound in time to become applied biochemistry, it will hardly be surprising if Embryology suffers a similar beneficent change.

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SOME CAUSES OF A C_3 POPULATION.

By Prof. E. W. MacBRIDE, F.R.S., D.Sc.,

Chadwick Lecture delivered at the Royal Society of Arts, May 9, 1924. Published by kind permission of the Chadwick Trustees

In selecting a zoologist to give a lecture for the Chadwick Trust, the Council gave evidence of a desire to have the subject of public health dealt with from a broader point of view than would be possible if one were to limit oneself exclusively to the consideration of purely human data.

The advantage which the zoologist possesses above the student of medicine is that whereas both are researchers in the field of biology, the zoologist can subject his hypotheses to the test of experiment and can standardise the conditions of his experiments, whereas the medical man must draw conclusions from sporadic observations and from statistics—you must not therefore consider that I am wandering from the subject of hygiene if I deal with such creatures as gold-fish, newts, and toads. The laws of life are the same for all the higher animals, and the effects of evil surroundings on the growth and activities of the lower vertebrates throw a startling light on the conditions which cause the poor stunted human beings which fill our slums.

The phrase, "C_3 population," became familiar to us all during the closing months of the Great War. After we had exhausted the pick of young manhood, the A men, and had even depleted our reserve of men of ordinary ability but not first-class physique, the B men, we had to fall back on the residue who were neither mentally nor physically fit, and these were denominated the C class and the lowest grade of these were placed in the C_3 division of this class. My friend, Dr. Austin Freeman, the author of that important book *Social Decay and Regeneration*, has introduced the term "sub-man" to describe the C_3 people, a name suggested by the correlative term "superman" employed by Nietzsche and his followers for the type represented by those individuals of superior strength and ability who, according to him, should alone perpetuate the race. Dr. Austin Freeman, who, during part of the war, was charged with the medical examination of the recruits, had ample opportunities for observing the

C_3 class, and this is how he describes the "sub-man": "His mental condition is generally rudimentary. He is quite unable to take in a general idea even of the simplest kind. Since propositions remain in his mind separate and unconnected, he is practically unable to reason, and is insusceptible to demonstration or proof. The sub-man's vocabulary is extremely limited, he is ignorant of the meaning of quite ordinary words. He tends to speak with abnormal rapidity and to articulate imperfectly, and he seems to be unable to control this tendency. He is quite indifferent to noise, but is usually quite devoid of musical faculty. He has a rudimentary conception of cleanliness. He cannot be induced to keep dirty hands away from wounds or sores. He is usually quite helpless and unhandy, and has as a rule no skill or knowledge of handicraft or knowledge of any kind. Contrasted with him the negro is rather sprightly and humorous. The negro has some traditional knowledge of religion, myths and folk-lore, and some acquaintance with native music. He is handy and self-helpful, can usually build a house, thatch a roof, obtain and prepare food, make a fire (without matches), spin yarn, and can often weave cotton cloth and make and mend simple implements, and physically he is robust, active, hardy, and energetic."

Dr. Freeman concludes that the sub-man represents a "throw-back" to a far more primitive evolutionary level than that represented by the negro. In this conclusion I disagree profoundly with Dr. Freeman. The sub-man is not a throw-back at all—at no period in the evolution of our race were our ancestors represented by such worthless, helpless creatures; if at any time individuals like these had turned up they would have been ruthlessly cleared off the earth by natural selection. For, as Dr. Freeman himself points out, the sub-man is incapable of supporting himself without help from his fellow citizens. "It is he," says Dr. Freeman, "who has made Collectivism possible, it is he who, when it is fully established, will enable it to accomplish its mission of racial degradation and annihilation." The sub-man is a pathological defective; he is a reckless reproducer of his species and he always breeds his like. In fact, what Dr. Austin Freeman calls the "sub-man" the American sociologists term the "moron."

The moron is a mental defective, but he is a mental defective of a comparatively high grade, and precisely therein lies his danger to society. The grade of mental defect is defined by the extent to which the mind has expanded when growth has ceased. Two French doctors, Messrs. Simon and Binet, after years of experimentation with the elementary school children of Paris, succeeded in devising a series of

tests, arranged in steps of increasing difficulty. A normal child of a year old can accomplish the tasks of the lowest step, one of two years old those of the next step, and so on. If therefore an adult moron can only perform the tasks which can be accomplished by a normal child of eight he is said to be of the "mental age" of eight years. Mental defectives of the mental ages of one, two, and three cannot dress or feed themselves: they must therefore be confined for life in asylums, and are what we call idiots. Although they are a burden to the community, they do not procreate their species. Those of mental ages of four, five, six, and seven can be trained to dress and feed themselves and to perform simple tasks under direction, but they are utterly unable to maintain themselves by their own labours in competition with their normal fellows in the world outside. They, too, therefore must be maintained in asylums at the public expense: they are termed imbeciles, and are prevented from reproduction. But people of the mental ages eight, nine, and ten have just enough intelligence to be able to support themselves in the lowest paid and least skilled occupations; it is they who constitute our C_3 class.

The application of the Simon and Binet tests to the delinquents brought before the magistrates in America has revealed the fact that a very large proportion of the criminals, habitual drunkards, and prostitutes are morons. Mental defectiveness, in a word, is exhibited by want of self-control as well as by want of intelligence, and self-control is the basis of all morality. Want of self-control is seen not only in inability to resist the lure of drugs, as in the case of the drunkard, but in the tendency to give way to temptations of all kinds, and in the want of power to resist passionate impulses.

It is a matter of cardinal importance to discover whether or not mental defect is hereditary. This problem has been solved by Dr. Goddard, a distinguished alienist who was attached to the Vineland institution for the care of the mentally defective in New Jersey. Dr. Goddard trained a series of observers who co-operated with him for some time in the institution and learned his methods and were then sent out to seek out and become acquainted with the relatives of the children who were in the institution. In this way charts were constructed showing how many relatives of the afflicted child were of normal mentality and how many were obviously mentally defective. In cases of doubt blanks were left in the chart. Normal relatives were indicated by the letter N, defectives by black marks. The chart was revised at intervals of years by different observers and in no case was it found by later observers that their predecessors had underestimated

the number of mental defectives amongst the relatives of the child. In all cases it was found necessary to "blacken the chart." Dr. Goddard found (excluding doubtful cases) 482 children each of whom was the offspring of two mentally defective parents. Practically all of these children (476) were mental defectives. Where one parent was mentally defective and the other parent had mental defect in his or her ancestry about one-half of the children were mentally defective. Now if mental defect be regarded as a "mutation," that is, an abnormality which is "recessive" to the normal condition, and is inherited according to the Mendelian rules of heredity, these figures are exactly what we should expect. For when a hybrid, which has the appearance of the normal, is mated with the recessive, half the offspring are recessive, and when two recessives mate with each other all the offspring are recessive. It might perhaps be objected that Dr. Goddard found that six children both of whose parents were defectives were normal. But these exceptions prove nothing. Mentally defective women are usually immoral, and these six children were probably the result of the adultery of their mothers with normal men. Goddard cites one case where two mentally defective parents had a family of eleven children, nine of whom were mentally defective. Two were normal, but these two were black.

Having identified our C_3 men with high-grade mental defectives and having learnt that the condition is hereditary, we now approach the central problem with which we have to deal, viz. the original causes of mental defect. That it is not due to the atavistic cropping out of a primitive condition of affairs we have already seen. The case of the negro bastards just mentioned is sufficient to negative this supposition. Various causes have been suggested—sometimes, indeed, dogmatically asserted by emotional social reformers. Of these the most important are syphilis in the parents and intemperance of the parents. Now Goddard's observations throw light on both these supposed agents. If we divide the parents of mentally defective children into two groups, those infected with syphilis and those free from this taint, we find that these groups are equal in numbers; or, again, if we take syphilitic parents, we find that only a small proportion of their children are feeble-minded. These figures seem to show that syphilis of the parents has no bearing on the mental condition of their offspring. The same result follows if we examine the children of alcoholic parents. Some of them are feeble-minded, but an equal number are quite normal. But alcoholism appears often to be not the cause but a symptom of feeble-mindedness. There are, as Goddard points out, two classes of people addicted to alcohol: (1) normal people who have fallen into drinking

habits; these, if they become seriously alarmed at the results of their indulgence, can become completely reformed; (2) mentally defective people; these will repent many times with tears and will sign the pledge every week, only within a week to relapse into drink again.

Mental defect, as we have seen, behaves as a Mendelian recessive. If we examine the conditions under which other recessive variations turn up, we find that wherever plants are cultivated or domestic animals bred in large numbers occasional aberrations make their appearance and these variations, when crossed with the type, behave as recessives. Thus, in the most famous case of all, Prof. Morgan and his pupils have bred over a million specimens of the banana-fly, *Drosophila melanogaster*, and amongst these at least 300 abnormal specimens have appeared. These variations, when compared with the type, are all defectives; in most cases the eyes or the wings are imperfectly developed. The obvious suggestion is that there is something in the conditions of overcrowding and confinement which causes the defective aberration to appear.

This idea has occurred to many biologists, and I now wish to describe the way in which it has been followed up by one distinguished researcher, Gustav Tornier, whose work has been overlooked almost as much as was that of Mendel when Mendel's results were first published. Tornier was led to examine the history of the various varieties of goldfish and the conditions under which they had made their appearance. No animal has presented more numerous or bizarre variations. Thus, fish with two tails side by side are known: others occur with enormously long dorsal and anal fins—fins which are flexible with drooping ends, for which reason their possessors are termed VEIL-FISH. In other varieties the dorsal or anal fins, or sometimes even the tail fin, may be absent and the body is small and rounded. The eyes may bulge like telescopes, or the skin of the head may be puffed out or may project backwards in a fold like a hood, and this fold may be reversed. The scales may be absent and the well-known red colour may grade into silver or white, or the animal may be flecked with patches of colour (the so-called tiger pigmentation). These modifications are hereditary, and the most striking of them have been perpetuated as strains by the breeders. Moreover, when crossed with each other the offspring appear to follow the laws of Mendel. Now Tornier found that the original home of all these goldfish was China, where they had been cultivated for hundreds of years. It is true that many wonderful varieties are imported from Japan, but the Japanese writers are explicit on the point that all their goldfish came originally from China. The wild ancestor of all these fish

is *Carassius auratus*, a very ordinary grey carp which is found wild in the rivers and streams of China. This fish is normal in all respects. It has moderate-sized and normally placed dorsal, anal, and caudal fins. Its body is long and well developed, the eyes and skin of the head are normal, and the colour is the usual grey passing into silver below of most river fish. According to Chinese writers the goldfish was discovered about A.D. 1200. By this discovery is obviously meant that specimens showing the golden red colour turned up then. These attracted the attention of fanciers, and became popular, so that large numbers were bred and amongst them the other varieties made their appearance.

Now Tornier was struck by the unhealthy conditions under which the Chinese kept their goldfish. In winter these are kept in small earthenware pots ranged by hundreds on shelves in dark, insanitary huts. In summer they are kept out-of-doors in small, dirty tanks overgrown with weed, and in these tanks they spawn. A large proportion of the spawn dies, but the residue which survives gives rise to fish amongst which many abnormal specimens make their appearance. The abnormalities are of all kinds and of every degree of intensity; the most striking are selected by the dealers and used to propagate the "fancy" races. In breeding these races there is always a good deal of reversion towards the normal type; only a small proportion of the offspring show the "fancy" points in high degree. By rigorous selection carried on through several generations the amount of reversion may be very much reduced.

It occurred to Tornier that the evil conditions under which the early development of these fish was accomplished were responsible for the formation of these abnormalities, and he formed a theory as to how the abnormalities were brought about. This theory may be termed the hypotheses of germ-weakening. By the deprivation of light and oxygen, the most active part of the egg is weakened and its vital energy or impetus to development (*l'élan vital* of Bergson) is lessened. This loss of energy shows itself in two ways. First in the slowing of development, and secondly in the failure to regulate the intake of water. Davenport and Parker showed long ago that the principal factor in the early stages of growth was the absorption of water. This was determined by finding the successive increases in weight in tadpoles during the first six weeks of their life, and in discovering what proportion of these increases were augmentations of the "dry" weight.

In a fish's egg the embryo is developed from a patch at one side of it; the rest of the egg forms a bag containing a store of food-material termed yolk. As the embryo grows it becomes coiled round the egg. Eventually the whole of the yolk is

absorbed by the developing blood-vessels and used as food by the growing embryo.

Now, when the embryonic area is weakened, this intake of water is no longer controlled, and the yolky area becomes greatly swollen by absorption of fluid. Further, the natural cavities of the developing body, such as the mouth, the gill cavity, and the abdominal cavity, become engorged with water. The swollen yolk-sac opposes an obstruction to the elongation of the principal axis—that is, of the vertebral column of the embryo. This column, which is normally slightly concave upwards, becomes horizontal and even convex, and the fish appears short and plump, with a protuberant abdomen.

The fins are essentially folds of skin. If the area from which the fin is developed is put on the stretch by the swelling of the region below it, then the fin grows to an abnormal length, as in the case of the dorsal fin of the veil-fish, or comet-fish. If, on the contrary, this area happens to be compressed by the pressure of swollen yolk situated next to it, then the fin may be reduced in size or even absent, like the anal fin of the lion-head. Even the tail-fin itself may occasionally be absent. The tail-fin is normally formed by the coalescence of right and left rudiments. In many of these germ-weakened gold-fish part of the swollen yolk becomes intruded between the paired rudiments and separates them, and so a double tail-fin is produced. Sometimes the lower lobe of the tail is doubled, whilst the upper lobe remains single.

In a normally vigorous embryo movement begins at an early period of development. This movement distends the membranous egg shell and provides more room for the blows of the embryonic tail. But in a germ-weakened embryo movement is sluggish and the distension of the egg-shell does not take place, and so the elongating fins become pressed against this membrane and their tips bent, and so the drooping appearance which has given rise to the appellation " Veil-fish " is produced.

We have already mentioned that the mouth cavity becomes swollen with water. This swelling tends to buckle up the roof of the mouth and in particular the mesethmoid bone which forms the support of this roof. In this way the characteristic short round head of the fancy goldfish is produced. In the breed termed the " Lion-head," whilst the bony framework of the skull is shortened in this way, the skin covering the head is not affected, and so the skin, being longer than the case over which it is stretched, becomes wrinkled and folded so as to present the appearance of a series of blisters. In extreme cases this skin may be folded back from the neck region like a turned-up collar.

The cavity of the eyeball is a space enclosed between the

lens of the eye and the concave retina, which is an outgrowth from the brain. In certain breeds this, too, becomes swollen with imbibed water. As a consequence the eyes protrude till they become tubular, and the name "Telescope-fish" has been bestowed on this variety. When these eyes are examined microscopically it is found that the retina which clothes the interior of this enormous eye-ball is very thin and has evidently been subjected to extreme tension. Indeed, over large regions of the inner surface of the eyeball it is absent, so that the larger eye is not correlated with increased powers of vision. The brain cavity in this type of fish is distended with water, and the right and left hemispheres, which should be in contact, as also the right and left optic lobes, are widely separated by the enlarged ventricles.

Lastly, the germ-weakening accounts for the characteristic colour of goldfish. The wild Carassius, like the majority of river-fish, is grey above, merging into silvery-white below. Now in the weakened germ, when the yolk is swollen by the absorption of water, a portion of it becomes coagulated and is thereafter impervious to the liquefying ferments of the embryo. This useless portion is ejected through the anus. But this lessening of the nutritive reserves of the developing animal makes its effect felt later on. When there is no more yolk available and the development is not complete, the embryo falls back on the tyrosin which is universally present in the skin-cells. It is this tyrosin in the normal fish which, by becoming oxidised into melanin, gives rise to the characteristic grey colour. But when this material is utilised as food, the oxidation process may be stopped at any stage and every colour grade from dark red to white can be produced.

Such, then, is Tornier's theory of the origin of varieties in the case of the goldfish. The first question which arises in the mind is, what evidence has he brought forward in favour of it? The answer is that he has been able to produce experimentally quite similar aberrations in the embryo of newts, frogs, and toads, by subjecting the eggs to darkness and the lack of oxygen for periods varying from 2 to 24 hours after they were laid. In the case of that large newt, the axolotl, he used solutions of 10 per cent. of sugar for 1 to 2 days; in the case of the eggs of frogs, solutions of 25 per cent. of sugar for shorter periods of 4 to 8 hours. Too long immersion in these fluids killed the eggs.

I have repeated Tornier's experiments with the tadpoles of frogs in my own laboratory and have confirmed his results. I obtained sluggish tadpoles with protuberant abdomens and imperfectly developed tails. Tornier gives figures of axolotl embryos showing the shortening of the principal axis by the

protuberant yolk. Some of these he succeeded in rearing to the larval stage. The abdominal regions of these larvæ were round and protuberant: when they were dissected it was found that the abdominal cavity was swollen with water which had pressed on and impeded the growth of the liver, lungs, and alimentary canal.

In support of his theory as to the production of colour Tornier performed experiments with the eggs of the spade-foot toad (Pelobates). By stabbing these eggs with a fine needle in an early period of their development he caused the loss of a considerable quantity of their yolky contents. When these eggs developed into larvæ, these, instead of showing the normal brown colour of tadpoles, exhibited various shades of red merging into light lemon yellow.

Of course, an objector will at once answer that these experiments all referred to effects produced in the first generation, and that no proof was adduced to show that they could be inherited. This is no doubt partly true; few realise the enormous lapse of time required to carry out breeding experiments on animals such as these. To rear a newt or a gold-fish to maturity from the egg requires at least four and often five years. But a pupil of Tornier's, Milewski, has performed experiments which indirectly show that these aberrations are inherited. Milewski chose two veil-fish in which the fins were moderately elongated. Under ordinary circumstances these fish produced a brood 10 per cent. of which showed the mark of the "fancy," namely, very high dorsal fins in marked degree; the remaining 90 per cent. consisted of fish which exhibited every degree of development of the dorsal fins ranging from tall to normal. The same parents were allowed to breed again, but this time they were confined to a tank overgrown with water plants and kept in the dark. Under these circumstances the oxygen content of the water was reduced in extent. The eggs, after being laid, were allowed to remain for a week under the same conditions and were then transferred to well-aerated water. These eggs developed into young fish 90 per cent. of which had extremely tall fins—were, in fact, well-marked types of the "fancy" and 10 per cent. of which were constituted by various grades of reversion to the normal. If the tall fin were due, as Mendelians would suppose, to some "gene" unalterable by the environment which was "carried" by only a certain percentage of the germ-cells, then it is totally inexplicable why the stinting of the germ-cells of oxygen should alter the number of "carriers" from 10 to 90 per cent. Milewski reared some of this later brood of fish for three years, when the Great War supervened and put an end to his experiments. Before this time, however, certain

of these fish began to exhibit a monstrosity which was not found in either parent, viz. the telescopic bulging of the eyes. This, as we know, is an inheritable variation, and it had obviously been produced in this breed by an increasing amount of germ-damage.

Apart altogether, however, from Milewski's experiments, the objectors to Tornier's conclusions must face the following dilemma. Admittedly the originators of the various breeds of goldfish have "cropped up" in the cultures of the Chinese breeders who keep their fish under insanitary conditions. When these conditions are artificially reproduced and the eggs of other animals subjected to them, the embryos which develop from these eggs show precisely the same type of aberration as is exhibited by the fancy breeds of goldfish. Is it seriously maintained that the abnormalities of goldfish have occurred by "chance" and are therefore inheritable; and that the similar variations in tadpoles produced under the same circumstances are purely environmental and non-inheritable? We believe that no reasonable person would endorse such a conclusion. We must, I think, agree with Tornier that what is inherited is not a gene or "determiner" for a specific factor, but a grade of germ-weakening and that the abnormal structures which appear are the result of it. Tornier goes on to point out that, once one learns to recognise the characteristic stigmata of germ-weakness, one can observe them throughout a wide range of domestic animals. Thus, for instance, he shows that if we compare the skulls of the Yorkshire pig and of the wild boar we see in the former the characteristic shortening of the face and buckling up of the facial bones which is so clearly exhibited by the breeds of goldfish. He points out that domestic pigs are kept in dark, filthy sties full of manure, whilst the wild pig brings up its young in the open. The mammalian egg, it is true, has no yolk, but draws all its nourishment from the maternal womb; but it develops into a vesicle filled with fluid, and the embryonic portion is closely invested by a membrane termed the amnion, between which and the embryo itself there is an accumulation of fluid. It is obvious that we have all the conditions necessary for the production of excessive pressure and of all the evil results that flow from it in the case of germ-weakening. No one can see what goes on in the developing mammalian embryo, but the embryo of the bird is more accessible to observation. A hen's egg, for instance, can be kept alive in an incubator and by making a suitable window in the shell we can see something of the embryonic processes which are proceeding. If we do this we find that when the amniotic investment has been completed the embryo begins to carry out a series of rocking

movements, the result of which seems to be the enlargement of the amniotic space so that the amnion no longer clings closely to the embryo, just as the tail movements of the goldfish embryo distend its egg-membrane. We are fully entitled, I think, to assume that similar movements are carried out by the developing mammal and lead to similar result.

There is a type of mental defective termed the Mongolian idiot which crops up repeatedly amongst the offspring of our slum population. These unfortunates have received their name from the superficial resemblance which they exhibit to typical members of the Mongolian race. Now, it has been pointed out that the skull of the "Mongolian" exhibits a characteristic shortening, like that of the "fancy" goldfish, and that this shortening may plausibly be attributed to excessive pressure during the early stages of growth from a too closely adherent amnion.

Before we pursue the question of how far other human abnormalities may be due to germ-weakening, we must touch on the question as to whether a germ once weakened is irretrievably damaged. According to Tornier, if the animal which has developed from such a germ be removed to healthy surroundings a certain amount of recovery is possible amongst a certain proportion of the offspring, and this recovery takes the form of a reversion to type. Thus, the fancy strains of goldfish, when bred from, always give rise to a certain proportion of offspring approximating more or less closely in resemblance to the normal type, and it is for this reason that, in perpetuating these breeds, such rigorous selection is called for. Morgan, who has bred hundreds of thousands of the banana fly, *Drosophila melanogaster*, and who has discovered hundreds of "mutations," has recorded several cases when frequent "mutations backwards" occur. These "mutations backwards" are returns to the normal. In the case of our domestic plants such "throw-backs" towards the wild ancestor are known as "rogues," and are ruthlessly hoed out.

Several cases are known where domestic animals have become feral—that is, have run wild. In all these cases—such as those of the mustang ponies of North America and the dingoes of Australia—there has been a marked approximation in size, proportions, and colour of the feral race to the wild ancestor. It is often casually assumed that this is due to the natural selection of chance variations in the direction of the normal; it seems a great deal more probable that not chance but germ-recovery has been the cause of the reversion.

Now, quite independently of Tornier and without apparently any knowledge of his work, another writer has arrived at very similar conclusions; this is Dr. Murk Jansen, who has

examined human abnormalities. What Tornier calls "germ-weakening" Jansen calls "feebleness of growth." He lays down three principles, viz. (1) feebleness of growth is proportional to the intensity of the nocivity (noxious agent); (2) feebleness of growth is proportional (in the damage caused) to the rapidity of the growth of the individual and of its parts, and (3) feebleness of growth is characterised by enhanced irritability and enhanced fatigability. Jansen points out that when we examine the physical condition of the members of large families who have been born at short intervals from the same mother, we find, as we proceed from the elder to the younger, increasing signs of "feebleness of growth." Not only does the weight fall below that proper to the age of the child, but so also does the height; the abdomen tends to be protuberant, not owing to the deposition of fat, but owing to the flaccidity and feebleness of the muscles. The symptom of "knock-knees" also makes its appearance. This is due to a lessened growth of the cartilages tipping the bones which take part in forming the knee-joint and which have to bear the pressure of the weight of the body. In the normal child these cartilages respond to the growing pressure by enhanced growth, but in the germ-weakened child diminished growth occurs and the leg becomes crooked.

In several such families examined by Jansen there is a break in the continuity of progressive degeneration as we approach the youngest child. Two causes have been detected for this. In one case two children in the series died in infancy; their early deaths relieved the mother of the labour of nursing and rearing them through their first year of life, and she thus recovered strength to meet the demands of the pregnancy which resulted in the birth of the next surviving child. In another case the mother was attacked by appendicitis in the sixth month of her pregnancy and had to remain in bed for six weeks. As a result of this rest the child when born exhibited little feebleness of growth, and was, in fact, nearly normal.

Rickets is regarded by Jansen as an extreme instance of feebleness of growth. He divides the degrees of feebleness of growth into three stages, which he defines thus: (1) the type with weakness of muscle and enhanced body-height (outgrowing strength), for, as he points out, the muscles are the tissue which normally show the greatest increase in bulk; (2) the type of average height with muscle-weakness and knock-knees; (3) the type of diminished height and thickened, curved cartilages. This last grade is marked by general diminished energy; the child does not walk at the proper age, sometimes not until it is five years old. The thickened cartilage does not imply increased growth of that tissue, but

delay and impeding of the process of ossification which converts this cartilage into bone. In fact, in the development of this cartilage there are three factors, viz. (*a*) cell division, (*b*) cell enlargement, and (*c*) cell transformation. The last factor is the one that is most susceptible, so that cell-growth and cell-enlargement continue when cell-transformation has been entirely checked, and so the cartilage increases in thickness.

The production of new genital cells goes on throughout life in the male, but ceases at birth in the female. Hence noxious agents have a special influence on these cells, and Jansen attributes a considerable amount of the slighter degrees of feebleness of growth observable among the children of the families of North European peoples to over-indulgence in tobacco on the part of their fathers. Jansen now leaves the consideration of general germ-weakening which affects the growth of the whole body in greater or less degree and proceeds to consider germ-weakening in the early stages of development, which, as in the case of gold fish, may affect particular organs of the body by causing local pressure. He distinguishes in this process the following factors, viz.: (1) Ischæmia, *i.e.* lessened blood supply to the developing part; (2) dwarfing or death of the affected part in consequence of diminished blood supply; (3) the most marked dwarfing is seen in the parts which grow fastest.

Now, the cavities in the growing embryo are principally two, viz. the general cavity of the swelling egg or blastodermic vesicle, which may be termed the chorionic cavity and the amniotic cavity which lies between the amnion and the embryo. The embryo, invested by the amnion, hangs as it were down into the chorionic cavity, and increase of pressure in this would act on the body as a whole. Hence the pressure which produces local malformations must proceed from the amnion, and Jansen goes on to show that according to the time at which that pressure acts is determined the kind and extent of injury produced. He gives reasons for supposing that the amniotic pressure may only last for a short time, but that this brief duration is sufficient to cause permanent injury to the embryo. After an interval the amnion responds to the stress of the growing tissues by yielding and the pressure passes off. Thus, to pressure in the third week of fœtal life Jansen attributes "anencephaly," *i.e.* the complete absence of a brain, which sometimes occurs in monstrous infants. To pressure at the same period but of lesser extent may be perhaps attributed "microcephalous idiocy," in which the brain and skull are abnormally small. We have already seen that "Mongoloid idiocy" is due to pressure; and Jansen estimates the age at which it takes place at about eight weeks. The

resemblance to the Mongolian skull which the name implies is enhanced by the breadth and flatness of the nose. This is due, as Jansen shows, to the disappearance of the nasal bones owing to antero-posterior pressure and their compensatory replacement by processes from the frontal bones. Still later the limbs begin to grow in length and amniotic pressure at this period impedes their growth, so that when ossification begins the epiphysis very early unites with the long bone and short legs and arms—in a word, dwarfism results. This, termed "achondroplasia," is almost always accompanied by curvature of the spine, a phenomenon which bears direct testimony to the existence of unusual pressure at the time when the deformity takes its origin. After the limbs have elongated almost to the full length which they attain in the embryonic condition amniotic pressure may still occur. In this case it affects the growths of the fingers and toes, causing them to assume a peculiar blunt shape, but this modification is often accompanied by other deformities. Thus, the shoulders may be pressed forwards, the clavicles buckled up, and their growth so impeded that they may be wholly resorbed or disappear, and the sides of the chest may be flattened. These abnormalities are spoken of as "Cleedo-dysostoses"; pressure at a still later period may cause infantile dislocation of the hip joint and club foot.

So far we have spoken of the results of direct amniotic pressure; but, as in goldfish, so in man, the germ-weakening may lead to a distension of the natural cavities with water. Thus, side by side with the microcephalous idiot in which brain and skull are stunted, we find the so-called hydrocephalous forms with enormous skull and brains which are superficially large but which prove on examination to owe all their size to the accumulation of fluid within the brain cavity. In extreme cases the hemispheres of the brain are reduced to thin shells and the characteristic layers of nerve-cells are suppressed. There is, however, another type of defective with a large head —but in this type the brain is apparently large and solid, nevertheless microscopic examination shows that it is made up chiefly of supporting (glia) cells which have increased enormously in amount, whilst the number of nerve-cells has remained small. In this case the weakness of growth has affected different groups of cells in different degrees.

If we now endeavour to sum up the results of our survey of the causes of defect both mental and bodily in man and other animals, we find that they can all be subsumed under the general heading of Germ-weakening, and the most important thing about this weakness is that, once produced, it tends to be hereditary. Hence it is a matter of the first moment to

ascertain the causes of this germ-weakness, and these causes resolve themselves into injury or suffocation of the germ in its earliest period of development, which impedes its power of growth and affects the different tissues into which it develops in different degrees, so that the harmonious development of the various organs is disturbed. When we, however, push our enquiries still further back and seek to find out how the primary injury is brought about, then we must differentiate between animals which lay their eggs, like fishes and amphibia, and those which, like mammals, carry them in the womb.

In the oviparous forms want of oxygen in the surrounding medium is the most potent cause so far as has been yet ascertained, but in a mammal the surrounding medium is the mother's blood, and it is to alterations in this that we must look for the principal causes of human germ-weakness. It is true that Tornier suggests that, in the case of our domestic animals, their dark and insanitary surroundings may have a similar effect on the germ-cells as is produced by the filthy water in which the Chinese keep their goldfish; but Jansen's work suggests that the blood supply of the mother is also prejudicially affected by too rapidly recurring pregnancies and by too much work during pregnancy. Apparently her blood becomes overloaded with toxic substances, due to fatigue. If this be so, we arrive at the conclusion that the causes of overpopulation and the causes of the production of a C_3 population weak in body and mind are one and the same, and may be reduced to one, namely, RECKLESS REPRODUCTION, and for this there is one remedy and one only—rigid birth-control and eventual sterilisation of the unfit. The delusion of the nineteenth century was to place all the blame for the imperfections of our slum population on their surroundings. Critical research does not bear out this conclusion. Drugs may be contributory causes—thus, as Jansen suggests, over-smoking may be responsible for the lighter varieties of feebleness of growth; but alcohol, even in excess, does not seem to be a potent cause of that particular type of degenerate known as the mental defective. It is known from the experiments of Stockard that guinea-pigs exposed continuously to the fumes of alcohol produced a weakened and stunted offspring, defective in eyes and brain, which in three or four generations dies out; but Pearl found that the domestic fowl similarly treated did not yield a similar result. In this case fewer and more vigorous offspring are produced than in normal fowls. Pearl explains this anomaly by supposing that the alcohol taken into the parent bird's system kills the weaker germ-cells, and the more vigorous germ-cells, relieved of the competition of their feebler brethren, grow

correspondingly better. It is therefore worthy of note that, although Goddard could find no evidence in favour of the production of mental defect by the drunkenness of the parent, he did find strong evidence that alcoholism tended to increase the number of miscarriages and of deaths in infancy, and also, when taken in large excess, to produce offspring of weakened constitution and mental instability. Now these researches of Pearl suggest another and still more disturbing question, viz. is not one of the potent causes of a C_3 population the absence of a rigorous natural selection?

Bethnal Green is at present the home of one of the poorest populations in London. We possess an account of its condition eighty years ago when it first became a town. Then, as now, it was the home of a working-class population, but its sanitary condition was frightful. There were no drains, and the only supply of water was from wells. All rubbish and manure was simply flung into the streets and was raked together into dung-heaps, which were cleared away at long intervals by an utterly insufficient band of scavengers mostly consisting of old men. There were large families and an appalling death-rate, yet the survivors constituted a healthy population of tremendous vigour. During the nineteenth century a pure water-supply and proper drainage were introduced, yet the surviving population is not as healthy now as it was then, and the proportion of paupers is just as high. The efforts of emotional philanthropists to keep all babies—however feeble—alive at the public expense seem to have undermined the general health of the population, whilst they have enormously added to the taxes and indirectly helped to reduce the birth-rate of the more valuable stocks from the higher strata of society.

It seems, then, that we are faced with two alternatives, either we must revert to a policy of *laissez faire*, emphasise parental responsibility and allow the seed of the unworthy to die out, or else, if we are determined to provide free meals, free schooling, and outdoor relief, and still wish to avoid the appearance of a C_3 population, then we must take means to properly space the births and diminish their number, whilst sterilisation must be resorted to in the case of those people whose lack of responsibility makes them impervious to public opinion. I know that these are unpopular conclusions, but they are deductions which seem to me to be forced on every scientific man who considers the facts in a dispassionate manner. Is it not self-evident that if we eliminate natural selection—nature's broom, by means of which she keeps other types of life healthy and clean—we must replace it by an equally efficient broom of our own? Till we do so I feel sure that we shall be continually encumbered by the problem of a C_3 population.

POPULAR SCIENCE

STEPHEN GRAY (1696—1736)

An Early Electrical Experimenter

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An historian, in chronicling the events of the past, has two primary duties to attend to if his record is to be of any intrinsic and lasting value. He must, in the first place, attain an indisputable accuracy in the events of the period with which he is dealing, and secondly, and more especially in records of a general nature, an historical writer should endeavour to regard in a true perspective the period which comprises his studies. Again, an historian should not neglect to take into account the extent to which, in the inexorable balance of Time, the events with which he is concerned are related to the occurrences of earlier and later dates.

More especially is it wise for these precautions to be observed in the recording of the history of any particular branch of science. At the present day, the trend of scientific thought and invention is very evident, but during the past, a step forward in the advance of science has seldom been made with any startling suddenness. Leading up to each discovery in the domain of natural philosophy may be traced threads of half-conscious thought, apparently disconnected, yet upon closer examination proving to be intimately related to one another, and which in the course of time have combined together to make the discovery of the principle upon which the new invention is based an almost inevitable occurrence. Gossamer, and apparently broken and interrupted threads these thought-filaments may be, yet they represent the continued, if somewhat puny, efforts of the human mind to attain the summit of the Parnassus of Truth. Advances in science and the discovery of new principles in natural philosophy have not in the past been made by individuals who were actuated by the commercial instincts of gain. The intellectual honours which

proceed from a new revelation of the workings of Nature's forces have generally accrued only to a small section of humanity, striving with the tools of its own forging to unravel the mysteries and to penetrate the gloom with which the natural laws have been enshrouded.

Nowhere more than in the science of electricity will the above observations be found to possess a greater truthfulness. The small isolated band of pioneers who, by rubbing various substances together, generated small amounts of frictional electricity and attempted to investigate its origin and nature, dreamt but faintly of the possibility of applying the newly discovered force to turn the wheels of industry and to perform the everyday work of the world. To these early experimenters the manifestations of static electricity, the phenomenon of the "electric fire," and the wonders of electrical energy were merely intellectual curiosities which were regarded in almost the same light as the mathematical demonstration of the existence of the Fourth Dimension is at the present day.

Like other natural philosophers, the early investigators of electricity were generally individuals who, freed from the anxiety and mundane cares of everyday life, and possessing a tolerably fair amount of this world's goods, turned to the investigation of the problems of the newly awakening science in order to satisfy an inherent intellectual craving, as well as to relieve themselves of the utter stagnancy and boredom which must have resulted from an over-indulgence in the pastimes and recreations which usually attracted the attentions of the nobility and country gentlemen of the seventeenth and eighteenth centuries. Not, of course, that this motive can be considered to be the only one which turned the attention of intellectual and leisured men to the investigation of experimental philosophy. A natural curiosity and an inherent desire for knowledge made up the chief source of energy which enabled them to overcome the tremendous difficulties which beset the path of the early philosophic investigator. But at that period of the world's social history it was almost impossible, in England, at any rate, to devote one's life to the patient, toilsome, and wholly unrewarded pursuit of scientific knowledge without being possessed of a sufficient monetary competence, in addition to an unlimited amount of "philosophic leisure" and freedom from the everyday cares of the commercial world. Hence we find that the greater part of the early workers in natural philosophy and experimental science came from a noble stock, or belonged to the social class known at that period as the "country gentleman" section of the community, and that they were permanently endowed with a sufficiently ample amount of this world's goods to allow them to indulge, almost

to their fullest extent, their intellectual and philosophical desires.

It is customary to commence a biographical notice at the beginning of a man's life, by duly recording, as a matter of fundamental importance, the exact date of his introduction to the scene of his terrestrial labours. Let us, however, in the case of Stephen Gray, reverse the order of things and state in all preciseness the date of his death. That event, which occurred during the fortieth year of his life, took place on the evening of the 15th of February 1736, and by it science lost, at a too early age, a devotee of extraordinary alertness and intellectual clarity.

Little is known of the life of Stephen Gray. Indeed, the date of his birth is not known with any great amount of certainty. That he was born in the year 1696 appears to be without a doubt, but the exact day and month of that year on which the event occurred is apparently unascertainable. However, as the obscurity in this direction does not influence the future works of the man, we shall not deem it a matter of importance.

Born into an age which witnessed the dawn of rising inquiries and activities in all directions, Stephen Gray was apparently a native of Canterbury, and in all probability he received the rudiments of a classical education in the neighbourhood of that city. His profession, however, before he took up the study of electrical phenomena is quite unknown. Now and again, one may obtain glimpses of the early life and habits of the philosopher from occasional remarks which he makes in the papers published in the *Philosophical Transactions* about his former residence at Canterbury, and the astronomical observations and calculations which he made there. The same papers sometimes allow us to obtain fleeting glances of the relations which existed between Gray and his philosophical friends, and more particularly of the numerous visits which he made to the house of his able co-worker in electrical science —Dr. Granville Wheeler.

Gray has been referred to as the "Father of Electrical Science," a designation which he justly deserves. His division of material substances into "electrics" and "non-electrics," and the pioneer work which he carried out on the subject of electrical conduction and insulation, are factors which aided to the greatest degree the work of his followers, and which eventually culminated in the discovery of the Leyden jar, as well as enabling the true nature of lightning and atmospheric electricity to be fully understood.

Before going into the subject-matter of Gray's experiments in more detail, let us for a moment consider the state of

electrical knowledge which prevailed immediately before his discoveries. The property possessed by amber of attracting light bodies when rubbed was very well known to the ancients, but it was not until the time of Elizabeth of England that a strict inquiry was directed into the nature of the peculiar phenomenon. William Gilbert, a Physician-in-Waiting to the Queen, published at the beginning of the seventeenth century a treatise[1] on the subject of Natural Magnetism and the property of various bodies of taking upon themselves an attractive virtue when rubbed. Such bodies were termed by Gilbert "electrics," an appellation derived from the Greek word for amber.

Gilbert undoubtedly awakened an interest in the investigation of these phenomena, but it was half a century before his introductory work on the subject of electrics began to be followed by further investigations. Newton and Boyle were the next workers who are worthy of any mention. They performed a few isolated experiments on the subject of frictional electricity and on the peculiar "luminosity" which proceeded from certain gems when they were rubbed in the dark. To Otto von Guericke, the ingenious Burgomaster of Magdeburg, inventor of the air-pump and of several other scientific devices, must go the honour of being the first to construct an electrical machine. Guericke conceived the idea of whirling a ball of sulphur upon its axis by means of a suitable mechanical contrivance, and of collecting electricity from it by the aid of the hand or a silken pad. Newton is generally said to have followed the Burgomaster's example, and to have improved the machine by employing glass spheres instead of balls of sulphur. But the creative force of the great English philosopher was destined to expend itself in other and more comprehensive generalisations, and it was left to a much lesser light in the realms of natural philosophy to construct the first efficient instrument for generating frictional electricity. Hauksbee, for such was his name, devised an arrangement for rapidly rotating glass spheres upon their central axes, and the frictional electricity was generated by the contact of the hand or of other suitable bodies with the revolving spheres.

Such was the state of affairs which prevailed before Stephen Gray came into prominence. Hauksbee had risen to the zenith of his fame as an "electric philosopher"—some called him a wizard. He had collected considerable quantities of frictional electricity with the aid of his new machine, and had performed a few experiments of a random nature in an endeavour to determine the properties of the electric force.

Hauksbee was no theoretical reasoner, and the same

[1] *De Magnete*, 1600.

description applies, but to a lesser extent, to Stephen Gray. Gray began his investigations into the subject of electricity by a recapitulation of the earlier work of Gilbert, Boyle, and others, and almost immediately came his first generalisation. Finding that many substances such as hair, feathers, glass, sulphur, etc., could be highly electrified by rubbing, whilst, on the other hand, bodies of a metallic nature could never be imbued with the electrical influence, his alert intelligence at once differentiated between these two classes of bodies, and he accordingly divided up all material substances into *electrics* and *non-electrics*.

As a source of electrical energy Gray employed a large glass tube, 3½ feet in length and over an inch in diameter. This he closed at both ends with corks "to keep out the dust." The tube was electrically excited by rubbing with a feather or with a pad composed of a suitable non-conducting material. During the course of the investigations in connection with the electrical excitation of the tube, Gray was amazed to discover that not only would the glass walls of the tube strongly attract light objects, such as leaf metal, when they were placed in the immediate vicinity, but that the light objects were attracted to the cork and also to an ivory ball which, in subsequent experiments, was attached to the end of a 4-foot rod inserted into the tube through the cork. The conductable nature of the electric force thus became readily apparent to Gray's mind. He suspended the ivory ball from the cork, which was firmly fixed into the mouth of the tube, by means of a long line of "pack-thread"—a coarse hempen fibre—measuring 26 feet from end to end, and on rubbing the glass tube whilst standing on the balcony of his residence he found pieces of leaf brass to be strongly attracted by the ivory ball which hung at the other end of the line in the area below.

Having thus conveyed electricity perpendicularly, the next problem was to carry it in a horizontal direction. However, the achievement of this feat proved to be more difficult than Gray had at first suspected, for on suspending the line of pack-thread from nails driven into the beams of the ceilings, and upon exciting the glass tube by friction, the leaf brass failed under any circumstances to be attracted to the ivory ball which was suspended a short distance above it. After conferring with his philosophical friend, Granville Wheeler—whose biographical details, incidentally, are even more scanty than those of Gray—the following experiments were carried out at Wheeler's residence at Otterden Place during the summer of 1729.

Together with Wheeler, Gray had grasped the important fact that the failure of his attempt to carry the electrical

attraction in a horizontal direction had been caused by the escape of the electricity into the wooden beams of the room at the points where the thick pack-thread was suspended on the iron nails. Wheeler had suggested that the electrical leakage might be *lessened* to some extent by the use of threads of a thinner material, and eventually his suggestion was put into action. On the 2nd of July 1729, "about ten in the morning," the electrical attractive force was successfully conveyed along a line of pack-thread, 80 feet in length, which was allowed to rest upon a series of silken lines placed at right angles to it, and attached to the sides of the room.[1] Upon the glass tube being excited, the leaf brass was immediately attracted to the ivory ball attached to the other end of the line.

The experimenters subsequently attempted to double the length of the line through which the attracted force was to be conveyed by arranging for its return to the neighbourhood of the exciting tube after being led to the far end of the room. Under these conditions, although the glass tube and the ball of ivory were only separated from each other by a short distance, the line of pack-thread which connected them together ran from the other end of the room and back, being supported at intervals upon cross-lines of silk. But thin silk supports were unable to bear the additional weight of the increased length of pack-thread, and they broke under the strain of it almost before the experiment had been commenced. Thin brass wires were therefore used in place of the silken threads, but all attempts to carry the electric influence through lines of pack-thread resting upon these metallic supports failed signally. However, the continued lack of success in these attempts brought to Gray's mind a true understanding of the cause of the failure, and allowed him to grasp fully the fundamental principles of electrical conduction and insulation. The experiments were unsuccessful, Gray reasoned, because the electrical influence from the excited glass tube passed off from the pack-thread at the points where it rested on the supporting brass wires. The lines supporting the thread through which the electrical attractive force is to be conveyed must be composed of a material which is itself an "electric," or to use the modern phraseology, must be of a non-conductable nature.

Thus, by a process of logical reasoning did Gray successfully master the principles of electrical conduction. By strengthening the silken supports of the pack-thread cable he successfully carried the "electric virtue" for many hundreds

[1] Or, rather, the *gallery*—a narrow passage which ran from end to end of the upper stories of many houses of the period.

of feet in a horizontal direction. As a child delights in the manipulation of a new toy, and as an ardent experimenter revels in the fascinations of a new discovery, Gray for a period following the experiment described above devoted his entire energies to fanciful and spectacular experiments in electrical conduction. Up and down the length of the rooms of his residence he carried the electric force through pack-thread cables resting on insulated cross-supports of strong silken lines. Out of the window, down into the garden, and even sometimes as far as the neighbouring fields, ran the electrical lines of this early investigator, and without a doubt to the astonishment of his fellow-creatures. Had Gray realised that the pack-thread line was enabled to convey the electrical influence solely on account of the moisture which it contained, and that had it been perfectly dry his experiments would have resulted in failure, he might have discovered further facts concerning the conduction of electricity which, as it was, were left for succeeding investigators to find out.

However, Gray was not content to remain satisfied for very long with even the great advance in electrical science which he had made. He showed that an electrical attractive force could be communicated to the line of pack-thread even without any actual contact of the glass exciting tube. Here was the great principle of electro-static induction discovered by Stephen Gray in 1729,[1] and allowed to remain unnoticed by many of the subsequent workers in electrical matters.

The experiments of Gray which attracted greatly the popular and scientific notice of the times were those by which he demonstrated the electrifiable nature and conducting properties of the human body. Like the celebrated hero of remote antiquity who first consumed an oyster, Gray would no doubt have had to display considerable daring at passing, for the first time, an electrical charge through the body of a human being. Hence an individual of only a menial status was enlisted in the cause of science. His own footboy was chosen for that honour. This youth, "a good stout lad," was suspended horizontally on strong silk lines, and the "electrical virtue" from the exciting tube was allowed to flow into his body. The electrical attractive force was passed from his head down to his feet, and also in the reverse direction. It was found, under these conditions of insulation, that when the electrified tube was presented to the soles of his feet, a thin piece of leaf brass resting on a small stand placed a few inches below the boy's face was immediately attracted and repelled

[1] *Phil. Trans.*, 1731, vol. xxvii, p. 81.

by his cheek. Thus the electrification of the human body was accomplished, and it gave rise to a good deal of speculation in intellectual circles, while the unlearned of the period indulged in wild guesses as to the nature of Gray's compact with the Evil One.

The electrical experiments of Gray had now attracted the fullest attentions of men who were interested in the new science, and especially on the Continent did Gray's investigation strike the imaginations of the rising men of science. Du Fay, a French experimenter, repeated much of the work of the English electrician, and in very many respects fully confirmed the original observations of Gray.

The success of the experiments in electrical conduction led Gray to the investigation of the conductive properties of other substances besides linen threads. He discovered, for instance, that the "electric effluvia" could be equally well transmitted through carefully insulated metal rods as it could be through lines of pack-thread, and that the attractive force which was given to a metal rod by the act of rubbing could even be made to show itself in another insulated rod, one end of which was separated by a distance of a few inches from the extremity of the first rod. Gray had again unheedingly discovered the principle of electro-static induction.

The phenomenon of a peculiar luminescence which was often produced by the attrition of certain bodies in the dark, and which was thought to be of an electrical nature, next attracted Gray's attention. Pointed metal rods, when rubbed in a darkened room, were found to give off a peculiar light, which "extended itself in the form of a cone whose vertex was at the end of the rod." Thus, says Priestley in his *History of Electricity*,[1] "This was the first time that this phenomenon, which is now so common, was distinctly seen." Gray was even able to astonish his onlookers by drawing an electric spark from the surface of water. A drinking-glass, filled to the brim with water, was allowed to rest upon an insulating cake of rosin and was electrically charged by means of the excited glass tube. On applying a finger a little distance above the water, the liquid rose in the form of a conical protuberance, and after a spark had passed between it and the finger of the experimenter, the cone of water subsided, the surface of the water being disturbed by a series of ripples which were formed upon it. "And," observes Gray,[2] "although these effects are at present but *in minimis*, it is probable in time there may be found out a way to collect a greater

[1] Third Edition, 1775, p. 71.
[2] *Phil. Trans.*, 1735, vol. xxxix, p. 166.

quantity of it; and so consequently to increase the force of this electric fire, which by several of these experiments seems to be of the same nature with that of Thunder and Lightening."

Here we may obtain an insight into the theoretical reasonings of Gray and his shrewd predilections as to the probable developments which would take place in electrical science. The observation above quoted is, in fact, the first definite postulation of the identical nature of the electric spark and lightning. How well Gray's prediction was confirmed a few decades afterwards by Franklin, and how, at an even earlier date, Musschenbroek gave to the world the Leyden jar as a method of storing electrical energy, are incidents in the history of science which are too well known to warrant repetition.

Towards the end of his life, Gray considered that to a certain extent he had even mastered the problem of electrical storage himself. He was able to maintain spheres and conical-shaped masses of sulphur in an electrified state for weeks and even months together by placing them on insulating stands and covering them with dry glass vessels, or by wrapping them up in blue worsted stockings. The colour of the insulating stockings had to be blue, for Gray believed, and indeed performed a series of experiments in an attempt to show, that the colour of a body affected its insulating properties. As a result of these experiments upon the capacity of bodies for storing up frictional electricity, Gray devised a theory which, with a little imagination, may be looked upon as containing the first glimmerings of an electrical theory of matter.

He concluded that because of the success of the experiments, "we have some reason to believe that we have discovered that there is a *perpetual attractive force* in all electric bodies without exciting by either rubbing, heating, etc., or any other attrition." [1]

So much for his experiments. Of the man Stephen Gray very little is known, and no biographical account of his life has been attempted. Indeed Thomson, the historian of the Royal Society, comments on the extreme paucity of detailed particulars of the life of the man "to whom electricity lies under such obligations" as remarkable.[2] Priestley in his *History of Electricity* gives an account of the electrical experiments of Gray, but beyond observing that the individual in question was a pensioner at the Charter House, he omits any semblance of detailed particulars of the habits and customs

[1] *Phil. Trans.*, 1732, vol. xxxvii, p. 397.
[2] Thomas Thomson, *History of the Royal Society*, 1812.

of the philosopher. The same author, however, assures us that "no person who ever applied to this study [electricity] was more assiduous in making experiments or had his heart more entirely in his work." [1]

Rutter, a somewhat obscure writer and experimentalist of the middle of the last century, laments upon the fact that the personal details of the life and private circumstances of Stephen Gray have been allowed to sink, almost beyond recall, into the abyss of oblivion. "Of this remarkable man," he writes, "very little is known. The date and place of his birth I have not been able to discover, nor can I find any record of what was his occupation or profession previous to his becoming a resident in the Charter House." [2]

That Gray lived in tolerably affluent circumstances is fairly certain. Before taking up his residence at the Charter House and devoting almost his entire energies to electrical researches, he resided at Canterbury, the city which was very probably his birthplace, and which saw much of his earlier life. In some of his papers in the *Philosophical Transactions*, Gray refers to his house at Canterbury, and to the astronomical observations which it was his custom to make from the window of his room. Besides these observations, Gray was also greatly interested in microscopy, and he invented a peculiar form of microscope in which a drop of water was made to act as a lens by virtue of its convex surface. The investigations which Gray subsequently made with this primitive instrument are duly recorded in his papers in the early volumes of the *Philosophical Transactions.*

From some observations made by Desaguliers, an experimenter who took up the study of electrical phenomena with great success, it would appear that Gray was a man of great exactitude and precision, and by no means amiable in disposition. In a paper read before the Royal Society in 1739,[3] Desaguliers makes the following remarks, which are worth while quoting in full:

"I have hitherto avoided entertaining the Society on this subject, or pursuing it so far as I might have done, (considering that I can excite as strong an electricity in glass, by rubbing it with my hand, as anybody can) because I was unwilling to interfere with the late Mr. Stephen Gray who had wholly turned his thoughts that way; but was of a temper to give it entirely over if he imagined that anything was done in opposition to him."

[1] *Op. cit.*, p. 32.
[2] J. O. N. Rutter, *Human Electricity*, 1854, Appendix xxiv.
[3] *Phil. Trans.*, Abridged, vol. viii, pt. 2, p. 419.

Gray's work, however, was appreciated by his contemporaries, and he was elected to the Fellowship of the Royal Society in 1732, being formally admitted into the Society on the 15th of March of the same year. Du Fay, the French electrician, more especially realised the value of Gray's experiments, and he applied the principles upon which they were based for the purpose of further discoveries, subsequently communicating the results of his investigations to the English experimenter, who, for once in a way, received them in good favour.

It is somewhat remarkable that Gray and his co-workers, Wheeler and Godfrey, did not employ rotating glass spheres as a source of frictional electricity. Throughout the whole course of their investigations the only method of electrification which they made use of consisted of the tedious and rather prosaic process of rubbing a long glass tube with the hand or with some other suitable non-conducting material. Had the workers employed in their experiments the rotating glass sphere of Hauksbee, they might have stumbled against the discoveries which, as it happened, were left for succeeding experimenters to make. But Gray was apparently satisfied with the use of the glass tube, and it never seems to have occurred to him that a more reliable and efficient generator of the electric force was easily within his reach.

Stephen Gray died suddenly, and at a comparatively early age. The cause of his death is now unknown and perhaps will remain so for ever. Nevertheless, on his deathbed, Gray dictated to Cromwell Mortimer, M.D., the secretary of the Royal Society, particulars of some experiments which he had carried out respecting the motions of small bodies. Here, however, the dying man had been deceived into thinking that the force which caused the revolutions which were seemingly performed by light bodies suspended by a thread, the end of which was held between the fingers and thumb of the experimenter, was analogous to the energy which is responsible for the revolutions of the heavenly bodies in well-defined orbits, and that a study of the subject would, indeed, eventually disclose a universal electro-gravitational law. He communicated the particulars of his experiments to Mortimer with great preciseness, and gave instructions as to the way in which further experiments were to be carried out, even going as far as to predict the result of such investigations. The following day he died, and thus passed from the sight of men, leaving a great reputation of being a singularly learned gentleman and a daring experimenter.

But Gray's last effort in the cause of experimental science was founded upon a deception. The familiar conjuring trick

of the Goblet and the Ring, in which a metal ring suspended at the end of a thread held between the fingers of the operator is made to perform revolutions, and to strike the walls of a glass vessel placed in its vicinity, is as old as the proverbial hills. The first description of the phenomenon to be written in the English language appears in Scott's *Discovery of Witchcraft*, 1665, in which work the author maintains its cause to be due to magical influences. It may be almost unnecessary to remark here that the rotations and oscillations of the ring pendulum have their origin in psycho-physiological causes which produce a series of unconscious muscular movements in the hand of the operator, and so direct and amplify the movements of the dependent body. Rutter,[1] however, falls into the same deception in considering the motions of the pendulum to be due to electrical causes, and he even goes so far as to devise an instrument for the purpose of studying the varying nature of the oscillations.

Mortimer, however, after Gray's decease, continued the experiments, particulars of which he subsequently communicated to the Royal Society.[2] They were entirely negative in results, and proving contrary to Gray's predictions, they were soon discontinued.

The first epoch in the history of electricity may be said to have ended with the work of Hauksbee on the electrification of bodies. Gray's experiments brought a new outlook upon the science and aroused a more widespread interest in it. The manifestations of frictional electricity ceased to be regarded as mere speculative concerns, intellectual curiosities, and the like. Gray had demonstrated the controllable nature of the electric force, and, still further, he had even given faint hopes that at some future date it might be harnessed for the benefit of mankind. He blazed a trail through the virgin forest of natural philosophy, and by his crude experiments he laid open the path to future discoveries.

Gray was essentially a pioneer, and he has suffered the pioneer's fate. It is to be regretted that the memory of such an experimenter has been allowed to remain in an almost complete obscurity. Such has been the case, however, but it is hoped that this inadequate and necessarily incomplete biography will arouse an interest in the subject. Stephen Gray was more than a mere dabbler, impelled out of curiosity and by reason of much leisure to investigate natural phenomena. He was an earnest worker in the philosophic cause, and to him much is due as an intrepid experimenter, and as the "Father of Electrical Science."

[1] *Op. cit.*
[2] *Phil. Trans.*, Abridged, vol. viii, pt. 2, p. 405.

APPENDIX

A LIST OF PAPERS ON ELECTRICAL SUBJECTS CONTRIBUTED TO THE *Philosophical Transactions* OF THE ROYAL SOCIETY by STEPHEN GRAY, ESQ., F.R.S.

New Electrical Experiments. Vol. xxxi, p. 104, 1720.
More Experiments concerning Electricity. Vol. xxxvii, p. 81, 1731.
Concerning the Electricity of Water. Vol. xxxvii, p. 227, 1731.
Further Experiments concerning Electricity. Vol. xxxvii, p. 397, 1732.
Experiments and Observations upon the Light that is Produced by Communicating Electrical Attraction to Animal or Inanimate Bodies, together with some of its Most Surprising Effects. Vol. xxxix, p. 16, 1734–5.
Some Experiments Relating to Electricity. Vol. xxxix, p. 166, 1734–5.
Electrical Experiments taken from the Mouth of Mr. S. Gray, F.R.S., by C. Mortimer, M.D., F.R.S., on Feb. 14th, being the day before he died. Vol. xxxiv, p. 400, 1736.

NOTES

DURING the war the length of each number of SCIENCE PROGRESS was reduced from 192 pages to 176 pages, though the matter actually contained was not much diminished owing to the fact that more of it was then put into small print. It has now been decided to return to the original length, namely, 192 pages; and we hope that this extension will be welcomed by our readers. But, in consequence of the great rise in the price of printing, paper, and binding, it will be necessary to increase the price to 7*s.* 6*d.* net (postage extra 3½*d.*): Annual Subscription: 30*s.* (postage extra 1*s.* 2*d.*).

Arthur Gordon Thacker, A.R.C.Sc. (Hugh Scott, University Museum of Zoology, Cambridge).

BY the early death of Mr. A. G. Thacker on March 30, 1924, zoology has lost a hard worker and a man possessed of shrewd judgment, wide interests, and good literary style. A scientific trend had previously manifested itself once at least in his family, his maternal grandfather, Edmund A. Kirby, M.D., having been the author of certain works on pharmacology.[1] Thacker was born on January 5, 1885, and from early boyhood displayed a strong bent for natural history. He soon struck out an original line, showing himself less eager to make such collections as many boys form than to record observations. He was keeping regular field notes when he was fourteen, and, though interested in all kinds of animal life, was most attracted by vertebrates, especially birds, and, above all, mammals. When still only in his fifteenth year he visited the late Prof. G. B. Howes at the Royal College of Science, and aroused such interest in the Professor, that the latter arranged for him to attend the biological classes during a whole session without having formally entered the College. In 1900 Thacker temporarily left that institution and continued his general education, subsequently re-entering the College as a normal student in October 1902; there he followed the courses till he gained the Associateship in 1906. He then held a Research Scholarship till 1908, examining the Calcareous Sponges collected by Mr.

[1] "The Administration of Phosphorus and its Therapeutic Uses," 1877, and other writings.

Crossland in the Cape Verde Islands; a report on these appeared in June 1908 (*Proc. Zool. Soc.*, p. 757) and Thacker's slide-preparations are now in the British Museum. The knowledge of sponges then acquired was again called into play later, when for some time before his death he was engaged in editing, in collaboration with Dr. G. P. Bidder, a Naples Monograph on that group left unpublished by the late Prof. G. C. J. Vosmaer of Leyden.

From 1910 to 1916 Thacker was Curator of the Public Museum at Gloucester, where he reorganised much of the collections, setting up exhibits with explanatory labels, and where he also gave public lectures. Leaving early in 1916, he was for a short time at the Wellcome Research Bureau under the Royal Society's scheme for training biologists for the diagnosis of disease, and was then engaged till 1919 on diagnostic work at the Kitchener Hospital, Brighton, and at a military hospital at Bournemouth. Early in 1920 he went to Cambridge as a Demonstrator in Zoology under Prof. Stanley Gardiner, and was there till his last illness. Throughout these four years he demonstrated in the courses of Biology and Zoology; latterly he was in charge, under Dr. Gadow, of the demonstrations in Vertebrate Comparative Anatomy, besides demonstrating regularly in Embryology under Dr. Shearer and Prof. Wilson, directing the practical work in Dr. Bidder's research-lectures on sponges, and giving advanced lectures on certain groups of invertebrates to students for Part II of the Natural Sciences Tripos. He joined with Mr. W. F. Lanchester in research on the circulatory system of the cat, a preliminary note on the Superior Vena Cava appearing in January 1921 (*Proc. Camb. Phil. Soc.*, xx, p. 228), and he would doubtless have produced further original work had his time been less fully occupied with teaching. He built up a large "coaching" connection, in which he earned the reputation of sparing no pains on his pupils, while he frequently declined to accept full remuneration from students in poor circumstances.

In much of Thacker's work there is far more originality than appears on the surface. Many other writings besides those mentioned above came from his pen. Keenly interested in anthropology, he regularly contributed the notes on that subject to Science Progress from vol. x (1915–16) till his death, as well as publishing several original articles on anthropological subjects in the same journal. While at Gloucester he devoted some time to determining the stratigraphy of the prehistoric remains in King Arthur's Cave, Herefordshire, but the printed results (1915) seem only to have been Museum leaflets which do not bear his name. A sound knowledge of German, gained during four months' stay in Hanover in 1907,

enabled him in 1913 to make a free translation of H. v. Buttel-Reepen's *Aus dem Werdegang der Menschheit*, under the title *Man and his Forerunners*, in the preface to which Buttel-Reepen acknowledged that the account incorporated in the book of the then recent discoveries in Sussex and Suffolk was due to Thacker alone. In an essay-review of Dr. J. C. Willis's "Age and Area," entitled "The Dynamics of Distribution" (SCIENCE PROGRESS, Jan. 1923), he brought forward original criticism of Willis's theory. His comments in the same periodical (July 1923) on Dr. Kammerer's lecture in Cambridge concerning the inheritance of acquired characters were also critical, and evoked some correspondence.

In 1909 Thacker assisted Mr. Hugh Elliot to translate Carl Hagenbeck's book of experiences among wild animals, under the title *Beasts and Men*. During Mr. Elliot's editorship of the *Annual Register* Thacker wrote the greater part of the foreign section of that work annually, from 1915 to 1921, besides contributing articles on the events of the war to other serials. That he should thus have thrown himself into the task of recording contemporary history is less remarkable in view of the fact that history had always been one of his leading interests; he had, indeed, in past years often displayed an astonishing grasp of some of its aspects, especially of the various territorial changes in Europe during the Middle Ages. In politics also he was a ready participant, and as a younger man was ardent in many athletic pursuits. He was unmarried. Those who knew him feel the loss of a highly valued and very loyal friend.

A Norwegian Discovery: Frozen Air. The Secret of the Aurora Borealis (H. C. S. Colborne.)

THE actual scientific cause of the wonderful coloured lights known as the Aurora Borealis in the northern skies at night which have always aroused astonishment and admiration in travellers and discoverers in the polar regions, has hitherto been shrouded in mystery; it was a thing which nobody could understand. But, according to information given a few days ago by M. Deslandres to the French Academy of Science, a Norwegian professor has made a discovery by which he has been able to reproduce in his laboratory at home the brilliant characteristic yellow-green radiations, the hidden secret of the Aurora Borealis.

It appears that, contrary to the old theory that at high altitudes the atmospheric composition changes into hydrogen and helium, the experiments of Prof. Vegard in his Christiania laboratory go to prove that the real fact is that at a height of about 100 miles, the usual altitude of the Aurora Borealis, the nitrogen of the air is still present, but frozen into solid crystals, which, when strongly electrified, cause the marvellous and mysterious northern coloured lights.

It is interesting to recall the graphic description Nansen gives of the sight of the Aurora Borealis from the deck of the *Fram* in his *Farthest North*. One night, in his diary, Nansen wrote: "Late in the evening Hansen came down to give notice of what really was a remarkable appearance of Aurora

Borealis. The deck was brightly illuminated by it, and reflections of its light played all over the ice. The whole sky was ablaze with it, but it was brightest in the south; high up in that direction glowed waving masses of fire. Later still Hansen came again to say that now it was quite extraordinary. No words can depict the glory that met our eyes. The glowing fire-masses had divided into glistening, many-coloured bands, which were writhing and twisting across the sky both in the south and north. The rays sparkled with the purest most crystalline rainbow colours, chiefly violet, red, or carmine and the clearest green. Most frequently the rays of the arch were red at the ends, and changed higher up into sparkling green, which quite at the top turned darker and went over into blue or violet, before disappearing in the blue of the sky; or the rays in one and the same arch might change from clear red to clear green, coming and going as if driven by a storm. It was an endless phantasmagoria of sparkling colours surpassing anything that one can dream. Sometimes the spectacle reached such a climax that one's breath was taken away; one felt that now something extraordinary must happen—at the very least the sky must fall."

Professor Vegard is continuing his experiments, endeavouring to discover some further secrets of the Aurora Borealis; and he also expects to be able to explain the mysteries of phosphorescent light.

H. C. S. C.

The Germ-theory of Disease.

THE date when philosophers or men of science first began to conceive the idea that many diseases are due to intruding parasites is not known with any certainty. We are therefore much obliged to Mr. E. Heron-Allen, F.R.S., for sending us the following paragraph from Restif de la Bretonne, *La Paysanne Pervertie*, 1770–80, Gay's Edition, Bruxelles, 1883, vol. iii., p. 43, and we shall be glad if any of our readers can inform us of any earlier mention of the hypothesis.

"Mais ce qui est bien singulier, pour cette maladie,[1] et pour toutes les autres qui sont contagieuses, comme la petite sœur[2] de celle dont je parle, la peste, la rage, les fièvres, la gonorrhée, c'est qu'elles m'existent pas en nous; se sont des êtres moraux, pour ainsi dire, qui, une fois engendrés, s'étendent, se propagent, se conservent, comme des germes d'animaux, des années entières sans altération. Cela est presque inconcevable, à moins de considérer ces miasmes, ces germes, comme des animalcules imperceptibles, dont les semences ont la faculté, de se conserver longtemps, et qui ne se développent que dans le corps humain, ou au moins dans des corps animés."

The King and Tropical Medicine (R.R.).

The King opened the great British Empire Exhibition at Wembley on Wednesday, April 23, and medical investigators were gratified to note that His Majesty found room in his short opening address for the following remarks: "The Exhibition," he said, *inter alia*, "stands for a co-ordination of our scientific knowledge and a common effort to overcome disease, and to better the difficult conditions which still surround life in many parts of the Empire. Think, for example, of the scientific work accomplished in recent years for the prevention and treatment of tropical diseases."

Enlightenment begins at the head, and our sovereigns have always done their best for science and art; but I have often thought that, in spite of the toys, the conveniences, the benefits, and the vision which science has conferred upon humanity, the mass of our people still remain in the intel-

[1] Syphilis. [2] Smallpox.

lectual condition of the Wigwam Age. Their bodies can travel in trains and aeroplanes, their eyes can watch scenes and their ears can hear sounds thousands of miles distant, they can traverse the ocean in floating palaces, and they can measure the planets and stars of heaven; but their spirits really still jog along barefoot in the deserts of barbarism and worship the fetishes and Molochs of long, long ago; and when the New Idea appears to them they throw their tomahawks at it—just as they used to do in the old, old days! Thus Edward Jenner, the discoverer of vaccination, was so persecuted all his life that in 1814, when he was sixty-five years old, he said to the Tsar of Russia in London, "I have received the thanks and the applause, but not the gratitude of the world." So it is still with many workers in medicine. Most of the poor men who were foolish enough to try to benefit their fellows in this field have been chased about for years by nimble people with assegais. Twenty years ago Mr. W. M. W. Haffkine, who gave us prophylactics against cholera and plague, was nearly scalped for his pains; and even such a humble adventurer as myself am now in process of being skinned alive (in the columns of *Nature*) by two Professors one of whom has been stalking me for a quarter of a century round the thorns and the cactuses which grow in the great desert of "parasitology"!

And I fear that even when the wild men of the world have got the discoveries ready made for them by others, they do not know how to use them and do not try to learn. Are we really civilised, after all? Seriously, matters of public health are of first-rate importance—not tenth-rate, as most of our politicians, officials, and journalists seem to think. Life and health are of much greater value to the thousand million people of the world than are the theories of philosophers, the mock-heroic combats of politicians, and the money-making of tradesmen: yet few pay any attention to the first. For example, one has only to mention that insignificant insect, the mosquito, to a British audience, and one will raise a hearty laugh at its expense; but really it kills about 2,000,000 human beings every year and keeps many more millions constantly ill, besides rendering many of the richest areas in the world almost uninhabitable. Yet, though we know how to deal with the pest, we make little effort to do so. Science advances, it is true; but the masses of mankind do not keep up with it.

The Organisation of Discovery.

Nature has been recently discussing the whole subject of the management and encouragement of scientific research, especially medical research. On April 26 and May 3 it described the recent proposals of the Intellectual Co-operation Committee of the League of Nations, which had decided to submit a draft convention for the protection of scientific discovery by something like patent. On April 19 Sir Ronald Ross advises a return to the proposals of the British Science Guild in 1920 suggesting payment for discovery; and on May 17 he writes an analysis of the whole subject and compares *subsidies for research* (that is, payment for expectations) with *rewards for discovery* (that is, payment for results). On the other hand, in *Nature* of April 26, Prof. E. H. Starling, F.R.S., deprecates any rewards for research. Discovery brings its own reward, he says; and he compares it with "The joy of being first on a virgin peak." Unfortunately, medical research is not a form of personal amusement, but a very serious matter concerned with the lives of millions of human beings; and men, if they are wise, will encourage it by every means in their power, including the unfamiliar method of paying for discoveries when made, as they now often do for other advantages which they receive.

Byron's Fatal Illness (R. R.)

We were very glad to see that the centenary of Lord Byron's death on April 19, 1824, was made the occasion for many articles in the press, and that most of these gave due praise to a man who, with all his faults, was a great personality and a great poet. What we have always admired about Byron was his possession of a virtue which ancient Greeks considered to be the first of virtues, namely, absolute courage. He never hesitated to say what he thought—even when he decided to condemn his own faults. To judge by the great volume of his writings during his short life, he must have been a very hard worker; and, though his numerous detractors have tried to belittle his efforts on behalf of Greece, he not only lost his life in that cause, but did so much for Greece that his name remains highly honoured in that country even to-day. There was nothing of the hypocrite about him; he was a man; and as a poet he had the almost supreme virtue of spontaneity. His thoughts flowed from him, and were not merely the result of infinite tinkering.

The cause of his death has always been a subject of great interest to medical men. It has been generally attributed to malaria, and *The British Medical Journal* for April 19 considers the matter at very considerable length and gives most of the necessary details. I was asked to contribute to the discussion, and added a note in which I showed that we are by no means certain as to that diagnosis. I think that the illness may have been "malarial fever by residual diagnosis," which "is a very poor form of diagnosis." Evidently the physicians in charge of the poet were themselves not too competent; and the result is that at this distance of time the actual diagnosis can probably never be fixed.

Notes and News.

H.M. the King has approved the award of the Founder's Medal of the Royal Geographical Society to Ahmed Hassanein Bey for his journey to Kufra and Dafur in 1923, and of the Patron's Medal of the Society to Commander Frank Wild for his services to Antarctic Exploration.

Dr. Horace Lamb has been elected President of the British Association for the meeting at Southampton in 1925. For the meeting of the Association in 1926 an invitation has been received from the University and City of Oxford.

Sir E. Sharpey Schafer has been elected corresponding member of the French Academy of Medicine and Sir William M. Bayliss has received a similar honour from the Brussels Academy of Medicine.

The Longstaff medal of the Chemical Society was presented to Prof. F. G. Donnan at the annual general meeting of the Society on March 27.

Dr. J. L. E. Dreyer has been elected President of the Royal Astronomical Society for the Session 1924–5, Capt. C. J. P. Cave of the Royal Meteorological Society, Mr. A Chaston Chapman of the Royal Microscopical Society, Mr. F. E. Smith of the Physical Society, Prof. G. G. Henderson of the Institute of Chemistry, and Prof. A. Barr of the Optical Society.

Prof. A. N. Whitehead has been appointed to the Chair of Philosophy at Harvard University for five years.

Prof. A. W. Porter, F.R.S., has been appointed University Professor of Physics at University College, London; Prof. A. E. Jolliffe, University Professor of Mathematics at King's College, London; Dr. C. K. Ingold, Professor of Organic Chemistry at Leeds; and Dr. I. Masson, Professor of Chemistry at Durham.

Dr. F. A. Bather has been appointed keeper of the Geological Department of the British Museum (Natural History). Mr. W. H. M. Greaves succeeds

Mr. Spencer Jones as Chief Assistant at the Royal Observatory, Greenwich, and Mr. F. S. Sinnatt, of the University of Manchester, has been appointed Assistant Director of Fuel Research.

Dr. Elihu Thomson has been awarded the Kelvin Gold Medal for Engineering. This medal is awarded triennially by a committee of the Presidents of the British Engineering Societies. The first award was made in 1921 to Dr. W. C. Unwin.

Among the many well-known men of science whose deaths were announced during the past quarter were the following: Dr. N. Annandale, Director of the Zoological Survey of India; Prof. G. A. T. Cole, Director of the Geological Survey of Ireland; Dr. R. E. Froude, who for many years had charge of the Admiralty experimental tank at Haslar; Hans Geitel, physicist; Prof. M. M. Hartog, biologist; Prof. J. W. Jack, mathematician and educationist; Mr. A. H. Jones, entomologist; Dr. W. H. Maw, one-time president of the Royal Astronomical Society; Dr. L. Péringuey, Director of the South African Museum, Cape Town; Prof. G. H. Quincke, For. Mem. R.S., physicist; Prof. J. Symington, anatomist; Capt. T. H. Tizard, formerly Assistant Hydrographer of the Navy.

The Royal Society has received £10,000 from an anonymous donor for the prosecution of research in preventive medicine, especially for cancer, tuberculosis, and tropical diseases. It has also received a donation of £525 from Messrs. Brunner, Mond & Co., towards the cost of publication of results of research in the physical sciences.

Brigadier-General C. G. Bruce, who was in charge of the Everest Expedition, has been forced to relinquish his command as a result of a severe attack of malaria.

It has now been decided by the Government that the Exhibition Galleries at the Imperial Institute shall be kept open. Viscount Cowdray has offered £5,000 a year towards the cost of the upkeep of the galleries, and other contributions have been promised by certain Dominion Governments.

The State scholarships awarded to students from State-aided schools to enable them to continue their education at the Universities are to be renewed this year. The original scheme was commenced shortly after the Armistice, but was stopped as a result of the recommendations of the Geddes Committee in 1922.

The Ontario Government has purchased thirteen aeroplanes for its Forest Service. They will be used for survey work, and as a fire patrol, in which capacity it is expected that they will effect a yearly saving of £50,000.

The Jubilee of the Physical Society of London was celebrated in a most interesting and satisfactory manner at a series of functions on March 20–22. An exhibition of apparatus was held at the Institution of Electrical Engineers on all three days and many objects of historic interest were shown. On the 20th the foreign delegates were received by the President (Mr. F. E. Smith), and the Guthrie lecture was delivered by M. le Duc de Broglie, who discussed some effects of high frequency radiation. At the end of the lecture Prof. Wien, in a most effective little speech, warned his hearers against too complete an acceptance of the ideas of the quantum theory, in spite of the fact that it was "made in Germany." In the evening Sir Richard Paget gave his interesting lecture on vowel sounds. The meetings on the 21st were devoted to reminiscences by Fellows of the Society of long standing. Sir William Barrett gave an account of its foundation by Guthrie and himself. Prof. Fleming dealt with similar matters, and was obviously delighted that, fifty years previously to within an hour, he had had the privilege of reading the first paper before the Society. Prof. Boys, who was due to speak, was, most unfortunately, kept away by illness, but sent a contribution which was

read, rather inaudibly, by the Secretary, Dr. D. Owen. Finally, at the end of the afternoon session, Sir Richard Glazebrook gave an account of the establishment of the international electrical units. His address, however, had far more connection with the Cavendish Laboratory as he knew it and with the National Physical Laboratory than with the Physical Society. In the evening addresses were delivered by Sir Arthur Schuster, Prof. H. E. Armstrong, Dr. Chree, and Sir Oliver Lodge. Finally, on Saturday evening a banquet was given in the Connaught Rooms, when H.R.H. the Duke of York and the Prime Minister addressed the guests.

The annual conversazione of the Royal Society was held on the evening of May 14. Perhaps the most striking exhibit was that shown by Prof. E. N. da C. Andrade of the Ordnance College, Woolwich. It was an apparatus designed to demonstrate the movement of a viscous liquid contained in the annular space between two rotating cylinders. The inner cylinder was a rod of silver steel 5 mm. in diameter, and the outer cylinder a glass tube of 10 mm. internal diameter. The motion of the liquid was indicated by suspended particles of colloidal silver, illuminated at any desired cross-section by a flat beam of light, optical distortion being avoided by running the glass cylinder through a cubical block of glass, the interspace being lubricated by a liquid of the same refractive index as the glass. The abrupt change of the motion of the liquid at the critical velocity was shown very beautifully with this apparatus. Another striking exhibit was a new form of cathode ray oscillograph shown by the Western Electric Co.

The new buildings of the National Academy of Sciences of the United States at Washington, erected at a cost of about 1,500,000 dollars, are now open. The most striking feature of the building, from the point of view of the general public, is the museum containing a unique collection of experiments in working order. The visitor can observe the motion of the sun-spots on a six-inch image of the sun projected on to a table in the central hall, and can compare the solar spectrum with that of the iron arc. He can manipulate the mirrors of a Michelson interferometer and control the exhaustion of a vacuum tube through which an electric discharge is passing: observe Brownian movements, the tracks of α-particles, and the growth of plants under different kinds of light. Most of the exhibits will be changed from time to time to make room for new inventions and discoveries in the realms of science.

As a result of experiments carried out in the cryogenic laboratory at Leyden, Prof. L. Vegard of Christiania states that he has identified the green line of the aurora with the light emitted by crystals of solid nitrogen when they are exposed to the influence of a stream of cathode rays. Under the influence of the rays the nitrogen, in his experiments, partly evaporated and began to emit the characteristic nitrogen red. Vegard therefore attributes the phenomena of the aurora to the presence of a "shell" of frozen nitrogen at a height of some sixty miles above the earth's surface. The greater frequency of auroral displays in polar latitudes is attributed to the fact that the atmosphere is shallower in those regions.

The *Proceedings of the National Academy of Sciences* (U.S.A.) for Nov, 1923 contain a detailed account of Prof. H. Shapley's investigations on the velocity of light of different wave-lengths in inter-stellar space. His work shows conclusively that a 25 per cent. difference in wave-length does not alter the velocity by more than one part in 20,000,000,000. The result provides striking evidence of the very small amount of absorbing matter in space, for his observations were made on stars at distances of the order of 40,000 light-years. Prof. Shapley has also made estimates of the distance of the object No. 6822 in Dreyer's General Catalogue of nebulæ by three different methods and finds that each gives a result of the order of 1,000,000 light-years. This is five times the distance found for the farthest

globular cluster, and the object, which is described as an aggregation of faint nebulæ and stars, is inferred to be external to our sidereal system.

We have received a number of interesting reports from the Bureau of Standards dealing with very diverse matters. The first (*Technologic Paper*, No. 240) contains the results of a number of dynamometer tests of motor tyres. Previous work by Prof. E. H. Lockwood of Yale had shown that, in average passenger car running on a hard level road at 20 miles per hour, about half the power developed by the engine is used in overcoming the rolling resistance of the tyres. This being so, the new results show that petrol consumption can be altered as much as 10–20 per cent. by the choice of tyres. The greater part of the power loss occurs in the carcass of the tyre. Cord tyres are superior to fabric tyres, and the best results were obtained with tyres with a very uniform lay-up of the cords. The thickness of the rubber between the different plies of the carcass has also a very marked effect. Wherever this thickness is low the power loss is high. The design of the tread is relatively unimportant—grooves running round the periphery of the tyre seem to have given the best results—at any rate, as compared with criss-cross patterns. Tests on treads having cavities forming so-called "vacuum cups" showed that this design causes no perceptible loss of power. Tubes, too, play a negligible part in the power loss—even when used in conjunction with a "puncture-proof" rubber band.

Scientific paper No 478 from the Bureau contains an account of a redetermination of secondary standards of wave-length from the new international iron arc. The system of secondary standards of wave-lengths now in common use was derived from an axial part about 2 mm. wide in the centre of an iron arc about 6 mm. long, and was established before the full importance of operating conditions of sources was recognised. Because of the pressure shift of certain lines in this type of arc the International Astronomical Union in 1922 recommended that the arc-length be 12–15 mm. and that the light used be taken from a central zone not to exceed 1–1·5 mm. in width. The measurements described in the paper were made with the new arc by the Fabry and Perot interferometer method. The values obtained depend on the group to which the lines belong, but are, on the average, from 0·0072 to 0·003 Ångström units less than the previous standard values. The paper contains a photograph of a new form of stand for the iron electrodes, which has an unusually large range of adjustment.

The third paper is mentioned to illustrate the wide range of investigation of a bureau of standards. It deals with methods of measuring the sizes of stockings! As a result of much study and analysis of the methods used by different manufacturers, it has been decided that in future the length shall be measured from the tip of the toe to the heel along a line passing through the bottom of the heel gore. Thus size 10 refers to a stocking in which the length of this line lies between 10¼ inches and 9¾ + inches. If the length was exactly 9¾ inches the size would be given as 9½.

The fascinating story of how the British Empire, in the comparatively few years since the beginning of this century, has risen to the proud position of being supreme in the world's rubber industry is told by Mr. W. A. Maclaren in a book on *Rubber, Tea, and Cacao* (published by Ernest Benn, 25s. net.), one of the series of twelve volumes on the Resources of the Empire prepared by the Federation of British Industries.

In the first year of this century, he says, the world's production of rubber was just under 55,000 tons, all of which, except a small matter of five tons, was wild rubber collected from virgin forest areas in South and Central America, Africa, etc. In 1905 the world's output had reached 62,145 tons, of which 4,172 tons were produced in the British Empire (principally West Africa) and 145 tons were plantation rubber. The next five years brought the word's output to 77,000 tons in 1910, of which the British Empire

contributed 11,217 tons, including 8,406 tons of plantation and 2,811 tons of wild rubber, still representing under 14 per cent. of the total output. By 1913 (the pre-war year), however, a great change had come over the scene. The British Empire supplied 48,187 tons out of the world's total of 115,000 tons, and of this quantity 46,919 consisted of plantation rubber. To come to 1922, the latest date for which full statistics are available, the Empire contributed 272,000 tons out of a world supply of 402,000 tons, viz. 68 per cent. and 100 per cent. of this was plantation rubber. Again, of the total world supply 377,000 tons was plantation and 25,000 tons wild rubber. The British Empire has thus moved rapidly forward from a position of comparative unimportance as a supplier of a variety of inferior wild rubber to one of commanding importance as the principal supplier of rubber of the world's markets—the work of British hands and brains, and financed by British capital. This growth has coincided exactly with the expansion of motor-car and tyre manufacturing.

The author gives some interesting figures to show the immense cost of starting a new plantation at the present time. In detail he estimates the expenditure on a 2,000-acre plantation—the minimum area for economical working—for the first five years before it begins to bear. This amounts, with interest on capital, cost of land, buildings, etc., to the enormous figure of £167,664, or £83 6*s*. 8*d*. per acre. As showing the absence of inducement to plant-up new acreage at anywhere near that figure, Mr. Maclaren tells us that at the end of 1923 there were numerous existing rubber companies whose shares could be bought at figures from £30 to £60 per planted acre.

The trustees of the Carnegie Trust for the Universities of Scotland have issued an elaborate document containing a list of the Fellows and Scholars appointed by the Trust, together with their publications and a brief account of their subsequent careers. Since the awards were first made in the academic year 1903–4, 306 Fellowships and 599 Scholarships have been awarded. Originally of a maximum value of £150 and £100 respectively, the annual value has now been increased to £250 and £175. In addition the Trust has made a large number of grants in aid of specified researches. The distribution among the several branches of learning which may be studied under the terms of the Trust Deed has been as follows: Physics, 45, 70; Chemistry, 71, 165; Natural History, 43, 91; Medicine, 74, 112; History (including Economics and Modern Languages and Literature), 73, 161—the number first given referring to Fellowships and the second to Scholarships. The records of careers and of publications which were the direct result of awards under the scheme are most imposing and show that the Trust is performing a great work in the country.

We have received copies of the first four numbers of the official Journal of the newly established Imperial College of Tropical Agriculture, Trinidad. It is entitled *Tropical Agriculture*, and is published monthly at 6*d*. net (London Office, 14, Trinity Square, E.C.). It is written brightly, well printed, and contains, in addition to articles contributed by well-known experts, a survey of conditions in all parts of the tropical world.

The pamphlets received from the Department of Scientific and Industrial Research this quarter include a Report from the Food Investigation Board entitled, *The Thermal Properties of Methyl Chloride* (price 1*s*., from H.M. Stationery Office, Imperial House, Kingsway, W.C.2), by D. N. Shorthose, M.A., which supplements the Report on Ethyl Chloride referred to last quarter, and the Third and Final Report of the Mine Rescue Apparatus Research Committee (1*s*. net). This Report deals (*a*) with liquid air rescue apparatus, (*b*) with metallic vacuum flasks, and (*c*) with miscellaneous matters, including compressed oxygen reviving apparatus. The air supply of the mine rescue party is carried in liquid form on the back of each man in a pack known as an *aerophor*. This is charged with about 6 lb. of liquid air soaked up in calcined

asbestos wool; it also contains caustic soda granules to purify the air breathed out by the user. The supply lasts about two hours, and the pack weighs about 40 lb. An important point in the design is the heat insulation, which must be so adjusted as to evaporate the air at a minimum rate of from 2–3 litres per minute. The aerophor is charged from large metal Dewar flasks brought from a central station, where the liquefying plant is located, on motor-lorries. The design of these flasks has already been described in these notes in a review of the Report of the Oxygen Research Committee, but some further particulars of interest are given. It appears that the burnish on the metal surfaces which face each other in the completed flask is a matter of the first importance; the exhaustion of the interspace between them (which, of course, contains charcoal) need not be extreme. It may be effected with an oil-pump, and the pressure may be as high as 0·5 mm. without loss of efficiency. At intervals the vacuum is renewed after the flask has been steam-heated to 100° C. to drive off the gas absorbed by the charcoal. The equipment and maintenance of the central stations is a most serious problem. A two-days' supply of liquid air for the packs of all the rescue teams supplied from the station must be kept in constant readiness and the cost per lb. of air is consequently high—on the average 13·15*d.* per lb. of air, which is equivalent to £7 12*s.* per 1,000 cubic feet of gaseous oxygen at 70° F. The committee suggests that in future small plants capable of yielding about 40 lb. of 95 per cent. pure oxygen per hour shall be installed. Pure liquid oxygen has marked advantages over liquid air for mine rescue work, and in addition such plants might be used to supply oxygen for medical purposes, for welding, and in such quarries and mines as may be permitted to use it as an explosive.

"Natural History," the journal of the American Museum of Natural History, New York, January to February, 1924, is an extremely charming number because of the many fine illustrations which it contains regarding natural history in Australia. We cannot afford such scientific luxuries in this poor country, and if we could afford the money we would not spend it in this way, but on sports and kinematographs. There are some beautiful photographs of Australian mammals, birds, and reptiles, and of the great barrier reef of Australia, the geyser region of New Zealand, and the Taos Indians.

CORRESPONDENCE

To the Editor of Science Progress

DR. KAMMERER IN CAMBRIDGE

I—By R. H. Nisbet

Dear Sir,—In correspondence under this heading, Prof. MacBride accuses Mendelians of believing that germinal variations are causeless. May I say that I have never seen anything in the writings of any Mendelian to support this idea, and that I do not consider the examples quoted by him prove his case.

Even if Mendelians do explain hereditary changes by the dropping or the appearance of a factor, I do not think that any of them would hold that such a happening was causeless.

Prof. MacBride evidently holds that an explanation is not an explanation if it explains the phenomena as being due to a change which has a cause. Such an explanation may not be an ultimate explanation, but if no explanations that are not ultimate are to be accepted, there would be very little science.

Mendelism is, as a matter of fact, quite consistent with Prof. MacBride's form of Lamarckian theory. It is conceivable that the reactions of the body on the germ plasm, which that theory assumes, are in the nature of reactions on the physico-chemical factors underlying Mendelian genes. Such an assumption would merely amount to a more detailed assumption as to the nature of the machinery by which the process, postulated by Lamarckians, comes about. There is thus no *necessary* opposition between the Mendelian and Lamarckian theories of evolution.

I think Prof. MacBride's real quarrel with Mendelians is that he thinks that if they admit causes of changes in Mendelian factors, they ought to, but do not, admit that such causes possess Lamarckian characteristics. But I do not see why they should admit this.

In Messrs. Guyer and Smith's experiments on guinea-pigs the environmental change of an injection of lens serum produced two effects. It made the guinea-pig blind, and it affected the germ-plasm. The change in the germ-plasm produced alteration in the descendants, and this alteration was of the kind to make some of them blind, *i.e.* to possess the new characteristic produced in the parent.

It is quite easy to *conceive* of a change in environment affecting the germ-plasm so as to change the descendants, but for the change not to be the same as that produced in the parents by the change of environment.

Lamarckians virtually state that this is impossible, that the alteration in the descendants due to the effect of any agency on the germ-plasm *must* be the same as the direct effect of the agency on the individual.

Dr. Kammerer's experiments do not prove this, even if they do prove that *some* changes have Lamarckian characteristics.

The Lamarckian doctrine is, in effect, an arbitrary and unsubstantiated restriction on the possible causes of variations.

Yours truly,
R. H. Nisbet.

Longfield, Kent.
April 7.

II—By Prof. E. W. MacBride, F.R.S.

Dear Sir,—In answer to Mr. Nisbet I should like to make my position in the letter which he criticises a little clearer. In common with most of your readers, I deeply regret the loss to science occasioned by the untimely and unexpected death of Mr. Thacker, with whom I was carrying on a friendly controversy.

The prominent representatives of Mendelism may admit when pressed, as Mr. Nisbet says—that mutations have causes. As to the nature of their causes Dr. Bateson, our leading Mendelian, has nothing better to suggest than "accidents of cell-division."

My objection to the type of argument which Mendelians use is that, as Darwin said of the creation hypothesis, "it is a form of words rather than an explanation." For to explain an hereditary change as due to the appearance or disappearance of a "factor" which is only definable by the change which it is supposed to produce, is to give, as even Goldschmidt admits, a purely "formal" explanation. It is about as enlightening as the old theological expression "the sinfulness of sin."

I do not think that any Lamarckian would say that it was impossible for a cause acting on the parent to produce an effect on the offspring different from that which it produced on the parent. What they do say is this: "All the evidence at our disposal points to the conclusion that evolution has been a slow, functional, and continuous process. The researches of Kammerer, Durkhem, and Pavlov prove that inheritance of function, *i.e.* of acquired characters, really takes place: therefore we regard this type of inheritance as *the* type which has brought about evolution." *A priori*, there seems no reason to deny that sudden changes of hereditary character—*i.e.* mutations—may not have played some part in evolution. Ten years ago I should not have gone further than this myself. Since that time, however, I have collected evidence which throws light on the physiological causes of these mutations. I cannot trespass, sir, on your space and good-nature by setting forth this evidence here; but if substantiated, as I believe it will be, it shows that these mutations are all of them due to a pathological process of "germ-weakening" which would unfit their possessors to survive in the struggle for existence.

Yours truly,
E. W. MacBride.

Imperial College of Science and Technology, London,
May 6.

ESSAYS

THE ANTIQUITY OF MAN IN EAST ANGLIA (J. Reid Moir, F.G.S., F.R.A.I.)

During the past twelve years a great change in opinion has taken place in regard to the question of the antiquity of man in England. The discoveries that have brought about this altered view have been made, chiefly, in the counties of Suffolk and Norfolk, where there occur certain ancient deposits—not present in other parts of the country—which have been found to contain implements made by very remote, and hitherto unknown, races of men.

The whole series of strata forming the earth's crust have been divided up by geologists into various periods, ranging from the most ancient Archæan to that of the present day. In geological parlance, this is the Recent Period, and, going back in time, we find that behind us, in successive order, are the Pleistocene and the Pliocene epochs. There are, of course, many others, but evidence of man's presence on this earth, so far as England is concerned, is not found in deposits more ancient than those of the Pliocene, and it is, therefore, not necessary to deal here with those periods which extend further back into the past. The general opinion upon the question of the antiquity of man obtaining among English scientific men prior to the discoveries which it is the purpose of this article briefly to describe was that the well-known palæolithic flint implements of pointed and ovate form, found, usually, in the terrace gravels of existing river-valleys, represented the earliest efforts of man to shape flints intentionally, and were all referable to post-glacial times. It is, however, somewhat remarkable that these views should ever have received such wide-spread acceptance. The earliest palæolithic implements exhibit evidences of great skill in flint-flaking, and it was unreasonable to have supposed that such well-made artefacts represented man's first attempts at implement-making. Further, the belief that palæolithic man lived, in England, only after the glacial period had passed away was never supported by sound evidence, and, moreover, was entirely opposed to the findings of continental archæologists, who, for many years, had recognised that the various races of palæolithic people lived in Western Europe during the more or less prolonged, warm, inter-glacial episodes occurring during the Ice Age. When it is realised that, in palæolithic times, England formed part of the Continent, enjoyed similar conditions of climate, and was inhabited by the same races of implement-making men, and the same varieties of animals, it becomes obvious that, to claim that, while on the Continent these human beings and animals lived during the glacial period, they existed here only after the close of that period, was not in accord with reason and with common sense. The results of the researches carried out in recent years in East Anglia have gone far, not only to show the truth of the foregoing criticism, but also to bring the English archæological evidence into line with that afforded by the investigations of continental pre-historians. But, before dealing with this side of the question, it is necessary to give some account of the very primitive pre-palæolithic flint implements which have been found in England. These specimens provide archæologists with the long-looked-for types leading up from the most simple artefact to the earliest, though elaborately flaked palæolith, and demonstrate a slow, but continuous improvement in the art of flint-flaking. It is now

many years since the great geologist, the late Sir Joseph Prestwich, made known to the scientific world the nature of the flint implements found by Benjamin Harrison in and upon the highest portions of the plateau of Kent. It was shown that the *provenance* of these implements—to which the name "eolith"—dawn-stone—was given—indicated for them a vast geological antiquity, and it was claimed that the whole of the great depression—the Weald of Kent—lying between the North and the South Downs, had been formed by erosion since the makers of the eoliths lived. The specimens themselves are of the simplest possible description, being mostly, naturally, fractured pieces of tabular flint exhibiting human flaking along one or other of their edges, which were evidently used for scraping and cutting purposes of a rough and primitive nature. Thus, both from the geological and morphological standpoints, the Harrisonian eoliths of the Kent plateau appear to represent the handiwork of the earliest human beings. When we turn from Kent to examine the case for the great antiquity of man in East Anglia, we are at once met with evidence which makes it clear that the Harrisonian specimens must be relegated to, at least, an early phase of the very distant Pliocene period. During the greater part of this epoch, and of that of the preceding Pliocene stage, a land surface existed in Norfolk and Suffolk. This land surface supported a varied succession of plants and animals, and in Pliocene times, as we now know, it was inhabited by races of flint-using men. Towards the close of the Pliocene period the land surface of Norfolk and Suffolk—owing, probably, to a sinking of the earth's crust in this area—was slowly submerged beneath the sea, with the result that the terrestrial remains of animals and plants, together with the flint implements of man, were quietly swept into the hollows and depressions existing in that surface. This detrital matter—composed of constituents of different ages, and containing the remains of warmth-loving animals—was, as the sinking of the land continued, covered by deposits of marine sands and shells to which the name "Crag" has been given. It is in the detritus-bed below the Crag that the first signs of glacial conditions—which, in successive cycles, separated by mild, inter-glacial phases, occurred during the Ice Age appear. The evidence, in East Anglia, of the former existence of vast ice-sheets is clear and unmistakable. The wide-spread Boulder Clays, with their included specimens of striated and far-travelled rocks, the glacial sands and gravels, often violently contorted by the great pressure of the moving ice, speak eloquently, and in no uncertain language, of the arctic conditions which, for uncounted thousands of years, held this country in their grip.

The great cliff-sections of the north-east coast of Norfolk present some of the finest examples of glacial beds known, and, between Happisburgh and Weybourne, it is possible to see mile after mile of deposits laid down by the ice of early Pleistocene time, the period preceding that in which we live. In the sub-Crag detritus-bed are to be found specimens of rocks foreign to the eastern counties, and, in all probability, brought to their present position by the transporting power of ice. Further, many of the flint implements in the detritus-bed exhibit, upon their flaked surfaces, deep and well-marked striations which would appear to have been imposed by some form of ice action. Moreover, though the oldest layers of the Crag contain the fossil remains of warm water molluscs, yet, as the various zones of this deposit are traced in successive order northwards, it is found that an ever-increasing number of cold water species is present, until, in the Weybourne Crag of Norfolk—the latest of these marine beds—nearly the whole of the included fossil shells are boreal. This gradual change in the molluscan life of the Crag sea indicates that the land was slowly sinking towards the north, and letting in the cold arctic waters to the Crag basin.

Beneath the Crag have been found several examples of Harrisonian eoliths, considerably abraded, and indicating that they had had a long history before

arriving in the detritus-bed. It is thus possible to arrive at the conclusion that the makers of these primitive implements lived during some period prior to that in which East Anglia was submerged beneath the waters of the Crag sea. The other flint artefacts found in the detritus-bed are of a more advanced type, are less abraded than the Harrisonian specimens, and are represented by various forms of scrapers, borers, and choppers, pointing to a higher level of civilisation than that present in eolithic times. The outstanding implement of the sub-Crag horizon is the rostro-carinate, which is made from a nodule of flint so shaped that, at one end, there is a comfortable hand-grip, while the other is flaked to the shape of the beak of a bird of prey. These specimens, which were no doubt used as picks and cutting implements, are obviously related to the earlier Harrisonian artefacts, and, further, are with equal clearness seen to be the ancestral form from which the later pointed and ovate palæolithic implements were derived. Among the numerous animals whose remains have been found beneath the Crag may be specially mentioned the *Mastodon*, an elephant-like creature of great size and strength—which with early man existed in the warm and genial climate of pre-Crag times.

It is evident that, after the deposition of the Crag beds, the land began to rise, and these beds were subjected to atmospheric denudation as a land surface. Upon this denuded surface there lived—as recent researches at Cromer have demonstrated—a race of people making very large and massive flint implements of Early Palæolithic types. These specimens occur at the base of a series of strata comprising the well-known Cromer Forest Bed deposits. These accumulations are estuarine in character, and show that there occurred another gradual sinking (in this case of only slight amount) of the land, during the process of which the surface upon which the makers of the large flint implements had lived, was submerged. The Forest Bed, in complete contrast to the shelly Crag underlying it, contains, almost exclusively, the remains of warmth-loving animals and plants. The mammalian remains are of great abundance, and include three species of elephants, differing from each other, and all of them widely from the more primitive *Mastodon* found beneath the Crag. The upper divisions of the Forest Bed also contain flint implements, and it appears that the people who made them lived in a wide and shallow river-valley—a northern extension of the present Rhine—now covered by the North Sea. There would seem little doubt that both the implements and the mammalian remains of the Forest Bed are referable to the Early Palæolithic-Chellian period, and it will be realised that to thus place these specimens in the first inter-glacial epoch is to depart very widely from the old view of the post-glacial age of all evidences of man in this country.

The Cromer Forest Bed is covered, and in many places cut into, by the immense deposits laid down by the Scandinavian ice-sheet of Early Pleistocene time, showing that, once more, glacial conditions were present in Eastern England. The recession of this ice-sheet was accompanied by a period of higher temperature, during which the glacial deposits were eroded by water issuing from the melting ice, and channels and lake-like hollows formed in them. These depressions were afterwards filled with gravels, sands, and brick earths, which have been found to contain the flint implements of the Acheulian and Early Mousterian races, who were evidently moving northwards as the ice slowly retreated. The occurrence in these deposits of actual "stations," or occupation-levels of these people, would seem to indicate that the small lakes then existing were frequented by wild-fowl, which no doubt formed an important item in the *menu* of the ancient Acheulian and Mousterian hunters.

At the close of the Early Mousterian period East Anglia appears to have been again invaded by ice, and a wide-spread sheet of Boulder Clay laid down. This Boulder Clay is the last well-marked glacial accumulation in

the eastern counties, but, after the ice responsible for its deposition had disappeared, the climate must have remained, for a time, very cold, as is clear from the discovery of numerous remains of reindeer, associated with Mousterian implements, in a deposit uncovered at a depth of about twenty feet—in the floor of the Gipping Valley at Ipswich. There is not much doubt that the Upper Palæolithic races, the Aurignacians, the Solutrians, and the Magdalenians, arrived in East Anglia on their hunting expeditions towards the close of glacial times, and their ancient hearths and occupation-levels have been found in some places buried beneath sand and other deposits of, probably, æolian origin, pointing to a steppe climate in this area. The last phase of Upper Palæolithic times occurred about 15,000 years ago, and was marked by a condition of low temperature resulting in the deposition of certain hill-washes. Since the close of the palæolithic period no great geological changes have taken place in East Anglia, and the relics of the later, neolithic, pastoral peoples are found scattered upon the present surface of the ground.

It will be seen from this short summary that man has flourished in the past in England, as he did elsewhere, in warm inter-glacial epochs, and that to regard all his remains found in this country as referable to post-glacial times is entirely erroneous.

The clear proofs of the existence of man in a limited area like East Anglia, from the earliest eolithic period to the end of the Stone Age, is unique, and raises important considerations as to the place of origin of the human race, which, however, it is beyond the scope of this present article to discuss.

Sufficient is it to say that the researches carried out by archæologists in Norfolk and Suffolk have established beyond all doubt the vast antiquity of man; the fact that he existed prior to, and survived the rigours of, the Ice Age; and that his rise to his present status has been in accord with the broad principles of Darwinian evolution.

SIR RICHARD PAGET'S THEORY OF THE NATURE OF HUMAN SPEECH (Sylvia Paget).

There are few things more detrimental to an accurate observation of scientific phenomena than excessive familiarity with their manifestations.

This has been clearly proved in the case of human speech, for it is only by a very deliberate effort that we can appreciate its extraordinary complexity.

Human speech consists of an immensely varied collection of musical sounds. These are of several distinct kinds and, in the case of the English language, are broadly classified as Vowel sounds, Diphthongs, Consonant sounds, Nasal semi-vowels, Unvoiced consonants, and Plosive consonants.

At present we propose to consider only the vowel sounds and the mechanism of their production.

It is generally known that vowel sounds, in the human voice, are made by the action of the tongue and lips, and that, in voiced speech, the sounds produced by the vibration of the vocal chords are converted in vowel sounds by modifications of shape in the mouth and throat cavities through which they pass.

In unvoiced speech, the action of the larynx is suspended, and the air passes freely through it, without vibrating the vocal chords. The result is the sound that is known as whispering.

In the English language the vocal chords do not contribute in any way to the distinguishing characteristics of the various speech sounds. If all the English speech sounds are whispered, they are just as easy to identify as when vocalised. From this it would appear that the language consisted, in its original form, of whispered, or breathed, sounds, and that the vocal chords were later brought into play; first, to give greater carrying power, when communication by speech became more vital, and secondly, to infuse subtler shades of meaning, by producing what are known as "inflections of the voice."

Experiments in the open air, made to test the carrying power of the different English vowel sounds (when whispered and when voiced), showed that, generally speaking, the action of the vocal chords served to increase the carrying power of the vowel sounds from ten to twenty times.

During the earlier stages of our inquiry into the nature of human speech it is obviously simpler to confine ourselves to whispered speech, and to free ourselves of the idea that the vocal chords are indispensable agents in the production of vowel sounds.

It has long been known that the different vowel sounds are produced by alterations of resonance in the vocal cavity for each vowel or vowels.

Willis, of Cambridge (1826), was the first to recognise this fact. He reproduced a number of vowel sounds by passing air through a tube of which he could vary the length, and so vary its resonant pitch. In this way some good vowel sounds were produced, but not the whole series.

Helmholtz (1860) noticed the presence of a second resonance in some of the vowel sounds, such as i, ei, and ʌ (up). He, too, produced vowel sounds by means of a resonator and vibratory reed. Instead of a resonating tube of different lengths, Helmholtz used a resonator made like a hollow sphere with a mouth, which he tuned by varying the size of the sphere and the mouth, respectively. The larger the sphere, the lower the note ; the larger the mouth, the higher the note. This is the kind of resonator that we make with our mouths.

Within limits, the resonant pitch of a Helmholtz resonator depends, not on its absolute size, but on the proportion between its size and the area of its orifice, or orifices. Thus, mouths of varying sizes can produce the same resonances, provided they can maintain the just proportion between the capacity of the vocal cavity and the size of the orifice.

This enables us to see why children, with very small mouths, can make the same vowel sounds as ourselves, though their "voices" may be pitched very much higher.

Some years ago, Sir Richard Paget noticed that, in every whispered sound of his voice, there were always at least two audible resonances. These could be made audible to the listener by shaping the mouth as if to make any vowel sound, and then driving air into the mouth by clapping the hands in front of it. It was found possible to keep one resonance constant while varying the other, and to make the two resonances move in contrary motion.

Having discovered the main principles involved in the operation of the human speech organs, the next step was to apply those principles to the construction of a device whereby the various vowel sounds could be reproduced.

The first essential was evidently that a resonating cavity should be made, giving in each case the two principal resonances heard in the human whispered vowels.

With this intention, a rough model of the inside of the human mouth was made in plasticine (Fig. 1). Its two principal resonances could be heard by tapping the model, or blowing across the mouth; but these were not the same as those heard by Sir Richard Paget in his own mouth.

Further experiments were made (Fig. 2), to try the effect of changes of shape, corresponding to those produced by the movement of our tongue, lips, etc.

From these experiments it became clear that the human mouth was behaving like two Helmholtz resonators, one in front of the other, the vocal cavity being divided into two separate resonators by the tongue, which acted as a movable partition or constriction.

Finally, models were made giving the two principal resonances of the various English vowel sounds, with sufficient accuracy to enable the vowel character to be clearly identified.

These models consisted of plasticine cavities, made in two portions, with an orifice between them (see Fig. 2). In the earlier models, the larynx

consisted of a single strip of india-rubber, which was set in vibration by blowing air against one edge. The later models were fitted with brass reeds, like those used for the reed-pipes of organs.

In their final form the models bear no resemblance to a human mouth; in fact, the shape is not of vital importance. Even the material makes a relatively slight difference to the resonance, as far as the vowel character is concerned. Pipes made of wood instead of plasticine, and with cross-section square instead of round, gave the same vowel sounds as their corresponding plasticine models.

The experiment was made of placing two resonators side by side, each one having a separate passage from the larynx. Successful vowel sounds were also produced in this way.

It was found that a variable double resonator could also be made, by means of the two hands clasped together, so that the fingers of one hand act as the tongue, or variable constriction, while the thumb and first finger of the other hand act as the lips to control the front orifice.

By blowing air into the vocal cavity thus formed through a rubber-strip reed, it was possible to produce a variety of voiced vowel sounds.

We may now recapitulate the principal facts established by Sir Richard Paget's succession of experiments.

Whispered vowel sounds depend on the passage of a current of air, whistling through the vocal cavity. The cavity is divided into two by the tongue,

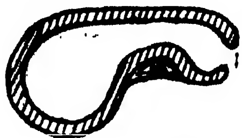

FIG. 1.—Original model made in rough imitation of the form and dimensions of the human mouth cavity.

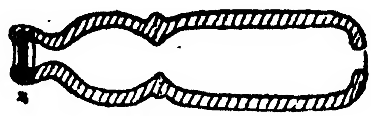

FIG. 2.—Model in final form giving 2 resonators in series. Vowel sound v. X = larynx.

which acts as a movable partition and converts the cavity into two Helmholtz resonators, each tuned to a particular note.

Our ears translate this "double whistle" into a single whispered vowel sound, much as our eyes convert two separate pictures into one distinct visual image.

By substituting an increased rhythmical air-stream, produced by the action of the vocal chords, we obtain voiced vowel sounds instead of whispers, and thus increase the carrying power of the sound, with no loss to its vowel character. The vowel sound also gains in interest and beauty from the possibility of variations in the pitch of the note produced by the vocal chords.

The two resonant pitches of the vocal cavity are unaffected by the pitch of the vocal chords.

Whispered vowel sounds can be produced in models by passing air through two suitably tuned Helmholtz resonators in series or parallel.

Voiced vowels are produced by vibrations of the air, similar to those created by the human vocal chords.

The shape and material of the resonators is of comparatively slight importance.

By applying the same principles as those used in reproducing the human vowels, it has also been possible to analyse the various consonant sounds and to reproduce them by means of models.

It is no exaggeration to say that Sir Richard Paget's models bear full witness to the complete success of his undertaking. He has found the key to a mystery which has been for some time as close to our ears as it has been far from our understanding.

ESSAY-REVIEWS

ABERCROMBY AND THE MODERN SCHOOL OF WEATHER FORECASTING. By E. V. NEWNHAM, B.Sc., Meteorological Office, London. Being a Review of **Forecasting Weather,** by SIR NAPIER SHAW, F.R.S., Sc.D. [Pp. xliii + 584, 195 figures.] (London: Constable & Co., 1923. Price 36*s*. net.)

THE present volume is a revised and enlarged edition of Sir Napier Shaw's classical work on forecasting weather which appeared in 1911. The merits of that book have been so widely appreciated both at home and abroad, that it is unnecessary to add anything further in that respect. The portions dealing with researches carried out under the direct supervision of the author himself, for example those chapters devoted to an account of line-squalls and the life-history of surface air currents, are particularly valuable, and these are repeated in the present work. At the same time the work of Continental meteorologists is not neglected, the methods of forecasting advocated by Van Bebber, Guilbert, and Durand-Gréville, for instance, being critically examined in an instructive way. During a period of several years subsequent to its first appearance the edition of 1911 was a very full account of the theory and practice of weather forecasting in vogue at the Meteorological Office in London. During the last twelve years, however, it has gradually ceased to convey an accurate impression of the mental attitude of forecasters towards their particular problem, and the need for a new edition has been felt for several years. Abercromby's *Principles of Forecasting* may well be described as the basis of pre-war forecasting as practised in this country, but they have tended to appeal less and less to forecasters both at home and abroad. In Norway, France, and England, many of the ideas underlying official forecasts can only be put into words by using expressions that were never used in the meteorological literature of Abercromby's time. The extent of the change is not, in the case of our own country, immediately apparent when the official " Inferences " of to-day are compared with those made twelve years ago. The reason for this lies in the complexity of considerations that are taken into account in the preparation of present-day forecasts—a complexity which tends always to increase, as the area covered by the network of telegraphic reporting stations becomes larger, and as the information transmitted to the central forecasting stations from each of the reporting stations becomes more detailed. It is frequently impossible for a forecaster to express completely his views about the significance of the general meteorological situation in a single paragraph suitable for publication as a " General Inference." The use of phraseology of a kind suggestive of the use of Abercromby's *Principles* often provides a partial solution of the difficulty, for the simplicity of such phraseology is admirably suited for the drafting of concise general statements. Its use, however, often results in a misleading impression being given of the forecaster's views about the situation. For example, he may be led to refer to the probable course of a depression the exact movements of which do not really concern him very much, since his deductions about the future weather may have been based mainly upon considerations connected with the distribution over Europe and the

Atlantic of those three very important elements in present-day forecasting—the air-streams (and their past history), the types of cloud reported, and the distribution of temperature both at the surface and at great heights. Sir Napier Shaw does not appear to take all the foregoing considerations into account, for, after quoting Abercromby to the extent of twenty-five pages of the latter's *Principles of Forecasting*, he makes the astonishing suggestion (p. 147) that "Upon the general principles there established the modern method of forecasting is based." It is difficult to reconcile this view with much that is said in other parts of the present work, and still harder to make it accord with much that has been written by others, in the last few years, on the subject of weather forecasting. Sir Napier Shaw's own contributions to meteorology have tended to make Abercromby's ideas out-of-date, as the following example well illustrates. Speaking of the conception of air-motion under balanced forces, for which there is some evidence in Shaw and Lempfert's *Life History of Surface Air-Currents*, he says (p. 207 of the present work):

"It is reasonable therefore to regard the normal state of the atmosphere as a steady state of circulation in which gradient is balanced by velocity disturbed by surface friction and other causes. This method of regarding the normal state is the more reasonable because it gives us a normal which actually corresponds with the average state as derived from the combination of observations the world over and is represented in its main outlines by the isobaric lines of the charts of average pressure, whereas an imaginary normal state of calm corresponds with nothing that, speaking generally, can be regarded as an average result for any part of the earth's surface. We shall therefore endeavour as far as possible to keep in view the idea that the physical processes which we are considering take place in some modification of a steady state of circulation, and not in a quiescent atmosphere.

"The idea is really essential to a proper understanding of the dynamical conditions, and hence of the stability or instability of the meteorological situation represented by synoptic charts. It suggests that the great streams of air, sometimes as much as 1,000 miles broad, are, from their own momentum, features of greater importance from the dynamical point of view than the positions of high or low pressure. The impression to be gathered from current meteorological literature is that the meteorological situation is represented by the meandering of high and low pressure areas in an inert medium, the undisturbed air. We are accustomed to speak of a cyclone encroaching upon an anticyclone, or an anticyclone resisting the advance of cyclonic depressions, as though there were a neutral field of operations in which these encounters take place. As a metaphorical description of a sequence of meteorological conditions, this particular form of literature has its utility, but the limits of utility are very easily passed."

The tendency to regard the air-streams as elements of the first importance in forecasting, and the areas of low pressure that happen to lie on the left of such air-streams as features of secondary importance, has been very pronounced in the forecasting branch of the Meteorological Office for years past, and has often resulted in the issue of forecasts different from those that would have been arrived at by an application of Abercromby's *Principles*. In any case, these principles are primarily concerned with the weather near the centres of cyclones, and could therefore only be the main basis of forecasting in regions subjected to an almost continuous series of cyclones separated from each other by more or less definite wedges of high pressure. This is not a normal state of affairs for the British Isles, which lie far to the south-east of the usual tracks of the more important Atlantic depressions. Those principles were formulated, moreover, at a time when the information available for the purpose of making forecasts was very meagre, and when in consequence it was legitimate, say, to assume, in the absence of definite information to the contrary, that the eastward or north-eastward passage of a

depression would be preceded by dull, muggy weather, and would be followed by clearer, cooler conditions, with passing showers. Nowadays there is no excuse for making such assumptions. During a spell of cyclonic weather an attempt is nearly always made to anticipate the great and often sudden changes of temperature and weather that generally occur at such times, not by the application of definite rules, but by an examination of both the present and past conditions over a wide area. The weather will not, for instance, be "muggy" in front of a depression unless the situation is favourable for the arrival of moist air from warmer regions, and it is the business of the forecaster to determine whether such is actually the situation. Similarly as regards the colder, clearer weather that so often follows the passage of the "trough" of a depression. It is no longer considered sufficient to predict the same changes in all such situations, for there is nearly always information available from ships out on the Atlantic to assist in an estimate of the order of magnitude and sometimes even the duration of the expected change. If, for example, a depression is followed by a wedge of high pressure of unusual extent—stretching, say, from the Azores to Greenland—this will favour the south-eastward movement of large masses of air from Arctic regions, and a change to much colder weather might be expected. In such a case the cold weather would probably last for several days. On the other hand, if the passing depression is only one of a series of such depressions, and high pressure is to be found over the southern part of the North Atlantic and over Southern and Central Europe, but not over Greenland and Northern Canada, a less definite and more transient fall of temperature is more likely, for any air which may arrive from the north-west is not likely to have come from high latitudes, and the situation is not one which is likely to result in a long-continued flow of air down from the Arctic regions.

If Abercromby's principles, as has been suggested, make at the most a very limited appeal even to those who hesitate to accept many of the new theories in their present form, still less will they appeal to advocates of the Polar Front Theory and of the present Norwegian System of Forecasting. Whether the new methods are resulting in better forecasts is, of course, another question, and one which is not easily settled because the definiteness now aimed at is much greater and makes comparison between recent and earlier forecasts difficult or even impossible.

To return to the discussion of the present work, it must not be supposed from the foregoing remarks that there is any tendency in it to belittle or ignore the activities of Norwegian and other meteorologists who have recently sought to develop new systems of weather prediction. A short account of Bjerknes' Polar Front Theory is given and also of L. F. Richardson's interesting attempt at a quantitative mathematical solution of the problem of weather forecasting. Most readers will, however, probably be disappointed at not finding more space devoted to such matters in a book which, judging from its title, one would have expected to be a more general discussion of forecasting, and less a history of the attempts in that direction that have been made in our own country.

FORECASTING WEATHER IN FRANCE. By E. V. Newnham, B.Sc., Meteorological Office, London. Being a Review of **Études Élémentaires de Météorologie Pratique**, by Albert Baldit. Second edition. [Pp. 428 with 132 figures.] (Paris: Gauthier-Villars et Cie, 1922. Price 24 frs.)

M. Baldit was, during the recent war, in charge of the "Service Météorologique du Groupe des Armées du Centre" on the Western Front. He was much concerned with the problem of forecasting weather at that time for military purposes. The present volume is devoted entirely to this problem. It is an enlarged edition of an earlier work, this enlargement being necessary

principally because of the researches made during the last few years both in Norway and France.

Among the many items of interest in M. Baldit's very clear and suggestive treatment of a complex and difficult subject, one of the most interesting is his discussion of Guilbert's rules of forecasting. M. Guilbert, one gathers, is not gifted with the art of lucid exposition, and has failed to convince the majority of the meteorologists of his own country that his rules contain anything of scientific value. He has not succeeded in placing the more simple and fundamental of these rules upon a sound physical basis, and the more elaborate rules seem to most people mutually contradictory in many instances. In M. Baldit's opinion, however, the fundamental rules concerning abnormal and divergent winds, the succession of cyclonic centres and ridges of high pressure, are of incontestable value when correctly applied. Depressions which give rise to winds on the ground which are abnormally strong for the horizontal gradient of pressure prevailing at the time will, according to Guilbert, "fill up" more or less rapidly; with abnormally light winds on the other hand a deepening of the depression is to be looked for. M. Baldit points out that the velocity of the wind for a given gradient of pressure depends to some extent upon whether that gradient is increasing or decreasing, for, as Ekholm has shown, the wind will tend to fit the gradient which existed at an earlier period. Thus, with a diminishing gradient, which more often than not corresponds with a depression of decreasing intensity, the wind will generally be abnormally strong for the gradient, and there will be some grounds for expecting the system to die out. This is the only rule for which a simple physical explanation seems to be forthcoming at the present time. It is worth noticing, in connection with this rule, that where synoptic charts are prepared for intervals of about six hours or less, it is not necessary to have recourse to this method of determining whether a depression is becoming deeper or not, for this information can usually be derived from a simple inspection of the isobars, barometric tendencies, etc., shown on successive charts.

The numerous papers of Bjerknes and other Scandinavian meteorologists that have been published during recent years show how dominant in Norway has been the consideration of discontinuities of temperature in the development of new methods of prediction. Such methods are probably better adapted to countries situated in the belt of prevailing westerly winds and on the extreme western margin of a large Continent, e.g. Norway, Scotland, and Ireland. The closer proximity of France to the subtropical belt of anticyclones, and its more continental position, necessitates that, even if entirely different methods are not adopted, at least the Norwegian method shall be supplemented by other methods better adapted to such a geographical position. This, at least, appears to be the opinion of Colonel Delcambre and some of his staff, who have carried the study of "isallobars," or lines of equal pressure change, further than anyone has done before. A French "forecaster" is generally able to identify a "noyau" of rising or falling pressure sufficiently soon to determine its probable behaviour while it is still of significance as a factor determining the probable change in the distribution of pressure over Western Europe. Some progress has been made towards explaining the behaviour of these systems, and M. Baldit gives a useful summary of the papers by Schereschewsky, Wehrlé, and others, that have been published under the direction of Colonel Delcambre. It is greatly to be hoped that still further research will be undertaken by these investigators. It is clear that a close connection exists between the "noyaux" and the depressions and anticyclones shown on ordinary synoptic charts, and the hypothesis that the velocity and direction of movement of a "noyau" is dependent upon the wind at some high altitude deserves thorough testing. M. Baldit's discussion in the tenth chapter of his *Études* of the causes determining changes

of pressure at various levels is of interest in this connection, although it contains nothing entirely new. It seems possible that the time is not far off when the elucidation of every important change in the height of the barometer will become part of the routine of anyone engaged in issuing forecasts based upon a consideration of synoptic charts. Whether this prove to be the case or not, the forecasters at the French "Office National" evidently find the isallobaric "noyau" an indispensable element in their work. The normal procedure there when forecasts are about to be made appears to consist in using every possible indication afforded by isallobaric charts. This will involve trying various intervals of time when calculating the changes of pressure, in order to select the interval which is most suitable for the kind of changes in progress. The deductions made from the isallobaric charts may be supplemented by the use of Guilbert's rules, which still enjoy a certain vogue in France, if the situation happens to be favourable for their application. The first step in forecasting is, however, the preparation of a chart showing the distribution of pressure to be expected after the lapse of a certain interval of time—an interval dependent of course upon the length of the period covered by the forecast. When this has been done the forecaster passes on to a consideration of the probable weather and temperature that are likely to be associated with the system of winds to which such a distribution of pressure must give rise. Although the indications given by clouds here play a very important part, M. Baldit has not a great deal to say upon this subject; it is probable that at the time of writing this book he was not in possession of full details about the researches then in progress at the "Office National" into cloud-systems and their movements, for the results of these researches have only recently been published.

Enough has now been said to indicate the general character of the work under review and its interest for English readers. In the present writer's opinion it is a work of great value, to which the author's command of advanced mathematical analysis, his wide reading of foreign meteorological literature, and his fortunate gift of clear exposition all contribute.

TO BE, OR NOT TO BE. By R. R., being a review of

Daedalus; or, Science and the Future. A Paper read to the Heretics, Cambridge, on February 4, 1923. By J. B. Haldane. Third Edition. [Pp. vii + 93.] (London: Kegan Paul, Trench, Trübner & Co., 1924. Price 2*s*. 6*d*. net): and of

Icarus; or, The Future of Science. By Bertrand Russell, F.R.S. [Pp. iv + 64.] (London: Kegan Paul, Trench, Trübner & Co., 1924. Price 2*s*. 6*d*. net.)

Wisdom cannot always follow Wit through the labyrinth of things; she is generally too tall to creep through the crevices which her nimble sister enters so readily and illuminates so brightly with her electric torch. Both these books, by our two prophets of science, are witty; but we are not convinced that they have thoroughly explored the whole subject. Each follows only a single line of conjecture. Dædalus flies along in the blue, quite optimistically as regards "science and the future"; but Icarus has a bad fall regarding "the future of science," and is pessimistic. Have the authors really considered all possible issues before selecting those which they think probable? For example, neither book mentions that most remarkable little work of Sir Flinders Petrie, *The Revolutions of Civilisation* (Harper Brothers, 1912)—a book which, if correct, would place quite a different view of the future before us. According to him, history strongly suggests that civilisation moves alternately upwards and downwards in a "sine curve" with a period of about fifteen hundred years; and certainly

very many facts support this view. When we witness the present rapid advance of science we think in our haste that the advance will always continue. Yet what has actually happened in the past? Take mathematics, for example—which practically began with Thales about B.C. 620 and almost ended with Diophantus about A.D. 280. This gives a period of only nine hundred years; after which came almost utter darkness until the time of Ferro, Cardan, and Copernicus—that is, the fifteenth century—an interval of twelve hundred years. What happened to the human intellect in this immense period? It was wandering about among the relics of Greece, like the Arabs among Babylonian and Pharaonic ruins. Almost nothing was done in mathematics; and the other branches of science, and literature, and even art, were almost equally forgotten during the same period. Is it that the mass-mind of men develops and degenerates in a regular sine curve? Is there not possibly behind this some extraordinary biological law which we have not yet penetrated? Petrie suggests that there have been eight such cycles in known history; and the stories of science in China and India tend to corroborate this view. Our authors think that progress is to be continued at the same rate indefinitely—may they not be very rash in doing so? Even if they hold a thousand years to be but a small interval, still we have not yet penetrated far enough into the mysteries of biology to predicate that the sine curve will continue for ever. It may flatten down to zero some day, when men shall reach the condition of degeneracy drawn by Mr. Wells in his admirable *Time Machine*. But without plunging so far into the fog of the future, we have to remember that time has already advanced seven centuries from Roger Bacon—what if only two more centuries of science are left to us? One thinks sometimes, on reading books regarding psychoanalysis and the subliminal self, that the end is not so far off. There were great men even in India and China—once. The creative spirit, in science as in art, appears to be only a rare efflorescence of the human tree. Each wave of progress is possibly not much higher than the previous wave, but the petals fall, perish, and are forgotten in the interval. Then lastly—terrible thought—whole species vanish utterly, but for their bones in the desert.

Another thing which our prophets seem to have forgotten: science may advance, but men may reject it. Dædalus imagines some day a system of "ectogenesis" in which selected ova are artificially fertilised by selected sperms *in vitro*. We might deduce from this process a race of magnificent human capons. One does not deny that in certain isolated tracts, as in England under the rule of the Ultimate Idealists, such a thing may be attempted—if it is not being subconsciously commenced already. But we fear that the whole "vision" will be rejected in most wild places where the voice of the teacher is not so easily heard in the land as the voice of the rifle. Men are obstinate; and, frankly, we can imagine that when Dame Nature attends the lectures of our professors she sometimes smiles behind her hand. She has been managing these little breeding affairs for many years past; she has not done so badly after all; many of her worst seeming failures have proved to become her greatest triumphs; and Eros can always vanquish Pallas Athene. As in Mr. Wells's romance, the Eloi will always be really controlled by the Morlocks, even as regards the continuation of their kind; and both will always be surrounded by a ring of virile barbarians ready to enter and take possession. Then again, in spite of extensive stock-breeding and pisciculture, we do not possibly know everything regarding the effect of substituting artificial selection for sexual selection. It may "breed-out" certain qualities which are really the vitamines of progress; and one can imagine a world-population entirely devoid of them. One sometimes sees people who look like it; and India and China are before us to-day.

Icarus is naturally much more pessimistic about the future of (racial) flying and doubts "whether, in the end, science will prove to have been a blessing or a curse to mankind." But it is not the fault of science if fools misuse it; nor need science be abandoned in order to prevent fools doing so. If they like to exterminate each other that will only raise the average standard of the rest of humanity! War seems to be his principal bugbear: but each of our chief diseases kills or maims more people every year than the recent war did while it lasted. Slums, poverty, drink, and immorality probably cause much more misery. After all, which particular inventions have done more harm than good to humanity? Ask your friends to name those which they would like to see abandoned—trains? motor-cars? aeroplanes? electric lighting? gas? ships? warm clothes? comfortable houses? sanitation? medical treatment? orderly government? And which particular scientific discoveries have injured mankind very materially—mathematics? astronomy? physics? chemistry? medicine? . . . None of them! The things which injure humanity are stupidity, selfishness, bigotry, superstition, sham politics, and spurious philosophy. Not Science, but Nescience injures humanity.

Nevertheless let us thank both authors for their pleasant imaginings. To form one good judgment many false ones must be sacrificed; and blessed are they who can put any new issues before us—to be turned and tested in the crucibles of thought—rejected—or accepted.

At one point Icarus takes us nearest the sun. "It is of the greatest importance to inquire," he says, "whether any method of strengthening kindly impulses exists." The economist will reply, mend the purse; and the physician, mend the body; but we can hardly believe in the author's humorous panacea of injecting hormones. Yet, on this crowded earth, his is the main point. Under their clothes men are much the same from age to age. The right prescription was written out and signed nearly two thousand years ago. A supreme science did that; and no science will ever do better.

REVIEWS

MATHEMATICS

History of Mathematics. Volume I: General. Survey of the History of Elementary Mathematics. By DAVID EUGENE SMITH. [Pp. xxii + 596, with many illustrations.] (London: Ginn & Company.)

WE are informed in the first sentence of the preface that "This work has been written for the purpose of supplying teachers and students with a usable textbook on the history of elementary mathematics, that is, of mathematics through the first steps in the calculus." It is difficult to judge how far the book will serve this purpose in America, because in this country the history of mathematics is scarcely ever considered by any but mathematicians and even seldom by them. The author says that the "subject is now recognised as an important one in the preparation of teachers of mathematics and in the liberal education of students in colleges and high schools"—in America! This first volume is composed almost entirely of short biographies of the most distinguished mathematicians, from ancient Chinese, Indian, Babylonian, and Egyptian mathematicians, through Greek mathematics from 1000 B.C., to European mathematics almost up to the present. Necessarily in a volume of only 547 pages each biography is a short one; but the stories are accompanied by many portraits, and also by many specimens of the handwriting of the subjects, and always by an extremely useful list of their principal works. For these reasons the work will be welcomed by many who are not engaged in teaching. It might be easy to criticise some of the particular biographies, but it must have been a hard task for the author to condense these life-histories so much, and we think that he has done the work remarkably well. Whether the revelations of J. M. Child in his "Geometrical Lectures of Isaak Barrow" (1918 (page 418), have been sufficiently discussed in the book in connection with the priority question between Newton and Leibniz is doubtful. It has always appeared to us that Mr. Child's investigations showed that the theorems often attributed to the latter had been enunciated quite clearly by Barrow, with the help of Newton, long previously. With our usual partiality for the foreigner, we have been tending for a long time past, at least since the time of De Morgan, to belittle the former in favour of the latter; but Child's work seems to have established that the latter's contribution to the calculus consisted of little more than his notation. The work closes with an excellent chronological table, and a good index. As Gauss said, mathematics is the queen of the sciences; and the story of it is really much more important than that of kings, queens, politicians, rascals, and criminals, which constitutes a large part of what is called "history" to-day.

A Short Course in Interpolation. By E. T. WHITTAKER, Sc.D., F.R.S., and GEORGE ROBINSON, M.A., B.Sc. [Pp. viii + 70.] (London: Blackie & Son, 1923. Price, 5s. net.)

OF late years the student who has been desirous of obtaining a knowledge of the theory and methods of interpolation has had to have recourse to books out of print, research publications, and books written by research workers in subjects other than Pure Mathematics, who have required for

their own use other methods of interpolation than that of "Proportional Parts." This book will be a boon to such students in the future. It consists of the first four chapters of a larger work by the authors on the Calculus of Observations, and is intended as a fit book for study by all mathematical students in their first year of a University Course.

The first chapter introduces the student to the operators of the Calculus of Finite Differences, and develops the simple forward difference interpolation formula. The second and third chapters are devoted to the consideration of the problem of interpolation, when the given values of the variate are at unequal intervals of the argument, and to the development of the formulæ involving central differences. The last chapter consists in applications of interpolation formulæ to other problems than finding the value of the variate for a specified value of the argument, such as the problem of sub-tabulation, the problem of finding the differential coefficients of the function, and so on. There are numerous worked examples illustrating the methods developed, at each stage; and at the end of each chapter are examples for exercise.

Not the least part of the book are the historical references to the work of other mathematicians in this subject. In this connection it is of great interest to note that the authors ascribe the discovery of the fundamental formula attributed to Newton to James Gregory. The authors are to be congratulated on the production of such a useful book, and should find that it satisfies a long-felt want.

E. C. Rhodes.

Cours de Mathématiques Spéciales. Par J. Haag. Tome IV: Géométrie descriptive. Trigonométrie. [Pp. xi + 152.] Exercices du Tome IV. [Pp. 154.] (Paris: Gauthier-Villars et Cie, 1923. Price 15 frs.)

These volumes conclude a lengthy work of which previous parts have already been noticed in Science Progress (July 1922, p. 154; Oct. 1922, p. 308). Except for two chapters on very elementary trigonometry, which seem to have been thrown in as a make-weight, they deal entirely with descriptive geometry. The general subject is the representation by means of two dimensional diagrams of surfaces and curves in three dimensions, and the methods are the traditional ones laid down in the main by Monge. It is no easy matter for one unversed in the science to say what will be the curve of intersection of a cube with a diagonal vertical and a regular octahedron with two faces horizontal, and this—with their positions and dimensions further particularised—is M. Haag's first problem. He himself admits that it is difficult and had better be omitted on a first reading, but it is a good example of the type of question proposed and solved by the methods given in the first of these volumes. From polyhedra, prisms, and pyramids the author goes on to cones, cylinders, spheres, surfaces of revolution, and quadrics, especially those with real generators. He then deals in a practical way with the interpretation and use of contour maps; the exercises have reference to a map of part of the Champagne front in 1917. Finally there are a few introductory remarks on perspective.

The author thinks it necessary to apologise for the small scale of the drawings, but they are really very clear indeed, and are worthy of so good an account of a complicated subject.

F. P. W.

Principles of Geometry. Volume III: Solid Geometry: Quadrics, Cubic Curves in Space, Cubic Surfaces. By H. F. Baker, F.R.S. [Pp. xix + 228.] (Cambridge: At the University Press, 1923. Price 15s.)

Prof. Baker's third volume amply fulfils the promise of the first two; in fact, from some points of view, it is even better, a synthetic treatment

of three-dimensional geometry being, perhaps, less familiar to English readers.

The first two chapters deal with the theory of a quadric surface, defined by means of its lines, and follow naturally after the theory of conics as developed in Vol. II. Particular attention is given to the properties of self-polar tetrads and self-conjugate pentads and hexads, as related to the expression of the equation of the quadric as the sum of four or more squares. The theory, which was much developed by Reye in the seventies, does not usually find a place in English textbooks. The co-ordinates of a line and the elements of the theory of the linear complex conclude the first chapter. Chapter ii, analogous to the second chapter of Vol. II, treats of the quadric in relation to a fixed conic and the properties of spheres, of circular sections of aquadric and of confocal quadrics fall into their place and find their natural generalisation.

In chapter iii the author goes on to the curve of intersection of two quadrics, and, in particular, to the twisted cubic. This curve is so simple and so similar to a conic, while affording an example of something quite new, that it surely should be known, in its principal properties, to every well-educated person, and not considered as "advanced." This chapter contains a mine of information about the curve, in summary form, with references for further study.

Chapter iv begins the theory of the cubic surface, taking first the double-six configuration. The most elementary synthetic proof of the existence of the figure, independent of the cubic surface, is due, as is well known, to Prof. Baker himself; it is reproduced here and occupies a page and a half. The cubic surface is then defined geometrically from two pairs of skew lines *a*, *b* and *c*, *d*, and a point O, as the locus of the intersection of the two lines joining the pairs of points in which a plane through O meets *a*, *b* and *c*, *d* respectively. From this definition the existence of the twenty-seven lines and of a family of twisted cubic curves on the surface easily follows and it is then assumed, as is stated explicitly on p. 175, although "in so doing we omit an algebraical discussion which should logically be given," that the surface so obtained has the generality of the locus expressed by the vanishing of a complete homogeneous cubic polynomial in the four co-ordinates of a point. Various properties of the surface are deduced, and we are then given another definition, by means of three related star-figures, with the representation of the surface upon a plane which immediately follows. The remainder of the book contains, in tabloid form, the Geiser deduction of the properties of the bitangents of a plane quartic curve from the cubic surface, the reduction of the equation of the surface to the Cremona form (the sum of six cubes) and the Sylvester form (the sum of five cubes), the properties of the Hessian surface, a note on the four-nodal cubic surface and on its dual, the Steiner quartic surface. Here, again, ample references are given.

Being highly compressed, the book naturally demands close reading; but it is invaluable, both as a store-house of information and as a source of inspiration to further research.

F. P. W

ASTRONOMY

Eclipses of the Sun. By S. A. MITCHELL. [Pp. xvii + 425, with 59 plates.] (New York: Columbia University Press; London: Humphrey Milford, 1923. Price, 17*s*. net.)

THE necessity for specialisation in various branches of science, which is continually increasing with the increase in knowledge and the refinements of observation, is strikingly exemplified by the fact that it is possible to write such a large book on a single department of astronomy, namely solar eclipses.

The appeal of this subject is undoubtedly great, even to the majority who have not been fortunate enough to glimpse the spectacle of a total eclipse, but their interest can scarcely fail to be heightened by the insight given with such descriptive ability by Dr. Mitchell into the difficulties and excitements experienced by eclipse expeditions and the nature of the problems which it is their object to solve. The question of eclipses is treated from the historical, theoretical, and observational points of view with considerable fullness, but in such a manner as to be intelligible to the general reader, who is still further assisted by the inclusion of such subjects as have any bearing on the main question or which are affected by the results obtained from eclipses. These subsidiary subjects range from brief histories of the earliest civilisations to the theory of Einstein, and include an account of the spectroscope with its application to solar physics, the constitution of the atom, and Saha's theory of ionisation. The book is not "popular," however, in the ordinary sense of the word. The spectroscopic and other related problems are discussed in some detail with few omissions even of minor importance, and an excellent feature is the copious number of references to original work which are given throughout the book. Errors are few (the most arresting being the repeated misspelling of Meudon), and the book is very well illustrated. It is the most complete treatise on the subject which has yet been written, and will undoubtedly be of considerable value to a wide range of readers.

D. L. E.

Time and Time-keepers. By WILLIS T. MILHAM, Ph.D., Field Memorial Professor of Astronomy in Williams College. [Pp. xix + 609, with 339 illustrations.] (London: Macmillan & Co. Price 30*s.* net.)

THIS book is designed primarily for the general reader, but it will also appeal to the connoisseur and to the scientist. The author's aim during the twenty years spent in its preparation has been to produce a work such that the answer to every question which might be asked would be found in it, or that the exact way of gaining the desired information could be obtained from the Bibliography. Many will wish to congratulate Prof. Milham on the success of his efforts. The production of a volume such as this is always in itself its own reward, but Prof. Milham will no doubt enjoy the additional gratification that he has been the giver of information and enjoyment to a large number of readers.

It is astonishing how many people there are who use clocks and watches every day of their lives and who at the same time have not the slightest idea of their history and construction, and do not even know how to take care of them. No doubt in some cases ignorance is due almost entirely to apathy, but there must be many others to whom Prof. Milham's book will be of great value. It assumes no previous knowledge of the subject, covers the whole field fairly thoroughly, and is lucidity itself. Those readers whose primary aim is to understand the mechanism of time-keepers would do well to begin by perusing chapter v, which is a description of the essentials of a clock in its simplest form. After that they may use their own discretion as regards what to read next, each chapter being practically an independent article.

The scientist, and more especially the astronomer, will naturally turn to chapters xvi and xix, which deal with chronometers and precision clocks. In these chapters the construction and care of these instruments are dealt with carefully but not too fully, as the book is not designed solely for the benefit of the would-be specialist. Still, there are probably many scientists who are somewhat in ignorance of these matters, and the chapters quoted will form a pleasurable introduction to a useful study. Further reading is suggested in the bibliography.

Those who are interested in the history of the subject and in clocks and watches as works of art in addition to being pieces of mechanism will find

that the author has not forgotten them. In this connection mention must be made of the numerous excellent illustrations and photographs which appear throughout the book. Indeed, the selection of illustrations seems to be a wise one from all points of view.

Mention must also be made of the excellent classified bibliography. Prof. Milham is to be congratulated on the production of a work which should be very valuable to all interested in the subject, however great their initial ignorance may be, and which should certainly be in every public or scientific ...

W. M. H. G

PHYSICS

The Structure of the Atom. By E. N. DA C. ANDRADE, D.Sc., Ph.D. [Pp. xiv + 314, with 49 figures and 4 plates.] (London: G. Bell & Sons, Ltd., 1923. Price 16s. net.)

DURING the last few years, besides monographs covering special branches of natural science, there has been published a large number of books purporting to give a survey of the great change in aspect of the ultimate constitution of matter, from the new view-point which has been reached by the study of the spontaneous disintegration of certain elements, and the extension of our knowledge of the various phenomena associated with electrical discharges. These books have been either semi-popular, like that of Graetz; or one-sided and full of specialised mathematics, like the standard work of Sommerfeld. Prof. Andrade's book is far more comprehensive than the first type, and more readable than the second. It is intended for serious students of the exact sciences, and the intention is so well fulfilled that any member of this class will be the poorer for not possessing it. It is also one of the few English books on pure science which is worth reading for its literary style alone. Some of the passages, in which shrewd philosophical observations are tinged with a lively touch of humour, are strangely reminiscent of Henri Poincaré.

The book is divided into two parts, the first of which is concerned with the nucleus of the atom. The pioneer work of Lenard on the passage of cathode rays through atoms receives the mention it well merits. The large angle scattering of α-particles is next dealt with, and two very fine photographs by Blackett are reproduced on plate I. Chapter iii describes radioactive phenomena and their indications of the probability of isotopes. The γ-ray spectra, measured directly by Rutherford and Andrade, and indirectly by Ellis, are then discussed and the theories of Ellis and Meitner are given. The next chapter describes Rutherford's well-known experiments on the disintegration of light atoms. A short chapter on positive rays follows. It is surprising that the remarkable confirmation of the results obtained for mercury is not mentioned. In the introductory account of X-ray phenomena W. L. Bragg is said to have investigated the intensity of the reflections from various faces of a crystal; and Prof. Andrade objects to the term "scattered radiation" for beams measured in this way. Surely the reflection does not come from the face of the crystal. The author's term, "regularly reflected beams," infers that the phenomenon is one similar to the ordinary reflection of light from a plane mirror. No such effect has ever been observed. It might be expected with very long waves at nearly grazing incidence, but W. L. Bragg was not working under these conditions. The great difference between the radiation passing into an ionisation chamber in the measurement of a "peak," and the radiation scattered by the atoms in other directions, is that the first is treated by the ordinary theory of diffraction, and the second by the quantum theory. The distinction is purely subjective. Perhaps the term "crystal reflexion," spelled with an "x" (page 99), refers to some other form of scattering which we must not call scattering. On the

other hand, Prof. Andrade's objection to the practice of omitting any effect of the nucleus from calculations of intensities of X-ray diffraction maxima is very sound criticism.

The final chapter of Part I is an introduction to optical spectra, and is in itself a little masterpiece. It is also very necessary to the average student. A large portion of the second part is devoted to an account of Bohr's theory of spectra and its extensions. The treatment is not new, but the great success of the quantum theory in this connection is well brought out. The account of Bohr's later work on the whole periodic system is of necessity incomplete.

The inclusion of a very readable account of the various static atom models, so dear to the chemist, is surprising in a book by a *fervent* of the quantum theory. The chapter on magnetism gives a short account of Langevin's theory, which, in spite of many difficulties, still accounts for the mass of magnetic phenomena better than the later theories. Probably the earliest conclusive proof of the rotatory nature of magnetism was its deduction from the Hall Effect by Kolácek in 1895. This has evidently escaped the notice of the author. The failure of the magneton hypothesis of Weiss is next discussed. If the graph of atomic susceptibility against atomic number for the metals of the iron group is a smooth curve, as has been suggested by Bohr and Cabrera, then the existence of a magneton is highly improbable. The success of the wonderful experiment of Gerlach and Stern is a great confirmation of the validity of quantum methods; but the suggestion that, for some quantum reason, an electron rotating in an atomic orbit produces twice the magnetic field to be anticipated on the classical theory is an extraordinary confession of faith. After this a rather unsympathetic account of Whittaker's magnetic atom model is expected. It is true, however, that it has not yet borne any experimental or theoretical fruit.

Any commendation of this book comes rather *au secours du vainqueur* at this stage. There is no doubt that the new editions will not lapse into the respectability of stagnation. The more things change the more they interest.

J. H. S.

Recent Developments in Atomic Theory. By LEO GRAETZ. Translated by GUY BARR. [Pp. xi + 174, with 39 illustrations.] (London: Methuen & Co. Price 9*s*. net.)

DURING the last few years publishers have been inundated with examples of scientific stocktaking, mainly by British and German authors. The standard book for the scientist is that of Sommerfeld, but it is unsuited for young students and for a larger public, with little or no mathematical equipment, which has nevertheless a keen interest in scientific progress. These lectures by Graetz are intended for these two groups. The translation, like the original, is very readable and concise. Here and there, however, occur phrases like "the higher the deflection" and "higher wavelengths," which, though unmistakable in meaning, are unusual in English. To Prof. Graetz himself must be paid the tribute that he shows the true scientific spirit in recognition of the work of men of all countries, without discrimination.

The book consists of six lectures. The first deals mainly with the kinetic theory of gases, and the author points out that Kelvin's vortex atom may yet help us to the realisation of a model of sub-atomic structures, although it must be discarded as a representation of the atom itself. The second lecture deals with the discontinuity of electric charges in electrolysis and vacuum-tube phenomena, and also gives reproductions of Aston's plates obtained with his mass spectrograph. Radioactive phenomena, with the nuclear theory and the theory of isotopes, are dealt with in the third lecture, whilst the fourth deals with X-ray spectra. The reason why Graetz insists

that for X-ray reflection the glancing angle must be small, *i.e.* incidence nearly grazing, is obscure, and the restriction seems unnecessary. The fifth and sixth lectures deal mainly with Bohr's theory of spectral lines and his models of electron distribution. A mention is made of the various geometrical theories of electron shells, but no account is given of them. It is true that up to the present they have not done any great service for us.

The book is not a critical survey of existing theories, but a short and, compared with its brevity, a remarkably complete account of them, without attempt at analogies which are supposed to appeal to the popular mind. It is not an attempt at " popular " science.

J. H. Smith.

Physics. Mechanics, Heat, and Heat Engines. By W. J. R. Calvert, M.A. Science for All Series. [Pp. x + 260 with 106 illustrations.] (London: John Murray. Price 3*s*. 6*d*. net.)

This is a useful book on modern lines and undoubtedly well suited as a general reader to accompany the practical science course of a secondary or high school. The book will perhaps be most appreciated in the less academic parts dealing with the practical applications of scientific principles to daily life.

W. C. B.

[illegible]ISTRY

Lead: its Occurrences in Nature, the Modes of its Extraction, its Properties and Uses, with some Account of its Principal Compounds. By V. A. Smythe, Ph.D., D.Sc. (Monographs on Industrial Chemistry.) [Pp. viii + 343, with illustrations.] (London: Longmans, Green & Co., 1923. Price, 16*s*. net.)

Lead shares with sugar the honour of being one of the purest substances obtained on a commercial scale; but, whereas the books dealing with sugar are almost legion, lead has been relatively neglected, so Dr. Smythe's excellent résumé of the subject will, for that reason, be doubly appreciated by chemists and metallurgists. Technical detail has, to a great extent, been subordinated, and, although attention has been given chiefly to modern methods, many of the older processes have also been dealt with either by reason of their intrinsic chemical interest or as the forerunners of modern methods. Stress has also been laid on the historical aspect of the subject and on the mutual relations of lead and its compounds in nature.

The greater part of the book deals, of course, with the extraction and metallurgy of lead; but there are chapters also on the history of lead, lead alloys, compounds of lead, and lead poisoning, so that the work is fairly comprehensive.

As the author points out, the development of the subject has been distinctly empirical, and it is to be hoped that the present careful and concise work may help in its way towards providing a more exact and scientific basis for the future development of this important industry.

F. A. M.

Conférence sur quelques Problèmes actuels de la Chimie Physique et Cosmique faites à l'Université de Paris en avril et mai 1922. Par M. Svante Arrhénius. [Pp. 120.] (Paris: Gauthier-Villars et Cie. Price 10 frs.)

In this very interesting book the author discusses a number of subjects which have formed part of the studies to which he has devoted his life. There are five essays in all, two problems much to the fore in physical chemistry:

(1) the third law of thermodynamics, (2) the dissociation of strong electrolytes, and (3) the theories of Bjerrum and Ghosh, (4) a problem which is of far-reaching importance to humanity at large, the world's sources of energy, and (5) an astronomical problem, the evolution of celestial bodies.

The chapters on physical chemistry are very valuable *résumés* of the present position and will prove extremely useful to teachers, and to research workers in this field. Perhaps the most interesting chapter, if one be permitted to choose among so much excellence, is that on the world's source of energy, particularly those parts bearing on the future of this country. We are very badly situated with respect to our supplies of all kinds of available energy, for our greatest asset, the coal supply, will be exhausted within 100 years. The author predicts that industry will be ultimately transferred to the more accessible regions of America, Africa, and Asia. In the last chapter he applies the modern theories of Saha and Eddington, etc., to the formation and dissolution of the celestial bodies. Incidentally, he examines the question of the conservation of solar energy and ascribes the unaccountably large evolution of energy to the dissociation of the atoms which occurs at the high temperatures in the interior of the sun.

This book is of wide interest to all scientific men.

W. E. G.

Theoretical Chemistry. From the Standpoint of Avogadro's Rule and Thermodynamics. By Prof. Walter Nernst. Translated by F. W. Codd, M.A. [Pp. xx + 922.] (London: Macmillan & Co., 1923. Price 28*s.*)

It is thirty years since the first edition of this book appeared, and, during this period, it has always been one of the foremost and perhaps the most important treatise on theoretical chemistry. The present translation is based on the eighth-tenth German edition, 1921. Eight years had elapsed between this and the previous edition, and numerous additions have been made to bring the book up to date. Certain sections, dealing with the periodic system of the elements, the structure of the atom, the application of X-ray spectroscopy to crystal structure, radioactivity, quantum theory, etc., have been widely modified and many minor additions are to be found.

The selection of those ideas of the modern science of to-day which will prove of permanent value in the future, is not an easy task. The choice made by an author will depend very largely on his views as to the future of the science, and he will naturally think those sections more important with which he is the more familiar. If the selection is carefully carried out, the book will not be a mere jumble of isolated ideas, but will form an organic whole, embodying not only the groundwork of the subject but also the considered views of one school of thought. It will not follow, however, that it is a complete survey of the subject, or that a student would be advised to confine his reading entirely to such a book.

In his textbook, Prof. Nernst not only deals with the fundamentals of the subject, but advances some of its sections up to the forefront of knowledge. This is so with the quantum theory and its application to the solid state, the third law of thermodynamics, Tammann's work on mixed crystals, Ghosh's theory of electrolytes, etc. Certain advances in recent years, to mention only a few, have received only slight attention, *e.g.* valency, complete dissociation of electrolytes and the modern activity theory, the work of Kraus on metallic solutions and the structure of metals, membrane equilibria, and the recent applications of physical chemistry to colloid chemistry, etc.

The book meets the needs of two classes of students, those reading for a first degree, and those carrying out original work. For the former, the book is rather too long, and students will need help in selecting suitable

portions to read. It would have been helpful if the author could have indicated the sections suitable for a first reading. Post-graduate workers, while finding it a very useful book of reference, will require to consult other books if they are to gain a comprehensive grasp of the modern trend of thought in theoretical chemistry.

In view of the subsequent criticism of the Ghosh theory, it would appear that its inclusion in the book was premature. Also, the statement that "four positive and two negative electrons form the doubly charged nucleus of the helium atom," is not so certain as was thought a few years ago.

The new edition is very welcome, and should form part of the library of all students of chemistry.

W. E. G.

The Determination of Hydrogen Ions. An elementary treatise on the hydrogen electrode, indicator, and supplementary methods, with an indexed bibliography on applications. By W. Mansfield Clark, M.A., Ph.D. Second Edition. [Pp. 480, with 42 figures.] (Baltimore: Williams & Wilkins Co., 1923. Price $5.50.)

No adequate definition of the concentration of an ion has yet been given and it is not surprising that the methods employed in the experimental determination of hydrogen ion concentration should not yield consistent results. Fortunately the discrepancies are not very marked in solutions of weak electrolytes and such methods may be used with a fair degree of accuracy for the solution of problems in many branches of science. There is, however, the possibility of serious error through a wrong choice of experimental method, unless the worker has a wide practical knowledge of such measurements or is able to refer to a comprehensive treatise on the subject. The author's book contains valuable information on the practical and theoretical aspects of hydrogen ion measurement, and the material which is presented has been carefully selected from a bewildering mass of original literature.

The author has very wisely chosen to retain the conceptions in common use, although he agrees that concentration may have to be replaced by activity in the near future. It is as yet uncertain whether the introduction of a thermodynamic basis will remove those inconsistencies which arise from the use of the more concrete model. It is rather doubtful whether, as the author states, measurements with the hydrogen electrode give the hydrogen ion concentrations in terms of a common experimental system of reference.

The most important sections of the work are perhaps those on the technique and use of indicators, colorimetry, hydrogen electrode, etc. Summaries are given such as those of the selected indicators of Sörensen, Clark and Lubs, and Michaelis, together with a list of their trade synonyms which are not found collected together in any other work of reference. Many of the individual peculiarities of indicators and their behaviour in protein and salt solutions are discussed. The preparation of "buffer" solutions, ranging from $pH = 1{\cdot}2 - 12{\cdot}0$, and the theory of their use with indicators are treated equally fully. The vagaries of the hydrogen electrode, the uncertainties of the liquid junction, the use of potentiometers and standard cells, and all those practical details which are so troublesome to collect from the original literature are described in an interesting manner. Chapters on reduction and oxidation potentials, on the errors of electrometric measurements, on supplementary methods of measuring hydrogen ion concentrations, and on the applications to different branches of science are given. The very full bibliography (107 pages) contains practically all the references on the subject, and the book concludes with an appendix of useful tables.

The book should prove a *vade-mecum* for workers with those elusive objects, the hydrogen ions.

W. E. G.

Manipulations de Chimie Colloidale. Par EDMUND VELLINGER. [Pp. 202, with 21 figures.] (Paris: Gauthier Villars et Cie, 1923. Price 10 frs.)

THIS book is a translation of the completely revised, fourth German edition of the well-known practical book by Dr. Wolfgang Ostwald. It contains 183, including 15 new, exercises on colloid chemistry which have formed the practical course in this subject at the Institute of Physical Chemistry of the University of Leipzig. A theoretical introduction is given before each set of exercises (see SCIENCE PROGRESS 1924, **72**, 655).

It is one of the best practical books on practical colloid chemistry.

W. E. G.

The Electron in Chemistry. By SIR J. J. THOMSON, O.M., F.R.S. [Pp. v + 144.] (Philadelphia: J. B. Lippincott Co., 1923. Price 10*s*. 6*d*.)

THIS book contains the substance of five lectures delivered by Sir J. J. Thompson at the Franklin Institute, Philadelphia, and is an account of the first serious attempt to deduce the properties of chemical substances in a *quantitative* manner from their sub-atomic structure. The author believes, and most chemists will agree with him, " that the introduction of the electron will break down the barrier of ignorance which has divided the study of the properties of matter into two distinct sciences, physics and chemistry." The success which the author has already achieved in this direction goes far to support his contention.

The electronic theory of matter, as developed by Bohr, has not yet been applied to chemistry in a quantitative manner. In its present form, it is perhaps too complex for this purpose. The author ignores this hypothesis, and commences by assuming a law of force between the nucleus and electrons which leads to a structure of the atom in agreement with the facts of chemistry. With it, he deduces the number and arrangement of the electrons about the nucleus, the size of the atoms, and arrives at a periodic relation between the elements which agrees closely with experiment. In the second chapter he develops an electronic theory of valency which in many of its aspects resembles that of G. N. Lewis and Langmuir. The " octet," however, takes the form of a twisted cube, and this modification brings the octet theory more into line with the physical properties of chemical compounds. On account of the quantitative basis, and the distinctive treatment of polar and non-polar valency, a very marked advance has been made. A polar compound is defined as one having a finite electric moment, and non-polar substances are those for which the electronic moment vanishes. The six pages on the connection between chemical constitution and chemical properties show clearly its value to chemists. For example, a simple explanation is given of the increase in acidic properties which occurs when methyl alcohol is converted into formic acid. The chapters on residual affinity and the mechanism of chemical combination are full of suggestions. One realises that chemistry, even its organic branch, must be "something more than free-hand drawing." The last chapter is devoted to a study of the solid state, to the compressibility of solids and their surface tensions, and the results are applied to mixed crystals and intermetallic compounds.

It is a critical discussion of the chemistry of matter which not only outlines the path by which chemistry may become more quantitative, but also enriches the subject with a wealth of material drawn from a lifelong experience in a sister science.

W. E. G.

GEOLOGY

The Geology of the Metalliferous Deposits. By R. H. Rastall, Sc.D., M.Inst.M.M. [Pp. xii + 508, with 81 figures.] (Cambridge: at the University Press, 1923. Price 21s. net.)

The fundamental note of Dr. Rastall's book is the treatment of ore deposits as rocks. He has sought, in the first place, to give a general account of ore deposition in the light of modern mineralogy and petrology; and, secondly, to illustrate his points by the description of typical examples. Since they are rocks, ore-deposits have to be dealt with by petrographical methods, and the book is therefore strong on this aspect. Dr. Rastall is a great believer in the igneous origin of metalliferous ores; but the distinction between the ultimate source of the material and its immediate mode of origin, is always kept in mind. An instructive example is that of the nickel ores of Sudbury (Ontario), which were unquestionably brought in by the great Sudbury intrusion of norite. But it can still be held that the ore was deposited in its present form mainly by hydrothermal agencies acting some time subsequent to intrusion.

The book is about equally divided into two parts, the first on General Principles, the second Descriptive. The introduction deals with the general composition and constitution of the earth. Then follow chapters on the igneous rocks, the sedimentary and metamorphic rocks, the relation of water to ore deposition, forms of ore deposits, composition and character of ore deposits, classification, relation of ore deposits to external influences (treating of such subjects as secondary enrichment, oxidation, weathering), metallogenesis, metallogenetic zones, and mineral formation. These chapters form an excellent compact summary of the principles of ore deposition.

The second part of the book contains succinct descriptions of an extremely well-selected series of typical examples. These are classified on the basis of the principal metal involved; hence, there are chapters on copper, iron tin deposits, etc. A feature of these descriptions is their geographical catholicity. They are drawn from all parts of the world, and are neither practically restricted to North America, as in some recent American texts, nor to European occurrences, as in a recent German work.

The scientific rather than the commercial value of the examples has been the criterion of selection. Hence we are glad to see extended descriptions of the iron, lead, zinc, and tin deposits of our own country; and descriptions of universally known ore fields within the British Empire, which figure but rarely in textbooks; such fields, for example, as the copper ores of Namaqualand, the tin of Malaya and Nigeria, the lead and zinc of Broken Hill (N.S.W.) and of the Bawdwin Mines, Burma, the iron ore of Wabana, Newfoundland, the tungsten ores of Tavoy, Burma, and the goldfields of Mysore and of the Porcupine region of Ontario.

The book is well printed, but, in order to keep down cost and size, it is not overburdened with illustrations. Misprints and mistakes are few; on p. 372 the thickness of the Insizwa picrite seems to have been multiplied by ten, and Fig. 44 (Skorovas Mine, Norway) is divorced from any connection with the text.

The book is to be recommended as certainly the best and most up-to-date text on ore deposits from the British side; but its leaning towards igneous origins will not commend it to some geologists.

G. W. T.

Geologic Structures. By Bailey Willis. [Pp. xi + 295, with 10 plates and 121 figures.] (New York and London: McGraw-Hill Book Co., 1923. Price 17s. 6d. net.)

This book describes geological structures such as stratification, flexures, folds, joints, faults, and cleavage, and applies the principles of mechanics

to their interpretation. Thus it clears the ground for the discussion of the larger earth problems such as orogeny and epeirogeny, although the scope of the book does not take in these subjects. The subject has also a most practical aspect in the problems of the winning, extension, and valuation of economic geological materials.

The interpretation of rock deformation rests on considerations of the strength of rocks, the arrangement of strata with respect to their relative strength or competency, their load and depth of deformation, and on the pressure and temperature conditions obtaining during the deformation. The forces which result in the deformation of rocks are everywhere existent, and always active or ready to act; they are gravity and molecular forces such as adhesion, cohesion, and chemical affinity. Pressure and heat are regarded as secondary agencies inasmuch as they depend on the two primary forces; but, as they are the immediately effective agencies, they have to be recognised as the proximate causes of deformation. Incidentally Willis's discussions and analyses expose the mechanical impossibilities of the more extravagant kinds of folds and thrusts in which we are asked to believe by certain Alpine geologists.

The nine chapters of the book deal successively with stratification, mechanical disturbances of strata, joints, structures of igneous rocks, faults, cleavage, mechanics of rock deformation, field methods, graphic methods, and practical problems. The chapter on the mechanics of rock deformation is the longest and the stiffest to read; but, while attention is demanded, the subject is put in the most lucid manner, and is perfectly understandable by readers who may have little acquaintance with the mechanical principles involved. The chapter on field methods, while admirable in itself, does not quite fit in with the scheme of the book. It contains, however, a valuable discussion of surface forms in so far as they help in the understanding of underground structures. The final chapter gives some useful geological geometry, graphical methods of determining thicknesses, strike, dip, and outcrop.

The author is alive to the horizontal displacement that must occur in trough-faulting as it is generally conceived and figured (p. 76). Also, with Shepard, Willis is sceptical of the alleged greater abundance of "normal" faults with respect to "reversed" faults; or, at any rate, their number and importance have been exaggerated (p. 52).

There are appendices giving dip tables, a proof of the law of maximum shear, and a striking series of plates illustrating Willis's experiments in folding, taken from his *Mechanics of Appalachian Structures*. The only slip we have been able to discover is the omission of the letters MN from Fig. 3 (p. 9).

The book constitutes a technical and advanced study, and it will prove invaluable to students of the larger problems of earth structure.

G. W. T.

BOTANY AND AGRICULTURE

Elements of Plant Biology. By A. G. Tansley, M.A., F.R.S. [Pp. 410, with 63 figures.] (London: George Allen and Unwin, 1922. Price 10s 6d. net.)

A number of elementary textbooks of botany have appeared during the years since the war, but this one differs from most works of a similar scope in a number of important ways. In the first place, more attention is given to facts of general significance in biology. As these can be best illustrated by reference to lower forms of life, we find in consequence considerably more space devoted to lower plants than is usual in elementary botanical textbooks. Thus non-vascular plants account for about half the book. In treating the higher plants, a good general account of structure, function, and development

is given, so that the reader should obtain a clear view of the mode of life of such plants. More attention is given to what may be termed the general physiology of the cell, and this matter is introduced quite early in the book.

The book thus differs not only to some extent in content, but more particularly in arrangement, from the majority of elementary botanical textbooks. After an introductory chapter on the characteristics of plants and animals and the scope of biology, two chapters deal with the chemical and physical characters of organic substances. The next chapter introduces the reader to protoplasm and two simple organisms, Amœba and Protococcus, a simple animal and a simple plant. After a chapter on vital functions come three chapters dealing respectively with the cell, the green plant, and the colourless plant cell as exemplified in the yeast plant. In the chapters which follow dealing with various lower plants, saprophytism and parasitism are dealt with in relation to bacteria and fungi, the origin of sex and of the soma with the green algæ, differentiation of tissues with the brown algæ and life on land with the Bryophyta. The remaining chapters deal with the structure and life-history of the flowering plant.

The author is much to be congratulated on providing a book which gives a sound and interesting introduction to the principles of plant biology, and which is not a mere catalogue of facts of structure interspersed with remarks on physiology illustrated, for the most part, with simple and inadequate experiments. The book is intended primarily for medical students, and the reviewer knows of no other elementary textbook he would more willingly recommend for this purpose. The book should also be of interest to the general reader who is not working for examinations and has no intention of specialising in scientific work. Every intelligent person should have some knowledge of the principles and scope of biology. This book will supply him with this knowledge as far as plants are concerned.

W. S.

Laboratory Manual of Fruit and Vegetable Products. By W. V. Cruess, B.S., and A. W. Christie, M.S. [Pp. vii + 109.] (New York and London: McGraw Hill Book Co., 1922. Price 7*s.* 6*d* net.)

This manual provides further evidence of the vigour of the College of Agriculture of the University of California, whose staff has provided a number of first-class works on applied plant sciences during the last few years. The book in question gives directions for the examination and testing, and in some cases for preparation, of canned fruit and vegetables, fruit juices and syrups, jellies, jams, marmalades and preserves, candied fruits, dried fruits and vegetables, essential oils, vinegar, sauerkraut and pickles, fruit acids and olives. A last section deals with the preparation of museum specimens. It will be seen, therefore, that the ground is well covered. Methods of analysis and a table of refer nces conclude the book.

The book is intended in the first place for use in agricultural colleges and domestic science schools, but, as the authors say, " much of the information given is of value to growers, manufacturers of fruit and vegetable products, food inspectors, and chemists, home demonstration agents, and girls' club leaders, and teachers of agriculture and of domestic science in secondary schools.

W. S.

The Manuring of Grass-Land for Hay. By Winifred E. Brenchley, D.Sc. [Pp. viii + 146 + 22 figures.] (London: Longmans, Green & Co., 1924. Price 12*s.* 6*d.* net.)

This is one of the Rothamsted Monographs on Agricultural Science. The nature and the quality of the work conducted at the premier agricultural experiment station are of such importance that it commands the respect of

agricultural scientists throughout the world, while its practical bearing is sufficiently important as to appeal to the more widespread farming community. In 1856 Lawes and Gilbert laid down the now world-famous Park Grass manurial plots and at the end of twenty years published their results. Since then a large mass of data and material has accumulated in respect of these plots, and the very complete records have not only had reference to the effects of manuring on yield, but also on the composition of the herbage. In the present monograph, Dr. Brenchley has succeeded in compressing this data into a form which is most intelligible, while the various deductions are not only highly instructive, but they are reinforced with that measure of reliability which can only be secured where an experiment is continued over a great number of years.

The first main section of the monograph deals with the effect of manures and lime on the individual plots, noting the herbage types and changes as well as yield variations, while the second section is most valuable in its treatment of the effect of manures and lime on individual species. In the interpretation of the results it is recognised that allowance must be made for the fact that the volume deals solely with the Rothamsted plots, which are situated on a heavy soil in a fairly dry climate. Nevertheless, as a guide to future experimentalists it is rich in suggestions, as well as being a model of conciseness.

H. G. R.

Extinct Plants and Problems in Evolution. By D. H. Scott, M.A., LL.D., F.R.S. [Pp. 240, with Frontispiece and 63 figures.] (London: Macmillan & Co., Price 10*s.* 6*d.* net.)

There are few botanists living so well qualified to deal with the subject of extinct plants as Dr. Scott, and in these pages we have a summary of the salient features regarding the vegetation of the chief geological epochs, embodying the mature experience of a lifetime devoted to the study of fossil plants. It is significant that, despite Dr. Scott's unrivalled knowledge, or rather perhaps we ought to say because of it, there is little of theory or speculation. Indeed, beyond pointing out that the results are on the whole favourable to the Darwinian conception of evolution, the author is content to leave us with the facts, and these are themselves sufficiently interesting.

Following an introductory chapter treating of evolutionary theories the second is concerned with the Tertiary epoch and the abrupt appearance of highly organised Angiosperms in the Lower Cretaceous, suggestive of a long history of which we have no knowledge. This imperfection of the record of the rocks is again brought home to us in the occurrence of the advanced Gymnospermous stem *Palæopitys milleri* in the middle Devonian. Thus, almost contemporaneous with the simple, or very possibly reduced, plants of the Rhynie chert, there were probably land plants of a very complex organisation whose origin is wrapt in mystery.

Chapter iii is concerned with the Mesozoic era of Cycadophyta, which flourished from the Middle Cretaceous right back to the Upper Triassic, being represented in the later horizons by the Cycadeoideaceæ and in the earlier by the Williamsoniaceæ. As we should expect, Dr. Scott writes very guardedly of the suggested relationships between the latter family and the Angiosperms, and whilst admitting the striking "analogy" he says that "after all, a wide gap remains."

The fourth and fifth chapters deal with the Permo-Carboniferous Flora, with its Pteridosperms, Lepidodendræ, Calamites, and Cordaiteæ, whilst the last chapter treats of the Devonian Flora already referred to.

The plants of the various horizons are simply described, but, though the work is intended to appeal to non-botanical students, we fear that without

a fair knowledge of this subject a great deal of the text would have but little significance to the reader. It is the anatomical viewpoint which is chiefly emphasised, and one cannot but regret that the fascinating subject of the geographical distribution of the floras of the past has not received fuller treatment.

E. J. S.

Farm Soil and its Improvements. By SIR JOHN RUSSELL, D.Sc., F.R.S. [Pp. vi + 126, with 37 figures.] (London: Ernest Benn, 1923. Price, 7*s.* 6*d.* net.)

THIS little book is written specially for the farmer, and aims at giving information with regard to the soil, soil fertility, and manuring which should guide the practical man. The information has all been obtained at first hand and is given briefly and in very readable form. As the author says in his preface, farming is too complex to enable the formulation of prescriptions for general use; the broad principles relating to farm soil and its improvement are therefore set forth with typical illustrations to show how they are to be applied on particular farms. It must always be left to every individual to apply the principles to his own case.

The book is well produced and the illustrations are, for the most part, of high quality.

W. S.

ZOOLOGY

Outlines of Evolutionary Biology. By ARTHUR DENDY, D.Sc., F.R.S. Third edition (revised and enlarged). [Pp. xliii + 481, with 190 figures.] (London: Constable & Co., 1923. Price 16*s.* net.)

PROF. DENDY is to be congratulated on the success of this book, the first edition of which was published in 1911, and the second, which has been reprinted several times, a year later. The book is intended for those who have no biological training as well as for students of biology. The chapters are grouped under five headings, namely, the structure and functions of organisms, the evolution of sex variation and heredity, the theory and evidences of organic evolution, and the factors of organic evolution. Throughout the treatment is physiological; in the first chapter an animal or plant is described as a complex and extremely delicate piece of mechanism, constantly employed in collecting energy directly or indirectly from the sun's rays and in using that energy to maintain an incessant struggle against the destructive forces of the environment. In the chapters on the evidences of evolution the modifications of the pentadactyle limbs of vertebrates in adaptation to different functions are described, and many other examples are given to justify the author's conclusion that "wherever we turn we find that novel requirements are met, not by the sudden creation of new organs, but by the gradual modification of old ones." After detailing a number of highly specialised and precise adaptations Prof. Dendy states that it is obvious that no theory of evolution can be regarded as satisfactory which does not offer some explanation of their origin. With regard to theoretical questions the various opinions that have been and are held are fairly stated, and the author's attitude towards controversial matters may be indicated by his own words. "In dealing with problems of this kind a rational conservatism, with a mind always open to conviction, seems the only safe attitude to adopt."

The book is full of information, is admirably clear and well written, and is distinguished by sanity and moderation, qualities that are sufficiently rare nowadays to deserve special mention.

C. TATE REGAN.

The Biology of Birds. By J. ARTHUR THOMSON, M.A., LL.D., Professor of Natural History in the University of Aberdeen. [Pp. xi + 436, with numerous illustrations in the text and 9 half-tone plates.] (London: Sidgwick & Jackson, Ltd., 1923. Price 16*s.* net.)

THIS volume is one of a series, *The Biology of the Seashore* being another that has already made its appearance, whilst others are yet in preparation. The general idea of the series is an excellent one. In the present instance it has been put into effect in Prof. Thomson's most characteristic style. In a wholly delightful and masterly manner he has applied the more general concepts of biology—variation, heredity, behaviour, adaptation, etc.—to birds.

Almost faultless typography, a good index, and a useful bibliography help to make the volume one of the most attractive bird books that has made its appearance for some time. It is, moreover, excellently and liberally illustrated, though it might be well to point out that Fig. 19 (p. 77), illustrating the flight of the heron, is very misleading. A bird with its wings upraised in flight is temporarily at a lower level than when its wings are at the end of the down-stroke, which is exactly the opposite to what is happening in the illustration.

Amongst biologists generally birds appear to have been greatly neglected. The ornithologist as we know him in the main is a student of skins and habits and cares little for the wider fields of biology. It is a pity that such a separation should exist. The present volume, with its wealth of information and thought-provoking suggestion, is an admirable illustration of what the welding of these all too frequently separated interests can achieve. It cannot fail to stimulate a study of the many unsolved problems that confront the ornithological biologist.

W. R.

A Practical Handbook of British Birds. Editor: H. F. WITHERBY, F.Z.S., M.B.O.U. Authors of the various sections: ERNST HARTERT, ANNIE C. JACKSON, Rev. F. C. R. JOURDAIN, C. OLDHAM, N. F. TICEHURST, and the Editor. [Vol. I, pp. viii + xvi + 532; Vol. II, pp. xii + 959; 30 coloured and monochrome plates and 350 text figures.] (London: Witherby, 1919–24. Price £4 10*s.*)

THE last part of Messrs. Witherby's *Practical Handbook* was issued on Feb. 26, thus bringing to completion a very remarkable and invaluable work. The first two parts to make their appearance were reviewed in an earlier issue of SCIENCE PROGRESS (Oct. 1919). It is therefore unnecessary to repeat a general account of the work, but some additional remarks are not out of place.

The completed work is in two volumes, the second of which, in order to obviate unwieldiness, to be bound in two parts. Each volume is supplied with its own index and list of illustrations. The altogether superfluous "Index of British Names" of Vol. I, a mere repetition from the general index that follows immediately behind it, has been judiciously omitted from Vol. II. The work concludes with a list of additions and corrections, extending to some fifteen pages, which bring the whole up to date to the end of 1923, followed by a brief systematic list of British birds in which these changes have been incorporated. This consists of the scientific name of each form approved by the authors in one column with the popular name in each case underneath and the briefest possible description of the bird's status in a second column. In these days of continuous nomenclatural change this list of the latest versions will be handy for speedy reference.

This most useful and important work, bringing up to date, as it does,

the mass of information about British birds for which the student has hitherto depended upon Yarrell and Saunderson, will inevitably supplant these two. Both have for a long time been very much out of date. But the *Handbook* achieves far more than merely bringing old information up to date. It contains what is no doubt the most complete regional account of moults and plumages yet published. Its comprehensive nature may be gauged from the fact that the plumage changes of the 229 occasional and irregular visitors now admitted to the British list are as carefully worked out as those of the breeding birds and regular migrants. This makes it a valuable contribution not only to British but to holarctic bird literature.

Critical comment, in the limited space at our disposal, is unfortunately impossible. It might even prove unfruitful in so controversial a field, for while one can disagree here and there with the solutions of nomenclatural knots and recognition of sub-species, the authors' own opinions are in every case so carefully weighed up that it would be no easy task to support a counter-contention with any prospects of successful conviction.

There is one point, however, to which attention might be drawn with a view to remedy in a future edition, since it directly affects both students in England interested in American species and American ornithologists who will make use of the Hand List as they inevitably will. Although the book is scientific in its conception and execution, it will be widely used by amateurs to whom a Latin name means little or nothing, and who habitually refer only to the popular name. The popular names used for American strays are in many cases the *British* names, practically unknown in America beyond a small circle familiar with British literature. Thus the Dowitcher of American literature and daily usage is called the Red-breasted Sandpiper with no clue to what may be termed its *real* name. The chance has, therefore, been missed of clearing up what is a very old source of confusion. And the book will certainly be used extensively by amateurs on account of its wealth of reliable information.

There is considerable lack of agreement on the two sides of the Atlantic with regard to certain questions of nomenclature and taxonomy. It is evident here and there that our authors have been in touch with American ornithologists. In view of the intimate relationships of the avifaunas of the two sides, this is as it should be. It is on the other hand a relief to note that they have kept the tendency to create unlimited new genera, so evident on the other side of the " pond," in reasonable check.

Mr. Witherby, in his triple capacity of part-author, editor, and publisher, together with his co-authors, deserves the heartiest congratulations on the successful completion of so comprehensive an undertaking. We can only hope that the price of the finished work, unavoidably high in these expensive days, will not interfere with the popularity and wide circulation to which the work is most deservedly entitled.

W. R.

The Principles of Insect Control. By ROBERT A. WARDLE, M.Sc., and PHILLIP BUCKLE, M.Sc. [Pp. xvi + 295, with 32 text figures.] (Manchester: The University Press; London: Longmans, Green & Co., 1923. Price 20*s*. net.)

THE literature which deals with the application of scientific methods to the control of injurious insects has become so extensive in recent years, and so widely scattered in various countries, that everyone interested in this branch of the subject will welcome the publication of this book.

It is a survey of the subject up to date and is a useful addition to the literature on the economic aspect of entomology.

The subject matter of the work is treated in four parts, together with an

Appendix, the latter dealing with apparatus and machinery used in connection with the application of insecticides.

Part I, entitled "Biological Control," deals with the questions of host resistance, climate, insect parasites and birds in relation to insect control. It should be noted that the experiments with *Pieris* referred to on page 3 should be credited to Verschaffelt (1910) and not attributed to Tragardh (1913). The chapter on parasites and predators might with advantage have been further extended, considering the international interest and importance of this aspect of the biological control of insects. The discussion regarding the definition of the terms "parasite" and "predator" tends to confuse the reader in a condensed treatment of the subject, which is unavoidable in a work of this kind.

Part II, on "Chemical Control," deals with insecticides, dips, attractants, repellents, and fumigants, and is a most interesting and comprehensive survey of the subject. The term "sterilisation" as used by the authors in this section is open to criticism, considering the sense in which the term is generally employed.

Part III, on "Mechanical Control," treats of such questions as cultural methods in relation to insect control and preventive measures which aim at restricting the spread of injurious insects. Here, also, the use of the term "disinfection" in the sense as defined on p. 179 is open to criticism, considering the generally accepted meaning of the term.

Part IV deals with the subject of "Legislation" in relation to insect control measures.

The book gives a comprehensive survey of the subject. It is the first of its kind, and the authors are to be congratulated on presenting this compilation, which covers a very wide field, in such an interesting and readable manner. The bibliography appended to each chapter is a useful feature. It is with sincere regret that we learn of the recent death of the junior author.

James Davidson.

The Marine Products of Commerce, their Acquisition, Handling, Biological Aspects, and the Science and Technology of their Preparation and Preservation. By Donald K. Tressler, Ph.D. [Pp. 762.] (New York: The Chemical Catalog Co., 1923.) Price £2 5s.

With such a romantic subject as the products of the sea an abundance of matter should be forthcoming of interest alike to the specialist and the amateur. Mr. Tressler in no way disappoints our expectations, and presents from an economic standpoint a bird's-eye view of almost the whole of the life of the sea which is in any way of use to man. In this most fascinating book he leads us through the intricacies of salt manufacture and the utilisation of seaweeds for chemicals and for food, which part occupies rather less than a quarter of the volume, up to the many animal products of the sea, from sponges, pearls, and precious coral to the various shell-fish, true fishes, marine reptiles and mammals which occupy the remaining pages.

Each group of plants or animals dealt with has a short introduction showing its structure and natural history, and articles on individual products are contributed by experts. Amongst these the account of "Pearl Essence" by Mr. Harden F. Taylor is peculiarly attractive, the history of an artificial pearl made from fish scales being almost as wonderful as that of the genuine article.

Naturally the fishes take up the largest part of the book and a number of pages is allotted to descriptions and figures of the different species. As these are mainly American, one is at times somewhat confused with regard to

the common or trade names of some of these. To quote but one instance, the term "sardine" in this book principally refers to the herring, *Clupea harengus*, of a certain size and preserved in a certain way, although it may also apply to the pilchard, as it does exclusively in Europe.

Great stress is laid on the importance of continuing such researches as those of Hjort, Drummond, and others on the vitamins in various fishes and their foods, and prophecies that extension of these investigations will probably result in an enormous increase in the utilisation of sharks and other hitherto almost valueless fishes lead us into future dreams where there are no waste products and the most neglected commodities become the most valued. Thus, if Vitamin "A" is contained alike in certain fishes, in the copepods they eat and in the diatoms which serve as food for the copepods, we may look forward to seeing the extension of marine industries into the minuter products of the sea with copepod and diatom culturing farms, and it is clear that no research undertaken even on these apparently insignificant organisms is without value.

Mr. Tressler's book fills a long-felt gap and will be useful in a variety of ways, containing as it does such a large amount of valuable and up-to-date information.

MARIE V. LEBOUR.

MEDICINE

The Action of Alcohol on Man. By PROF. E. H. STARLING, C.M.G., Sc.D., F.R.C.P., F.R.S. With Appendixes by ROBERT HUTCHINSON, M.D., F.R.C.P., SIR FREDERICK MOTT, K.B.E., M.D., F.R.S., and PROF. RAYMOND PEARL, Ph.D. [Pp. vii + 291, with illustrations.] (London: Longmans, Green & Co., 1923. Price, 12*s.* net.)

PROF. STARLING has left us with the impression that he has pursued his task with a lively zest. He has done more than merely present a popular account of the action of alcohol. The fundamentals of human physiology are presented with such naïve simplicity that the lay reader may scarce be persuaded of how much he has learnt. The main results of the scientific investigation of the action of alcohol in man are collected and presented in a pleasantly logical argument in which are considered not only nutrition, digestion, circulation, respiration, and body temperature, but also the effect of alcohol on human behaviour, of immoderate use, and of the influence of alcohol in the community. Prof. Starling permits himself the expression of a personal conclusion which must weigh with the reader. In his view, Prohibition by general consent would be contrary to the permanent interests of the race; Prohibition against the wishes of a considerable minority would be a national calamity.

The Appendixes to the volume merit more than the notice we must be content to make, as they form valuable monographs in their respective subjects by men whose names bespeak their authority. They deal with "Alcohol as a Medicine," "Alcohol in Problems in Mental Disorders," and "Alcohol and Mortality."

The descent of the pure scientist into the arena of popular controversy has seldom been an unqualified success. Let us be frank. The question of tolerance or Prohibition will not be decided by any committee of learned men. It is a question in which are involved powerful influences and dour prejudices on either hand, the instinctive rebellion of the human kind battling with its instinct for interference. How the verdict will be given will only be written in the history of the emotions of the event. Viewed, therefore, as a serious contribution to the controversy, whilst the book may assist some

careful minds to a decision, it is as likely to stimulate as many to an obstinate opposition. At all events, it will provide an arsenal of which both armies are likely to make much discriminating use.

R. K. C.

Canned Foods in Relation to Health. By WILLIAM G. SAVAGE, B.Sc., M.D., P.Ph. [Pp. vii + 146.] (Cambridge: at the University Press, 1923. Price, 8*s*. 6*d*. net.)

THIS book consists of the Milroy Lectures delivered before the Royal College of Physicians in February and March 1923. It gives an excellent general account of the history of the canning industry and the principles and practice of canning. As might be expected from the title of the book, special attention is directed to the bacteriology of canned foods and to the changes, both deleterious and the reverse, which may take place in canned foods. An account of botulism is naturally included. Two useful appendices deal with the principles involved in the processing of canned foods and with laboratory methods for the examination of canned foods. The general conclusion to which the author comes is that, while canned foods have definite and special risks of their own, these are not great and, for the most part, are easily guarded against.

The book can be thoroughly recommended to all who wish to obtain information on the question of canned foods. It forms a useful addition to the literature of food preservation, and fills a distinct gap.

W. S.

Vitamins: A Critical Survey of the Theory of Accessory Food Factors. By RAGNAR BERG, translated from the German by EDEN and CEDAR PAUL. [Pp. 415.] (London: George Allen & Unwin, 1923. Price, 18*s*. net.)

THE most useful purpose served by this book is to bring before the reader records of the German literature on this subject of which he might not yet be completely cognisant. Presumably the German work is accurately recorded; the English work is not. Prof. J. C. Drummond's proposal of the substitution of the spelling "vitamin" for "vitamine" is quite wrongly interpreted, a table of comparison of lipochrome content and vitamin A content of certain food-stuffs is inaccurately reproduced from the original paper; and numerous other mistakes might be quoted.

Berg is blinded by his own ideas and work on the importance of the inorganic salt content of a diet; and, not content with taking up a chapter of his book with the subject, he interprets the results of other workers, time after time, as being due to a "salt" deficiency rather than the deficiency recorded by the experimenters. (Apparently he ignores the controls.) One instance of this is worth quoting. Berg refers to the fact that certain workers have found dried milk to be a much better food for adults than fresh milk. Without giving any grounds for his criticism, he says, "But these same experiments really show that the reputed inadequacy is not due to any defect in the composition of the milk protein, but obviously to the modified requirements of the adult organism in the matter of inorganic constituents."

Berg makes most bewildering statements. On page 228 he says: "Funk and Macallum began by isolating A by the method then in vogue for the isolation of vitamin, namely by extraction with 0·5 per cent. hydrochloric acid; but obviously all they could get in this way were specimens of A containing vitamin as impurity." Why call it isolation at all? In the same paragraph he reports work by Steenbock, Boutwell, Gross and Sell, then a further contribution to the same subject by Drummond, then further work by the previous authors, as if it had been carried out by the latter. He concludes

the paragraph with the wish that, "with this end in view (*i.e.* the obtaining of A in the pure state) the physiologists would seek the aid of a competent chemist." It may be pointed out that McCollum, Hopkins, Drummond, Rosenheim, Harden, Osborne and Mendel (to mention only a few of the people who have put solid work into these questions) are primarily chemists.

There are no illustrations in the book, and the author suggests a new term, "complettin," for vitamins A and C on no other ground than their lack of nitrogen; and yet he calls his whole book *Vitamins.* Altogether it is a confused, and, in many cases, an inaccurate, account of the subject.

KATHARINE H. COWARD.

Introduction to Medical Biometry and Statistics. By RAYMOND PEARL. [Pp. 379.] (Philadelphia and London: W. B. Saunders Company, 1923.)

THIS volume by Prof. Pearl aims at the presentation of an introduction to statistical methods applicable to the fields of biometry and vital statistics. The illustrative examples, therefore, are such as are calculated to appeal to the biologist and medical man interested in the quantitative side of their subjects. Prof. Pearl is Professor of Biometry and Vital Statistics in the John Hopkins University, and in his preface states that the book is the result of many years' experience in attempting to teach biometric methods to men not primarily interested in mathematics. He has therefore made no attempt in the book to deal with the rigid mathematical proofs which are the basis of so many of the modern statistical methods. Also, as the book is intended merely as an introduction, the author has been able to cover a great deal of ground in this single volume, and has written about most of the statistical methods applicable to the subjects of biometry and vital statistics, stressing the underlying principles and presenting the results.

He gives in his introductory chapters some account of the history of the subject, and of the men whose names stand out in this survey. He stresses the importance of the proper tabulation of statistical data and the intelligent graphic representation of them, always most important parts of the work of a statistician. He has chapters on Life Tables, Death Rates, etc., discusses the elementary theory of Probability, and proceeds to consider the subjects of variation and correlation. He finishes up with a chapter on simple curve-fitting.

Throughout, the author makes extensive use of actual data and diagrams to illustrate the methods discussed in each chapter, and is very careful in difficult cases to indicate the pitfalls into which the unwary are likely to descend. At the end of each chapter is a list of books and papers which the diligent student of the subject would read with benefit, if he is interested in the development of the methods considered.

The book, by its very nature, is in certain parts of necessity sketchy, but should certainly prove of benefit to many types of workers. To the biologist and medical man, for whom it is primarily intended, it should prove of great use, in so far that it brings a great deal of matter within the covers of a single book, matter which will indicate to them the kind of problem which can be dealt with by quantitative methods, the methods to use, and, what is of great importance, how to use them. To the general statistician it serves the very useful purpose of indicating the type of problem which is met with in the fields of biometry and vital statistics. To the general reader there is a great deal of interesting information collated in the illustrative examples, which would well repay assimilation.

E. C. R.

ENGINEERING

The Inspection and Testing of Materials, Apparatus, and Lines. By F. L. Henley, Staff Engineer, G.P.O., London, M.I.E.E. [Pp. xi + 355, with 151 diagrams.] (London: Longmans, Green & Co., 1923. Price, 21*s.* net.)

Intended as a manual for post-office inspectors and contractors, the volume opens with a description of the systems of specification, purchase, and test, adopted by the Post Office. Then follows a résumé of the theoretical and applied strength of materials necessary to a clear understanding of the tests described.

The description of materials employed is very complete, ranging from the various metals and structural materials to porcelain, rubber, and allied products used in insulation. No attempt has been made to restrict detail in this section, and it forms a valuable reference when a specification is being drafted.

The telephone side of G.P.O. activity is given prominent treatment, several important tests being described complete, with circuit diagrams and test connections.

Later chapters are devoted to measurements at audio frequencies, line tests, and routine maintenance procedure, both for telephone and telegraph systems.

As an important accessory in repeaters, gain-measuring devices, etc., the thermionic valve justifies the thorough treatment accorded it. These chapters are supplemented by a mathematical appendix.

Produced for the service of those contracting for the Post Office, well provided with diagrams and reference-tables, all of a directly useful nature, and written from the inside point of view, the work cannot fail to be of great value. In condensing this broad field into a compact volume the author has made a judicious selection and treated it thoroughly.

A. N. Jackson.

Principles of Chemical Engineering. By W. H. Walker, W. K. Lewis, and W. H. McAdams. [Pp. ix + 637.] (London: McGraw-Hill Book Co., 1923. Price, 25*s.* net.)

The work under review is a valuable and much-needed contribution to the literature of chemical engineering in its scientific aspects. Chemical engineering, in the past, has been somewhat too much of an art and too little of a science. It will still remain an art to a considerable degree, but the authors have made a real contribution to the development of the subject as an exact science, and it is along these lines that the greatest progress in the design and operation of chemical plant and processes is likely to be made in the future.

The book deals only with the fundamentals; and those fundamentals are rightly assumed to be physical and physical-chemical in nature rather than mechanical or constructional. The book does not aim at being a treatise on chemical engineering, and it would be easy to point to considerable omissions, even of principles; the whole treatment, however, is stimulating and original in its form of presentation.

Starting with a chapter on Industrial Stoichiometry, which deals with the application of the stoichiometric and physical-chemical laws to the preparation of material and energy balance-sheets for industrial chemical processes, the authors pass to the dynamics of liquids and gases and the transference of heat. Both these subjects, which lie at the root of plant design, are treated at length and with great lucidity. Several original features are introduced, the derivation of a single equation for the expression of all types of liquid and gaseous flow being especially valuable. The theory of combustion and method of operation of a few typical furnaces, ovens, and gas-producers is

next dealt with in more descriptive manner and is followed by a somewhat sketchy treatment of the subjects of crushing, grinding, and mechanical separation.

The most valuable part of the book, after the sections on fluid and heat-flow, lies in the last seven chapters, which are devoted to the basic operations of filtration, evaporation, drying, distillation, humidification, and water cooling. Design of plant for each of these operations rests largely upon the laws of equilibria between phases and the rate at which such equilibria are attained. The authors give simple mathematical analyses of the various problems and show how the equations derived from first principles and certain assumptions can be employed in the design and operation of plant. Many of the equations so developed have been tested by the authors and their collaborators either on the laboratory or on the industrial scale, and although further experimental verification over a wider range of conditions appears desirable in many cases, the method of treatment is undoubtedly the right one, and must, as further data are secured, lead to great improvements in the design and operation of industrial plant.

The book is attractively written and produced, and will appeal to all chemical engineers and students who wish to approach the problems of chemical engineering from the fundamental scientific point of view. It can be recommended as probably the best work of its kind that has yet appeared; it is indeed unique in its own field.

E. C. W.

The Una-Flow Steam Engine. By PROF. DR. ING. H.C. J. STUMPF. Second Edition. [Pp. vii + 319, with numerous illustrations.] (London: Constable & Co., 1924. Price 20*s*.)

ONE perhaps feels on opening a book like the one under review that it will savour more or less of the trade-catalogue. The author has avoided this, and has gone into the theoretical problems connected with the engine in such detail, that the work must necessarily become a standard text on the subject.

The una-flow engine is certainly far more efficient than the usual reciprocator and is a serious competitor of the turbine up to about 1,000 h.p. size, although, previous to its development, the piston engine was not regarded as a rival of the turbine in sizes above 300 h.p. Probably the chief points which have led to the improved economy are reduction of initial condensation by the uni-directional flow, good jacketing of the cylinder head, and the use of single-beat inlet valves which provide a sharp cut-off. The large clear exhaust reduces the outlet losses and allows high vacua to be employed. The small clearance volume and high compression keep down the conducting surface and raise its temperature to nearly that of the incoming steam. All these points are thoroughly discussed in the earlier chapters.

It is possible that the theoretical considerations have been carried a trifle too far, however, in regard to the critical pressure of exhaust. It is suggested that too good a vacuum may actually increase the steam consumption. Prof. Josse is quoted, however, as stating "a further reduction in steam consumption due to increased vacuum was not noticeable." This is quite in order and in accordance also with turbine practice, where, after a certain vacuum is reached, any further increase does not materially improve the economy, but certainly does not make it worse.

The remaining chapters are given to the application of the engine to various practical drives, including the locomotive, and the designs of several different makers are indicated.

The illustrations are good, and the book should be read by all those interested in the reciprocator.

Ll. E.

Electrical Engineering Practice. By J. W. Meares, C.I.E., F.R.A.S., and R. E. Neale, B.Sc. Fourth Edition, rewritten and enlarged. Vol. I. [Pp. x + 589, with 94 figures.] (London: Chapman & Hall, 1923. Price, 25*s*. net.)

The complete work, in two volumes, of which this is the first, is intended to cover the general practice of electrical engineering.

Subdivided into three sections, it commences with a résumé of the acknowledged symbols and definitions in common use or adopted by international agreement. The fundamental properties of alternating currents are treated to some extent, and the properties of materials used in the industry lead up to consideration of methods of measurement.

Section II deals with the generation and sale of energy, points out the evils of low power factor, and the remedies available. A tabulated comparison of prime movers is incorporated and gives much useful information as to running costs, etc., for the more usual forms.

By means of this and similar tables in the other sections of the book one is enabled to compare rapidly the several alternatives available when a particular problem is being investigated. The design of water power schemes is compactly summarised, and possible development due to high speed runners of the Kaplan and Nagler types is indicated.

The chapters dealing with load and diversity factors present the fundamentals without unnecessary detail, while the section on costs and tariffs, clearly written, leaves one with the impression that unity of practice in this respect has been too long delayed.

Transmission and Control (Section III) provides a field which is of necessity restricted to broad treatment, but we find, as throughout the work, that the more immediately important practical branches are given precedence as far as detailed description is concerned.

The features desirable in switchgear are considered in some detail, the actual arrangements being shown by illustration of switchgear by representative markers.

In general the work presents the "pith" of the subject from the point of view of the practical man, while the comprehensive bibliography at the end of each chapter enables any particular item to be followed up if required. This key reference is perhaps the most valuable feature of the book, while the call for the fourth edition is its own testimonial.

A. N. Jackson.

Oil Engines. By A. L. Bird, M.A., A.M.Inst.C.E. [Pp. vi + 281, with 103 illustrations.] (London: Methuen & Co., 1923. Price 12*s*. 6*d*. net.)

It is probable that there is no branch of engineering in which so many developments and variations in design have arisen as in that covering the internal combustion engine. Even under the heading of oil engines, from which the petrol engine has been excluded, there are so many variants that few engineers outside the particular branch are *au fait* with all types.

The volume under review describes the general principles and discusses some of the special problems connected with these engines. A noteworthy feature of the text is that the mathematical work on these problems is closely interwoven with the practical considerations. Again, the volume has been kept within reasonable dimensions by the omission of the standard problems of strength calculations.

The first chapter is historical, giving a comparison of the Diesel and Ackroyd-Stuart cycles. Various theoretical cycles of operation are discussed in the next chapter, including the effect of variable specific heats. In comparison with these are given the cycles and indicator-cards of several actual engines. The testing of fuels for physical properties and their combustion

are dealt with in chapter iii, while the charging of the cylinder is fully discussed in chapter iv. Some interesting original work by Prof. Hopkinson and the author on this subject is also included. Chapter v is devoted to the various methods of fuel injection and ignition from both practical and theoretical points of view. In chapter vi, many tests are quoted, also useful characteristics, such as weights per horse-power of various types of machine. The only regret on reading this chapter is that little is said about the actual testing. The author might, for instance, have given us the benefit of his experience on indicators suitable for practical test work.

The last chapter deals with mechanical details, including air compressors, perhaps a trifle briefly in this case. For the Diesel engine proper the compressor is such an important organ that a discussion on the proportioning and the relative effect of actual clearances in the various stages of the machines would have been beneficial.

The volume is of decided merit, and, although intended primarily for engineering students, should appeal strongly to the trained engineer by assisting him to keep in touch with recent developments.

Ll. E.

An Introduction to the Study of Alternating Currents. By A. E. Clayton, D.Sc. [Pp. vii + 296, with 268 figures.] (London: Longmans, Green & Co., 1923. Price, 10s. 6d. net.)

The theory of alternating currents has been written and rewritten so often that one naturally expects a good deal of a modern textbook on this subject. In this treatise even the most critical will not be disappointed. The author's experience as a teacher is distinctly reflected in the clearness and conciseness of expression which characterises the whole book. One notices, however, a tendency to dodge the calculus, presumably to keep the reasoning within the understanding of the junior student, but there are many who consider the use of the more tedious alternative methods false simplicity.

It is to be regretted that the symbols, recommended by the International Convention are not strictly adhered to; for example, S is used for the number of turns instead of T. The author must know how easily misunderstanding is brought about by lack of uniformity in the use of symbols, and it is therefore surprising that he should depart from the only basis we have for international agreement. The general arrangement of the volume is excellent, the subject being continuously developed as we pass from one chapter to the next. A knowledge of the fundamental principles governing the theory of electricity and magnetism is assumed, and in the first pages the author deals with the generation of alternating electromotive forces and the mathematical and physical quantities employed in their analysis. The representation of alternating quantities by means of vectors is then considered, the method adopted for distinguishing between current, voltage and flux lines being distinctly useful. By this device much of the confusion liable to arise in a complicated diagram is avoided. The author points out that a vector diagram is generally constructed for virtual values, but it is perhaps not made quite clear that in this case the instantaneous values are not given directly by the vertical projection of the vector.

The various properties of circuits having resistance, inductance, and capacity are considered in considerable detail, and this is followed by a short chapter devoted to locus diagrams. Up to this stage no special mention is made of the influence of iron on an alternating magnetic field, and before treating the static transformer, a few pages are devoted to this part of the subject.

Polyphase currents and circuits, alternator windings and their characteristic wave-forms, the production of rotating magnetic fields, are each considered in turn. Before dealing with the induction motor, some of the

more practical problems in connection with alternators and synchronous motors are discussed, and in the short space of two chapters some very useful information is given. The treatment of the induction motor is perhaps rather brief in view of its practical importance. The alternating current commutator motor and repulsion motor are barely mentioned, but in omitting these in a book of this kind the author has shown a wise discretion. The concluding pages are devoted to the application of complex quantities to the solution of alternating current problems, and the treatise closes with a collection of very instructive examples on the syllabus covered. Throughout the book principles take first place, and detailed descriptions of special types of apparatus are entirely omitted. Thus the author has completely attained the object he had in view, namely to make fundamentals the primary consideration. The work should find a useful place among our best text-books on this subject.

H. Monteagle Barlow.

MISCELLANEOUS

Photography as a Scientific Implement. A Collective Work by A. E. Conrady, Charles R. Davidson, Charles R. Gibson, W. B. Hislop, F. C. V. Laws, J. H. G. Monypenny, Dr. H. Moss, Arthur S. Newman, Dr. Geo. H. Rodman, Dr. S. E. Sheppard, W. L. F. Wastell, Wilfred Mark Webb, and Colonel H. S. L. Winterbotham. (Applied Physics Series). [Pp. viii + 549, with 21 plates.] (London, Glasgow, and Bombay: Blackie & Son, Ltd., 1923. Price, 30*s.* net.)

In photography, as in other branches of scientific knowledge, the modern tendency is for specialisation and the issue of monographs dealing with particular aspects of the subject. These, however, are generally too highly specialised to be of any real value to those who wish to have a general knowledge of the guiding principles underlying the subject or to those concerned with the application of its methods. The present book provides the want in this direction. The fourteen chapters are written by thirteen different men, each of whom is a recognised authority on the subject with which he deals. A full consideration of each subject is not attempted, but in each chapter is brought together a mass of detail concerning methods of technique and underlying principles, which is normally hidden widespread in the scientific literature of the world.

The book fittingly opens with an exceptionally lucid consideration of the historical development of photography, full use being made of quotations from the writings of the pioneer workers. This chapter is followed by two chapters on the optics of photography, the first by Dr. Sheppard, who treats of the elementary side, and the second by Prof. Conrady, who deals with the subject from the view-point of the Abbe form of the general theory of lenses, and pays particular attention to the various faults to which lens systems are subject. The fourth chapter is the longest and most important in the book, and concerns the theory of photographic processes and methods. The author, Dr. Sheppard, is probably more competent to write on this question than any other worker, and, although his writings are usually very difficult to follow, the present one is a model of clarity of expression, and the reviewer knows of no more lucid account of the modern physico-chemical treatment of the subject. However, in writing chemical equations he seems to have gone sadly adrift, especially in that representing the reaction between ferrous sulphate and silver nitrate (p. 140) and those dealing with hypo-alum toning (p. 167). Also on page 131 we find that the "wet collodion" process is used in "photochemical" work, instead of in "photomechanical" work, and on page 148 we find 1 : 2 dihydroxy-benzene quoted as hydroquinone instead of pyrocatechol.

The important branch of astronomical photography is treated by C. R. Davidson, and Dr. Moss contributes an article on the application of photography in physics. Two chapters deal with photomicrography as applied to metallurgy and engineering, and to pathology and bacteriology respectively.

The most recent important applications of photography are in surveying and aeronautical photography, and two chapters on these branches are given by Colonel Winterbotham and Major Laws. Colour processes are discussed by Mr. Wastell, and Mr. Hislop deals with photography applied to printing processes. Chapters are also devoted to the "Technics of Kinematography" and "The Camera as a Witness and Detective."

The book is a very valuable one, and should be in the possession of all specialists in photography. It should also be of great interest to the general scientific public.

W. C.

The Story of a Great Schoolmaster. By H. G. Wells. [Pp. iv + 152, with 3 illustrations.] (London: Chatto & Windus. Price 4*s.* 6*d.*)

A sincere and deeply rooted friendship, accompanied by a keen intellectual appreciation of the ideals of Sanderson of Oundle, has undoubtedly aided Mr. Wells—if, indeed, his facile pen is ever in need of such inspiration—to write this vivid account of the life and ideals of a wonderful schoolmaster. So convinced was Sanderson of the rightness of his views as to the need for educational progress and reform to avoid social catastrophe that he devoted much of the last years of his life to teaching the outside world his belief that the Christian ideal to-day can only find true expression in a creative, co-operative, and non-competitive spirit. Like many another pioneer of human progress, Sanderson gave up his life for his ideals. All teachers, indeed all who have the responsibility of controlling the work of others, will do well to read this book and ponder well the problems that Sanderson has at least shown to be urgently in need of solution.

W. C. B.

BOOKS RECEIVED

(*Publishers are requested to notify prices*)

Traité Elémentaire des Nombres de Bernoulli. Par Niels Nielson, Professeur à l'Université de Copenhague. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1923. (Pp. x + 198.) Price 50 frs.

L'Analysis Situs et le Géometrie Algébrique. Par S. Lefschetz. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1924. (Pp. vi + 151.) Price 20 frs.

The Geometry of the Complex Domain. By Julian Lowell Coolidge, Ph.D., Professor of Mathematics in Harvard University. Oxford: at the Clarendon Press, 1924. (Pp. 242.) Price 21*s*. net.

An Elementary Treatise on Frequency Curves and their Application in the Analysis of Death Curves and Life Tables. By Arne Fisher. Translated from the Danish by E. A. Vigfusson. With an Introduction by Raymond Pearl. New York: The Macmillan Co., 1924. (Pp. xv + 244.)

Réception des Signaux Horaires Renseignements Météorologiques, Sismologiques, etc. Transmis par les Postes de Télégraphie sans Fil de la Tour Eiffel, Lyon, Bordeaux, etc. Publiés par le Bureau des Longitudes. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins. (Pp. ix + 226.) Price 27 frs. net.

World Weather, including a Discussion on the Influence of Variations of Solar Radiation on the Weather and of the Meteorology of the Sun. By Henry Helm Clayton. New York: The Macmillan Co., 1923. (Pp. xx + 393, with 265 figures and 15 plates.) Price 18*s*. net.

A Treatise on Light. By R. A. Houstoun, M.A., Ph.D., D.Sc., Lecturer on Physical Optics in the University of Glasgow. New Edition. London. Longmans, Green & Co., 39 Paternoster Row, E.C.4. (Pp. xi + 486, with 334 diagrams.) Price 12*s*. 6*d*. net.

Laboratory Experiments in Practical Physics to Accompany the Revised Edition of Black & Davis's "Practical Physics." By N. Henry Black, A.M., Science Master, Roxbury Latin School, Boston, Mass. London: Macmillan & Co., St. Martin's Street, 1924. (Pp. ix + 245.) Price 4*s*. 6*d*. net.

Nouvelles vues Faraday-Maxwelliennes. Par Charles L. R. E. Menges. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1924. (Pp. v + 93.)

Elementary Applied Mechanics. By T. Alexander, C.E., M.Inst.C.E.I., M.A.I., and A. W. Thomson, D.Sc. London: Macmillan & Co., St. Martin's Street, 1916. (Pp. xx + 512, with diagrams.) Price 15*s*. net.

Relativity for Physics Students. By G. B. Jeffery, M.A., D.Sc., Fellow of University College, London, and Professor of Mathematics at King's College, London. London: Methuen & Co., 36 Essex Street, W.C. (Pp. vii + 151, with 4 diagrams.) Price 6*s*. net.

Analytical Mechanics, comprising the Kinetics and Statics of Solids and Fluids. By Edwin H. Barton, D.Sc., F.R.S., F.R.S.E., Professor of Experimental Physics, University College, Nottingham. Second Edition, Revised and Enlarged. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1924. (Pp. xxi + 593.) Price 21*s*. net.

Hydrodynamics. By Horace Lamb, M.A., LL.D., Sc.D., F.R.S., Honorary Fellow of Trinity College, Cambridge. Fifth Edition. Cambridge: at the University Press, 1924. (Pp. xvi + 687.) Price 45*s*. net.

The Chemistry of Paints, Pigments, and Varnishes. By J. Gauld Bearn, M.Sc., A.I.C., F.C.S. London: Ernest Benn, 8 Bouverie Street, E.C.4, 1923. (Pp. x + 277, with 45 figures and plates.) Price 30*s.* net.

This book is written from a strictly practical standpoint and should prove of value to architects, engineers, and others whose calling necessitates the specification of materials for various protective uses; the descriptions of methods of analysis should also be of use to county analysts and others.

The Carbon Compounds. A Textbook of Organic Chemistry. By C. W. Porter, Associate Professor of Chemistry in the University of California. London and New York: Ginn & Company. (Pp. ix + 494.) Price 21*s.* net.

Lunge and Keane's Technical Methods of Chemical Analysis. Second Edition. Edited by Charles A. Keane, D.Sc., Ph.D., Principal of the Sir John Cass Technical Institute, London, and P. C. L. Thorne, M.A., M.Sc., Lecturer in Chemistry, the Sir John Cass Technical Institute, London. Volume I. London: Guerney & Jackson, 33 Paternoster Row; Edinburgh: Tweeddale Court, 1924. (Pp. xx + 704.) Price £3 3*s.* net.

Chemical Encyclopædia: A Digest of Chemistry and Chemical Industry. By C. T. Kingzett, F.I.C., F.C.S. Third Edition. London: Baillière, Tindall & Cox, 8 Henrietta Street, W.C.2, 1924. (Pp. viii + 606.) Price 30*s.* net.

Some Studies in Bio-Chemistry. By some Students of Dr. Gilbert J. Fowler, D.Sc., F.I.C., Indian Institute of Science. Bangalore: The Phœnix Printing House, 1924. (Pp. iv + 197.)

Chemicals. By A. W. Ashe and H. G. T. Boorman, A.I.C., with a Foreword by H.R.H. the Prince of Wales, K.G., and General Introductions by the Rt. Hon. Sir Eric Geddes, G.C.B., Sir Max Muspratt, Bart., and Robert Grosvenor Perry, C.B.E. London: Ernest Benn, 8 Bouverie Street, E.C.4, 1924. (Pp. xxvi + 207.) Price 21*s.* net.

A Dictionary of Applied Chemistry. By Sir Edward Thorpe, C.B., LL.D., F.R.S., Assisted by Eminent Contributors. Volume V, revised and enlarged Edition. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1924. (Pp. viii + 722, with illustrations.) Price 60*s.* net.

Elementary Crystallography. By John W. Evans, G.B.E., D.Sc., LL.B., F.R.S., F.G.S., and George M. Davies, M.Sc., F.R.S. London: Thomas Murby & Co., 1 Fleet Lane, E.C.4. (Pp. vii + 134.)

The Production of Field Crops: A Textbook of Agronomy. By T. B. Hutcheson, M.S., M.S.A., and T. K. Wolfe, M.S., Ph.D. London: McGraw-Hill Publishing Company, 8 Bouverie Street, E.C.4, 1924. (Pp. xv + 499, with 145 figures.) Price 17*s.* 6*d.* net.

A Manual of Land and Fresh-water Vertebrate Animals of the United States (exclusive of Birds). By Henry Sherring Pratt, David Scull Professor of Biology in Haverford College, Pa. Philadelphia: P. Blakiston's Son & Co., 1012 Walnut Street. (Pp. xv + 422, with 184 illustrations.)

The Peregrine's Saga, and other Stories of the Country Green. By Henry Williamson. London: W. Collins & Sons, 48 Pall Mall. (Pp. 301.) Price 7*s.* 6*d.* net.

Birds in Legend, Fable, and Folklore. By Ernest Ingersoll. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1923. (Pp. iv + 292.) Price 12*s.* 6*d.* net.

An Introduction to Oceanography, with Special Reference to Geography and Geophysics. By James Johnstone, D.Sc., Professor of Oceanography in the University of Liverpool. Liverpool: at the University Press. London: Hodder & Stoughton, 1923. (Pp. xii + 351, with 64 figures.) Price 15*s.* net.

The Marine Plankton: A Handbook for Students and Amateur Workers. By James Johnstone, D.Sc., Andrew Scott, A.L.S., and Herbert C. Chadwick, A.L.S., Department of Oceanography, University of Liverpool. With an Introduction by Sir William A. Herdman, F.R.S. Liverpool: at the University Press; London: Hodder & Stoughton, 1924. (Pp. xvi + 194.) Price 12*s.* 6*d.* net.

The Parathyroid Glands in Relation to Disease. By H. W. C. Vines, M.A., M.D., Fellow of Christ's College, Cambridge. London: Edward Arnold & Co., 1924. (Pp. viii + 127.) Price 10*s.* 6*d.* net.

Endocrin Glands in Health and in Disease. By Chandra Chakraperty. New York: Omin & Co., 1923. (Pp. 150.)

English Physicians of the Past: Short Sketches of the Life and Work of Linacre, Gilbert, Harvey, Glisson, Willis, Sydenham, Mead, Heberden, Baker, J. and P. M. Latham, Bright. By R. T. Williamson, M.D., F.R.C.P., Consulting Physician to the Royal Infirmary, Manchester. Newcastle-upon-Tyne: Andrew Reid & Co., 1923. (Pp. 96.)

Medicine, Magic, and Religion. The Fitzpatrick Lectures delivered before the Royal College of Physicians of London in 1915 and 1916. By W. H. R. Rivers, M.A., M.D., D.Sc., LL.D., F.R.C.P., F.R.S., with a Preface by G. Elliot Smith, F.R.S. London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Co., 1924. (Pp. viii + 147.) Price 10*s.* 6*d.* net.

A Biography of Eugenics. By Samuel J. Holmes, Professor of Zoology in the University of California. Berkeley, California: University of California Press, 1924. (Pp. iv + 514.)

Civilisation and the Microbe. By Arthur Isaac Kendall, Dean of North-Western University Medical School, Chicago. Boston and New York: Houghton Miflin Co., 1923. (Pp. xviii + 231, with 12 illustrations.) Price $2.50.

Selections from the Works of Ambroise Paré, with Short Biography and Explanatory and Bibliographical Notes. By Dorothea Waley Singer. Medical Classical Series. London: John Bale, Sons & Danielsson, 91 Great Tichfield Street, Oxford Street, W.1, 1924. (Pp. ix + 246.) Price 12*s.* 6*d.* net.

Human Physiology: A Practical Course. By E. G. Douglas, C.M.G., M.C., D.M., F.R.S., and J. G. Priestley, M.C., D.M. Oxford: at the Clarendon Press, 1924. (Pp. ix + 232.) Price 12*s.* 6*d.* net.

Sunshine and Open Air, their Influence on Health, with special reference to the Alpine Climate. By Leonard Hill, M.B., F.R.S., Director, Department of Applied Physiology, National Institute of Medical Research. London: Edward Arnold & Co., 1924. (Pp. vii + 132, with 8 plates.) Price 10*s.* 6*d.* net.

The Control of the Speed and Power Factor of Induction Motors. By Miles Walker, M.A., D.Sc., Professor of Electrical Engineering in the Faculty of Technology of the University of Manchester. London: Ernest Benn, 8 Bouverie Street, E.C.4, 1924. (Pp. 151, with 125 diagrams.) Price 18*s.* 6*d.* net.

Principles and Practice of Telephony: Volume IV—Circuit Refinements and Mechanical Switching. Volume V—Mechanical Manual Switching. By Jay G. Mitchell. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1924. (Pp. Volume IV, vii + 329, Volume V, vii + 263.) Price 12*s.* 6*d.* net each.

Théorie Générale sur les Courants Alternatifs. Par M. E. Piernet. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1924. (Pp. x + 100.) Price 12 frs.

Electrical Measuring Instruments: Part I—Commercial and Indicating Instruments. By C. V. Drysdale, O.B.E., D.Sc., M.I.E.E., and A. C. Jolley, A.M.I.E.E. London: Ernest Benn, 8 Bouverie Street, E.C.4, 1924. (Pp. 440, with 377 figures.) Price 55*s.* net.

The Modern Theory and Practice of Pumping: A Treatise on the Application of the Reynolds-Stanton Law of Viscous Flow to Modern Pumping Problems and the Flow of Liquids through Pipes. By Norman Swindon, M.I.Chem.I., A.M.I.Mech.E. London: Ernest Benn, 8 Bouverie Street, E.C.4, 1924. (Pp. 364, with 281 illustrations.) Price 42*s.* net.

Alternating-Current Armature Winding. By Terrell Croft. London: McGraw-Hill Publishing Company, 8 Bouverie Street, E.C.4, 1924. (Pp. ix + 352, with 326 figures.) Price 15*s.* net.

Automobile Repairing. By Ben G. Elliot, M.E., and others. London: McGraw-Hill Publishing Company, 8 Bouverie Street, E.C.4, 1924. (Pp. xii + 431, with 375 figures.) Price 15*s.* net.

Steel and Timber Structures. Compiled by a Staff of Specialists. Editors-in-Chief, George A. Hool, S.B., and W. S. Kinne, B.S. London: McGraw-Hill Publishing Company, 8 Bouverie Street, E.C.4, 1924. (Pp. xvi + 695.) Price 30*s.* net.

Principles of Electric Motors and Control. By Gordon Fox. London: McGraw Hill Publishing Company, 8 Bouverie Street, E.C.4, 1924. (Pp. xiv + 499.) Price 17*s.* 6*d.* net.

Management Engineering: the Design and Organisation of Industrial Plants. By P. F. Walker, M.M.E. London: McGraw-Hill Publishing Company, 8 Bouverie Street, E.C.4, 1924. (Pp. ix + 359.) Price 17*s.* 6*d.* net.

Principles of Electro-plating and Electro-forming. By William Blum and George B. Hogaboom. London: McGraw-Hill Publishing Company, 8 Bouverie Street, E.C.4, 1924. (Pp. xii + 356.) Price 20*s.* net.

A Textbook of Petroleum-Production Engineering. By Lester Charles Uren. London: McGraw-Hill Publishing Company, 8 Bouverie Street, E.C.4, 1924. (Pp. vii + 655.) Price 30*s.* net.

A Critical Examination of Psycho-Analysis. By A. Wohlgemuth, D.Sc. London: George Allen Unwin, 40 Museum Street; New York: The Macmillan Company. (Pp. 250.) Price 10*s.* 6*d.* net.

The Handbook of Sales Management: A Review of Modern Sales Practice and Management, Illustrated by the Methods and Experiences of Representative Selling Organisations. By S. Roland Hall. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1924. (Pp. ix + 995.) Price 25*s.* net.

Textbook of Industrial Cost Accounting. By Paul M. Atkins, M.A. London: McGraw-Hill Publishing Company, 6 Bouverie Street, E.C.4, 1924. (Pp. x + 396.) Price 17*s.* 6*d.* net.

Modern Theories of the Unconscious. By W. L. Northridge, M.A., Ph.D. With an Introduction by Professor J. Laird, M.A. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1924. (Pp. xv + 193.) Price 8*s.* 6*d.* net.

The Philosophy of Music. By William Pole, F.R.S., Mus.Doc. Sixth Edition. With an Introduction by Edward J. Dent and a Supplementary Essay by Hamilton Harbridge, M.A., M.D. London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Co., 1924. (Pp. xxiv + 342.) Price 10s. 6d. net.

Causation and the Types of Necessity. By Curt John Ducasse. Seattle: University of Washington Press, Feb. 1924. (Volume I, No. 2, pp. 69–200.)

Problems of Belief. By Ferdinand Canning Scott Schiller, M.A., D.Sc., Fellow and Senior Tutor of Corpus Christi College, Oxford. London: Hodder & Stoughton. (Pp. vii + 194.) Price 3s. 6d. net.

Spinoza, Descartes, and Maimonides. By Leon Roth, M.A., D.Phil., John Locke Scholar in Mental Philosophy in the University of Oxford; and Lecturer in Philosophy in the University of Manchester. Oxford: at the Clarendon Press, 1924. (Pp. 148.) Price 7s. 6d. net.

Visual Education: A Comparative Study of Motion Pictures and other Methods of Instruction. By Frank N. Freeman and Others. Chicago: University of Chicago Press. (Pp. viii + 391.)

Rural Highway Pavements Maintenance and Reconstruction. By Wilson G. Harger, C.E. London: McGraw-Hill Publishing Company, 8 Bouverie Street, E.C.4, 1924. (Pp. xvi + 636, with 211 figures.) Price 30s. net.

Economic Geography. By R. H. Whitbeck and V. C. Finch. London: McGraw-Hill Publishing Company, 8 Bouverie Street, E.C.4, 1924. (Pp. x + 558.) Price 17s. 6d. net.

Cambridge Readings in the Literature of Science: being Extracts from the Writings of Men of Science to Illustrate the Development of Scientific Thought. Arranged by William Cecil Dampier Whetham, M.A., F.R.S., and his Daughter Margaret Lampier Whetham. Cambridge: at the University Press, 1924. (Pp. x + 275, with 8 plates.) Price 7s. 6d. net.

Shirley Institute Memoirs. Volume II, 1923. Manchester: Shirley Institute. (Pp. vi + 394.)

Retail Advertising and Selling. By S. Roland Hall. London: McGraw-Hill Publishing Company, 8 Bouverie Street, E.C.4, 1924. (Pp. vii + 589.) Price 25s. net.

Valuation of Oil and Gas Lands. By Robert Wesley Brown, M.S. London: McGraw-Hill Publishing Company, 8 Bouverie Street, E.C.4, 1924. (Pp. viii + 215.) Price 15s. net.

An Introduction to the Study of Colour Vision. By Sir John Herbert Parsons, C.B.E., D.Sc., F.R.C.S., F.R.S. Second Edition. Cambridge: at the University Press, 1924. (Pp. x + 323.) Price 25s. net.

The Manufacture of Steel. By Frank T. Sisco. London: McGraw-Hill, Publishing Co., 6 Bouverie St., E.C.4, 1924. (Pp. x + 304.) Price 15s. net.

Constructive Conscious Control of the Individual. By F. Matthias Alexander, with an Introduction by Professor John Dewry. London: 36 Essex Street, E.C. (Pp. xxxiii + 317.) Price 10s. 6d. net.

The Subject-Index to Periodicals, 1920. Issued by the Library Association. K. Science and Technology. London: The Library Association, 7, Coptic St., W.C.1. 1923. (Pp. 231.) Price £1 5s. net.

The Nature of Laughter. By J. C. Gregory. London: Kegan Paul, Trench Trübner & Co., New York: Harcourt, Brace & Co., 1924. (Pp. v + 241.) Price 10s. 6d. net.

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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

PURE MATHEMATICS. By F. PURYER WHITE, M.A., St. John's College, Cambridge.

Function-theory.—A. Ostrowski (*Jahresber. D. Math. Verein*, **32**, 1923, 185–94) gives a report on general convergence theorems in the theory of functions of a complex variable. If an analytic function is regular in a region G and vanishes at all points of a subregion g, then it vanishes everywhere in G; moreover, for g we may substitute (1) a curve C lying wholly in G; (2) an infinite set of points with a limiting point in G; (3) if G is the unit circle an infinite set of points, which need not have a limiting point in G, if they do not lie too close to the circumference of the unit circle—the exact condition has been given by Blaschke; or (4) a set of points, lying on the boundary, which is measurable in Lebesgue's sense and has a positive measure. These propositions may be regarded as expressing the principle of analytic continuation for functional equations, *i.e.* for finite relations of analysis. Does the same principle hold for limit relations? In other words, if a sequence of analytic functions $f_n(z)$ which are regular in a region g converges uniformly to a function $F(z)$ which is regular in g, and if all the functions are regular in a region G enclosing g, can we deduce the uniform convergence of $f_n(z)$ to $F(z)$ in G? The answer is no; as is seen by the simple example of the sequence z^n, which converges to 1 only inside the unit circle. But the proposition is true if we add the Stieltjes condition that the $f_n(z)$ are uniformly bounded in G. Also it has been shown by Vitali and Blaschke that we can replace g by (1)–(4). (For an alternative proof to Blaschke's of (3) see Löwner and Rado, *Jahresber. D. Math. Verein*, **32**, 1923, 198–200). Is now the Stieltjes condition necessary? We can easily find sequences of functions with other properties which can be transformed into sequences which are uniformly bounded; we can use transformations of the theory of the elliptic modular functions, and there will thus arise, as is clear from the well-known proof of Picard's theorem, functions which cannot assume two fixed values. The idea originated with Boutroux and has been de-

veloped by Montel, and we finally have the theorem of Caratheodory and Landau that if the $f_n(z)$ are regular for $|z|<1$, converge for a set of points with a limiting point inside the unit circle and do not assume two fixed values a, b inside the unit circle, then the sequence converges everywhere within the unit circle. Ostrowski proceeds to sketch a method of extending these results, making use of the idea, which is capable of precise formulation, of the goodness of the convergence.

Invariants.—In a report on recent progress in the theory of algebraic and differential invariants, E. Noether (*Jahresber. D. Math. Verein.*, **32**, 1923, 177–84) shows how Hilbert's proof of Gordan's theorem on complete systems of invariants may be stated in the language of the theory of algebraic numbers and ideals and then discusses the questions of the actual formation of the basis in a finite number of steps and of finiteness for subgroups of the general linear group. With regard to differential invariants the author confines herself to the question of bringing them under the theory of algebraic invariants, which is done by the methods of the calculus of variations.

Binary Forms.—We have already had occasion to mention (Science Progress, **18**, 1924, 354) the work of Comessatti on the geometry of binary forms. He has now followed up his introductory paper on the rational normal curve and its projective groups by a further paper (*Math. Ann.*, **90**, 1923, 174–221) in which he begins an organic treatment of the whole theory of binary forms, independent of the classical theory. The representation of a covariant is obtained by a simply infinite system of varieties of the n-dimensional space, of which the coefficients of the binary form are the homogeneous coordinates and the x_1, x_2 are the parameters. The theorem of Roberts (1851) that a covariant is completely characterised by its leading term, and Faà di Bruno's deduction of the covariant from the leading term (1880) follow almost intuitively. We may then substitute semi-invariants for covariants, so that in many questions the variables are eliminated and we may restrict the group of transformations with regard to which invariant properties are to be considered. In the second part of the paper, Comessatti goes on to general methods for the determination of all the covariants of a binary n-ic, and is led to rational expressions of the covariants as functions of $n-1$ of them, the *typical representations* of Hermite and of Clebsch. The expression as rational *integral* functions, i.e. the theory of *complete systems*, seems, however, because of its more strictly arithmetical nature, to require the heavier methods of Gordan and Hilbert.

Following on an idea of Segre, A. Duschek (*Jahresber. D.*

Math. Verein., **32**, 1923, 234–9) shows how, in the case of the binary trilinear form, criteria for the occurrence of special cases may be obtained by simple geometrical considerations.

Geometry.—We welcome the republication (*Jahresber. D. Math. Verein.*, **32**, 1923, 97–119) of an article by M. Noether on Karl G. C. von Staudt (1798–1867), who gave to projective geometry "das Recht der Selbständigkeit und Unabhängigkeit."

The same volume also contains a notice of the work of Karl Rohn (1855–1920) by F. Schur (*ibid.*, 201–11), with a list of his papers, and one of Max Noether (1844–1921) by A. Brill (*ibid.*, 211–33).

Foundations of Geometry.—In the study of projective geometry one of the first steps, after the introduction of postulated (*uneigentlich*) elements to give unrestricted validity to the axioms of incidence, is to define a projectivity between two constructs of the first rank. This is usually done by means of a chain of perspectivities or by the invariance either of the general cross-ratio or in particular of the harmonic cast. An earlier definition, due to Steiner (1832), is that a projectivity is the relation between two constructs, originally in perspective, after they have been relatively displaced in any way. The objection to this method is, of course, that it assumes a knowledge of the properties of displacements, *i.e.* of metrically special collineations. It is, however, frequently used, as a first introduction to the subject, on pedagogic grounds, and so it is worth while pointing out, as has recently been done by R. Baldus (*Math. Ann.*, **90**, 1923, 86–102), that Steiner's definition does not agree with the others, and that not only in the complex but also in the real projective plane. For example, if *a*, *b*, *c* are three parallel straight lines, of which *b* is midway between *a* and *c*, and A, B, C are three points of a straight line, B not being the mid-point of AC, then it is impossible by a displacement to bring A, B, C into incidence respectively with *a*, *b*, *c*, although by the fundamental theorem a projectivity can be established between the pencil of lines and the range of points in which any three pairs of elements correspond. Baldus determines the limitations under which Steiner's definition agrees with the others, and formulates precisely theorems on the perspective orientation of projective constructs, with the consideration of complex constructs and complex displacements.

In the third and later editions of Hilbert's *Grundlagen der Geometrie* (not in the American translation) is an appendix dealing with the theorem of the equality of the base angles of an isosceles triangle (*Anhang* II). Among his axioms of congruence is the following:—III 6. If in the two triangles ABC and A′B′C′ the congruences AB ≡ A′B′, AC ≡ A′C′, ∠BAC ≡ ∠B′A′C′ hold, then the congruences ∠ABC ≡ ∠A′B′C′

and $\angle ACB \equiv \angle A'C'B'$ also hold. It is possible to take a narrower axiom in which the assumption is only made for triangles with the same direction of description. Since the ordinary deduction of the isosceles triangle property requires the congruence of symmetric triangles, it becomes a question whether the latter can be proved on the basis of this narrower axiom. Hilbert showed that this is so if all the other groups of axioms are kept, but that if the axioms of continuity are also given up, then the isosceles triangle property cannot be proved. For this purpose he sketches two "non-pythagorean" geometries in which the property does not hold. W. Roseman (*Math. Ann.*, **90**, 1923, 108–28) returns to the matter, with more detail and adding the proof that all the assumed axioms are actually satisfied in these two geometries. He also examines the validity of further theorems in Hilbert's geometries, and discusses what theorems can be proved from the first three groups of axioms, without the symmetry axiom, and which not. In Hilbert's geometries right angles exist and an angle can always be bisected; it remains doubtful whether these propositions follow from the axioms assumed or whether new geometries can be constructed, differing still more from the Euclidean, in which they no longer hold.

Conics.—In the German edition of Salmon's *Conics* there occurs the theorem that there is a central collineation between two conics, having as centre the point of intersection of two common tangents—such that both conics lie on the same side of each—and as axis a common chord. This is deduced from the theorem that if through the intersection of the common tangents of two conics we draw any pair of right lines, the chords of each conic joining the extremities of those lines will meet on one of the common chords of the conics" (5th English edn., p. 232). M. Pasch (*Math. Ann.*, **90**, 1923, 102–7) examines in detail the problem of relating two conics by a central collineation, showing that if the conics do not touch there are 12 solutions; if they touch in one point there are 7; for double contact there are 4, for osculation 1, and for four-point contact none. He also investigates the reality of the solutions, and applies his results to the problem of inscribing in a conic a triangle whose sides pass through three given collinear points.

The theorem, due to Miquel, that the foci of the five parabolas which touch fours of five straight lines lie on a circle, when generalised projectively and dualised becomes the theorem: If six arbitrary points 1, 2, 3, 4, 5, 6 be taken and the five conics passing respectively through the five points obtained by omitting in turn 1, 2, 3, 4, 5, then there exists a conic touching two arbitrary lines through the point 6 and triangularly inscribed to these five conics. It appears, moreover, as was

recognised by Wakeford, that the relation is symmetrical and that the conic obtained is also triangularly inscribed to the conic passing through the points 1, 2, 3, 4, 5. If the condition of touching the two arbitrary straight lines through the point 6 be omitted, we have a doubly infinite system of conics triangularly inscribed to the six conics passing through five of six points. It does not immediately appear how this family of conics depends upon the two parameters involved ; F. P. White (*Proc. Camb. Phil. Soc.*, **22**, 1924, 11–15) has now made a direct analytical investigation of the general symmetrical figure with a view to deciding this point. The results obtained can be put in a form which shows their connection with a geometrical proof of the theorem, due to H. F. Baker, which makes use of a construction in three dimensions, but which is as yet unpublished.

Geometry of the Triangle.—Among the " remarkable " points of a triangle, whose co-ordinates can be expressed rationally and symmetrically in terms of the co-ordinates of the vertices, three may be found from which all the others may be constructed with ruler and compasses. Expressed algebraically, given the aggregate of pairs of functions X, Y of the six variables x_i, y_i $(i = 0, 1, 2)$ which are rational, homogeneous, and of the first degree, which are unaltered by the permutations $S = (x_0, x_1, x_2)(y_0, y_1, y_2)$ and $T = (x_0, x_1)(y_0, y_1)$, and which are transformed by the group of displacements cogrediently with x and y; to find a minimal basis, i.e. three algebraically independent pairs of functions X, Y, from which all functions X, Y can be obtained by rational operations. S. Breuer (*Jahresber. D. Math. Verein.*, **32**, 1923, 119–32) carries this out, showing that the centroid of the triangle, the orthocentre and the centroid of the pedal triangle will serve as such a basis. He also applies the method to the " remarkable " points of a quadrangle.

Plane Curves.—A plane algebraic curve of order n can have at most $\frac{1}{2}(n-1)(n-2)+1$ real circuits ; this theorem is due to Harnack (1876), who sketched a method for constructing curves with this maximum number. In 1890 Hilbert gave a more far-reaching method. For $n=6$ Harnack had only given the case in which there is one circuit Z enclosing another, together with 9 external separate circuits. Hilbert in addition obtained the case in which Z encloses 9 separate circuits and there is one outside. Hilbert, however, confined himself to cases with the maximum number of " nested ovals " (*see* Hilton, *Plane Algebraic Curves*). A. Wiman (*Math. Ann.*, **90**, 1923, 222–8) has recently applied Hilbert's method to other cases. He proves, for example, that for n odd there are curves with the maximum number of real circuits, one odd and the others even, no one of the even circuits enclosing another. For

n even and curves in which only one circuit encloses others the method distinguishes, as regards the number enclosed, between $n \equiv 2$ (mod. 4) and $n \equiv 0$ (mod. 4), but it remains doubtful how far this is an essential difference. Wiman also proves, by a different method, that for any even order $2n$ there are curves with $\frac{1}{2}(n-1)(n-2)$ pairs of nested ovals and n^2 isolated circuits.

The plane quartic curves which pass through twelve fixed points g, of which no three lie on a straight line, no six on a conic, and no ten on a cubic, form a net of quartics represented by the equation $\lambda_0 S_0 + \lambda_1 S_1 + \lambda_2 S_2 = 0$. Any two curves of the net meet in four points besides in the points g, the sixteen points forming the base-points of a pencil of quartics contained in the net. If for any such pencil three of the four points lie on a straight line, then a similar property holds for every pencil of the net, the fourth point is fixed, and we have a net of a special character with *thirteen* base-points instead of twelve. For this to happen eleven of the twelve points g may be taken arbitrarily; there is then a single infinity of such nets, and the other two base-points are paired and lie on a definite hyperelliptic curve of order 7 which has the eleven points as double-points. Such nets of quartics arise in the mapping upon a plane, due to Clebsch, of a quintic surface with a double space cubic curve; plane sections are represented by quartics with eleven base points, and the hyperelliptic curve is the image of the double curve. This quintic surface again is perhaps best discussed as the projection of a sextic surface on four dimensions from a point of itself, the double curve arising from the trisecant of the surface which pass through the point. By considering similarly surfaces in space of higher dimensions, F. P. White (*Proc. Camb. Phil. Soc.*, **22**, 1924, 1–10) is led to nets of curves of any order with similar special properties.

H. W. Richmond (*Proc. Camb. Phil. Soc.*, **22**, 1924, 42–8) investigates the real space cubic curves which are geodesic upon a quadric; it is of course quite exceptional that geodesics, even of simple surfaces such as quadrics, should be algebraic.

Geometry of n Dimensions.—The theorem that if four arbitrary lines be taken in a plane the four circles circumscribed to the triangles formed by threes of these lines meet in a point can be generalised to space of any even number of dimensions; in the plane case the centres of the four circles lie on another circle passing through the point of concurrence of these four, and Kühne (*Crelle*, **119**, 1898, 186) has published a proof of an extension of the first half of this theorem to space of any even number of dimensions. H. F. Baker (*Proc. Camb. Phil. Soc.*, **22**, 1924, 28–33) points out that Kühne's proof is invalid, and gives a proof that the theorem in question is not true, except in the case of the plane.

The pedal property of the circumcircle of a triangle may be stated in the form: Given a circle, any four points upon it are so related that the feet of the perpendiculars from one upon the sides of the triangle formed by the others are collinear. H. W. Richmond (*Proc. Camb. Phil. Soc.*, **22**, 1924, 34–41 shows that curves of order n exist in Euclidean space of n dimensions on which any $n + 2$ points have a similar property, but that the curves cannot be real if n is odd.

R. Vaidyanathaswamy (*Proc. Camb. Phil. Soc.*, **22**, 1924, 49–53) determines, by enumerative methods, the number of lines which meet four linear subspaces of a space of n dimensions; in particular there are three lines which meet four planes in space of five dimensions.

Differential Geometry.—The student of differential geometry of three dimensions is very soon introduced to Bertrand curves, of which the principal normals are also the principal normals of another curve; as a generalisation we may consider two curves so related that the joins of corresponding points have constant, but different direction-cosines in the two principal trihedra. This was done by Demoulin, who, however, only considered special cases; more recently O. Perron (*Math. Zs.*, **4**, 1919, 231–70) and E. Salkowski (*Jahresber. D. Math. Verein.*, **30**, 1921, 91–9) considered pairs of curves in which the joins have the same direction-cosines. B. Spieweck (*Jahresber. D. Math. Verein.*, **32**, 1923, 271–85) adds the condition that the two trihedra shall have a constant relative position, i.e. that the tangents, principal normals, and binormals respectively make constant angles with each other, shows that, except for the helices, the principal normals must then be parallel, and obtains the extension of the Bertrand relation between curvature and torsion that must hold.

ASTRONOMY. By W. M. H. Greaves, M.A., Royal Observatory, Greenwich.

H and K Lines of Calcium in Early Type Stars.—J. S. Plaskett (*Monthly Notices R.A.S.*, vol. 84, no. 2, pp. 80–93, Dec. 1923) gives an account of the work which has been performed at the Dominion Astrophysical Observatory with regard to the radial velocities deduced from studies of the H and K lines in O-type stars. It had been noticed by previous observers that the radial velocities obtained from the H and K lines, and also from the sodium D lines in early B-type stars differed notably from the radial velocities obtained from other lines in the same stars, and it had been suggested that these H and K lines were due to interstellar clouds of calcium and sodium vapour. The chief objection to this hypothesis was the difficulty of understanding how, at the low temperature which would be

characteristic of such clouds, calcium vapour could be ionised so as to give rise to the H and K lines.

Further observational material is now provided by Dr. Plaskett's researches on the O-type and early B-type stars. In his paper the observational material is divided first of all into three classes, according to the position in the sky. In each of these classes there is a notable difference between the velocities from the H and K calcium lines and those derived from other lines. There is also a general agreement between the H and K velocities and the radial component of the reflex of the solar motion, although there are slight discrepancies which become more pronounced in the Perseus region. One of the subdivisions of the third group (Cygnus region) is devoted to the emission O-types or Wolf-Rayet stars. For these stars there are no sharp lines available for determinations of stellar radial velocities, but the narrow sharp H and K lines are present and the velocity determinations from these are in due agreement with the solar component, although there are slight individual discordances.

Three further groups are discussed. The first of these deals with a number of O-type stars which could not be included in the previous groupings. There are marked differences between stellar and calcium velocities, and small but probably real differences between the latter and the solar components. The fifth group brings together those stars for which the differences between stellar and calcium velocities are most marked.

The last group gives the results for a few of the brighter stars for which sodium D lines are available as well as calcium H and K. The stellar velocities differ notably from the calcium and sodium velocities, and the agreement between these last points to a common origin. It is pointed out that the D lines in these stars are very variable in character.

Proceeding to a discussion of his results, Dr. Plaskett points out first of all that they definitely negative the hypothesis that the calcium and sodium clouds are connected with the stars and are moving with them. He is then led back to the idea of diffuse and widely extended inter-stellar clouds with small space velocities, and points out that the small deviations of the cloud velocity for different stars may be due to small local motions in the clouds and are no greater than are known to exist in different parts of the Orion nebula. He also points out that the difficulty with regard to the ionisation of such a widely distributed tenuous cloud may be met by supposing that in the neighbourhood of high-temperature stars the calcium vapour may be ionised by the radiation from the stars themselves, and thus the conditions necessary for the production of

the H and K lines may be realised. The appearance of the sodium lines presents no difficulty.

In a further discussion Dr. Plaskett points out the connection between the above hypothesis and Hubble's conclusion that the luminosity of luminous nebulæ is due to excitation from stellar radiation. He also gives reasons explaining the absence of lines corresponding to other elements in the clouds.

Stellar Absorption Coefficients.—E. A. Milne in a paper entitled "Statistical Equilibrium in relation to the Photoelectric Effect, and its Application to the Determination of Absorption Coefficients" (*Phil. Mag.*, vol. 47, pp. 209–41, Jan. 1924) applies Einstein's method of probability coefficients to the determination of the absorption coefficient in a quantity of partially ionised gas. He shows that for small speeds the probability of electron capture must vary inversely as the square of the velocity of the electron, and gives reasons for suspecting that this law holds over a considerable range of velocities. Such an inverse square law had been previously deduced by Eddington from his theory of capture by collision with the nucleus. Assuming such a law of capture, Milne obtains a formula for the absorption coefficient which is substantially the same as the formula obtained by Eddington from the nuclear-capture theory, and only differs from Eddington's formula by a factor depending on the relevant ionisation potentials. The order of magnitude of these is determined as a function of pressure and temperature, and it is shown that, under typical stellar conditions, atoms of iron will have lost their L electrons, but will mostly retain their two K electrons.

The Relation between the Masses and Luminosities of the Stars.—A. S. Eddington (*Monthly Notices R.A.S.*, vol. 84, no. 5, pp. 308–32, March 1924) obtains from theoretical considerations a functional relation between the mass and bolometric absolute magnitude (with a small correction depending on temperature) of a star for which the equation for a perfect gas may be assumed to hold. On plotting out the masses and absolute magnitudes of various stars for which the necessary data are available, he finds that the representative points of all these stars lie approximately on the theoretical curve *regardless of whether they be giants or dwarfs.* It has been generally assumed that for dwarf stars of comparatively high density, the gas law would not hold, and consequently it would have been expected *a priori* that the representative points of such stars would lie considerably off the curve. Eddington shows that this is not the case. The mean density of the sun, for instance, is 1·4 times that of water, and yet we find its mass and luminosity closely satisfying a theoretical relation involving the equation for a perfect gas! The second

part of Prof. Eddington's paper is devoted to a discussion of the theoretical considerations thus opened.

In the first part of the paper Eddington derives the theoretical relation in question. Various theories of absorption coefficients in stars obeying the gas law lead to various relations connecting the absolute magnitude with the mass and effective temperature, but all these relations are very nearly the same, with the exception of a constant factor involved whose magnitude varies with different hypotheses. In the present paper this constant is derived from the mass, absolute magnitude, and effective temperature of the giant star Capella, and so the resulting relation is to a large extent independent of hypotheses as to the mechanisms of emission and absorption. The relation so obtained is:

$$L = \text{const} \times M^{7/5}(1-\beta)^{3/2}\mu^{4/5}T_e^{4/5}$$

$$\text{where } 1-\beta = 0{\cdot}00309\ M^2\mu^4\beta^4$$

In these formulæ L is the total radiation, M the mass (sun = 1), T_e the effective temperature, and μ the average molecular weight.

Eddington adopts the value $\mu = 2{\cdot}2$ and determines the constant factor from the data for Capella. He then prepares a curve relating M with the bolometric absolute magnitude m for an effective temperature $T_e = 5{,}200°$ A.

Good determinations of mass and absolute magnitude are available for eight components of double stars and less reliable ones for twenty-one double star components. On correcting for effective temperature and reducing the visual magnitudes to bolometric ones, the representative points are found to lie in all cases very close to the theoretical curve, and the divergences can to a large extent be attributed to errors of observation. One other representative point is found from six double stars in the Hyades, five more points are given by Cepheid variables (assuming the pulsation theory), and thirteen by eclipsing variables. These points all lie close to the theoretical curve. An investigation of the effect of an error in the assumed value 2·11 for the average molecular weight shows that the effect of such an error is small, and that the application of corrections for individual stars may possibly make the representative points fit the theoretical curve still more closely.

The theoretical formula then gives results in good agreement with observation, thus indicating that the equation for a perfect gas is true for most stars. Eddington suggests that the explanation lies in the high degree of ionisation prevalent in the stars. Any failure of the gas laws is due to the finite size of the molecules, and Eddington puts forward the view that the effective size of an atom for this purpose is determined by the orbit of the outermost electron. Under typical stellar

conditions the atoms must have lost several layers of electrons and their effective diameters will be of the order 10^{-10} cms. as against 10^{-8} cms. for un-ionised atoms. If this view is correct, a star with mean density below 1,000 grammes per cubic centimetre ought to behave as a perfect gas.

In this connection the companion of Sirius is of interest. The representative point of this star departs from the theoretical curve, its magnitude being too small. But if we take its effective temperature as 8,000° corresponding to its spectral type F_0, its density comes out to be about 53,000, and the theory could hardly be expected to apply. Some investigators have supposed that the effective temperature of this star is very low and is not in accord with its type. If this is the case, the visual magnitude will differ appreciably from the bolometric and it is possible that the necessary correction will put things right. o_2 Eridani is a similar star, but it agrees quite well with the curve. Eddington offers no explanation of this.

Eddington's results conflict to some extent with the giant and dwarf theory of evolution. According to this theory, each star changes its absolute magnitude continuously throughout the dwarf stage, and this can only be reconciled with the new results by supposing that the mass diminishes by radiation to an important extent. If this hypothesis is not accepted, the well-known statistical diagram of absolute magnitude and spectral type cannot be interpreted as indicating the course of evolution of a star, but rather as the locus of equilibrium points reached by stars of different initial mass.

Solar Research

Magnetic Polarities of Sunspots.—It seems now to have been definitely established that with the commencement of a new sunspot cycle there is a general reversal of the magnetic polarities of sunspot groups. Such a reversal was suspected at the minimum of 1912. A summary of the Mount Wilson observations of sunspots for September and October 1923 is given in *Publications of the Astronomical Society of the Pacific*, vol. 35, no. 208, p. 330, Dec. 1923. Of nine spot groups observed in these months, six were in high latitudes and must be identified as belonging to the new cycle. The three low-latitude groups had polarities characteristic of the old cycle, while all but one of the high-latitude groups had the opposite distribution of polarities.

Occurrence and Distribution of Faculæ in Solar Latitude.—A paper on this subject from the Royal Observatory, Greenwich, appears in *Monthly Notices R.A.S.*, vol. 84, no. 2, pp. 96–9, Dec. 1923. It is found that the observed mean daily areas of faculæ provide a very good criterion of solar

activity, and on plotting these areas against the time, the resulting curve is very similar to the sunspot curve. Furthermore faculæ, like spots, appear in high latitudes at the commencement of a cycle, and the mean latitude of facula areas decreases as the cycle progresses. A very interesting diagram illustrative of this tendency is given, which is prepared in the same way as Maunder's "butterfly" diagram. Various other phenomena are commented on in the paper.

Rotation Period of the Sun.—Another paper from the Royal Observatory, Greenwich, appearing in *Monthly Notices R.A.S.*, vol. 84, no. 6, pp. 431–42, April 1924, gives a determination of the rotation period of the sun derived from measures of faculæ. Only recurrent faculæ were used, the measures being made at intervals of approximately one solar rotation. In order to facilitate identification, the examination was chiefly restricted to years of lesser solar activity. Denoting by the mean daily sidereal motion thus obtained, the formula $\xi = 14^\circ{\cdot}49 - 1^\circ{\cdot}78 \sin^2\phi - 3{\cdot}16 \sin^4\phi$, where ϕ denotes heliographic latitude, is found to fit the observations very well.

Life-history of Bright Solar Calcium Flocculi.—A paper on this subject by C. P. Butler appears in *Monthly Notices R.A.S.*, vol. 84, No. 3, pp. 134–49, Jan. 1924. Butler finds that flocculi appear suddenly, elongate (usually with inclination equatorward to the west, but sometimes parallel to the solar equator or in a few cases equatorward to the east), and disperse gradually at each successive return until finally the area becomes indistinguishable from the solar reseau. There are no cases of the sudden disappearance of flocculi. Very often, but not always, spots develop in a flocculus area. There are no instances of spots occurring without some accompanying flocculus.

PHYSICS. By J. Rice, M.A., University, Liverpool.

Magnetism and Magneto-Optics.—For many years now it has been evident that our information concerning the magneto-optic properties of matter is very restricted, owing to the comparative weakness of the magnetic fields which can be produced in a physical laboratory. In the main experimenters have relied on the use of solenoids with an iron core, and in this way it has been possible to obtain fields of a magnitude 60,000 gauss in a volume of a few cubic millimetres. It is practically impossible to go beyond this limit by this method, owing to the fact that at this stage the iron becomes magnetically saturated. There is no reasonable ground for believing that we can obtain any ferromagnetic substance whose saturation value is higher than the iron used in the cores of modern electro-magnets, and

without such a material we cannot hope to increase the fields which can be obtained by such means.

If we choose to dispense with an iron core, two causes operate to keep down the values of the magnetic fields obtainable with an uncored solenoid. One is the difficulty of obtaining sufficiently powerful sources of electric energy; the second is the tendency for the coil to overheat. These factors are so important that it is difficult to obtain by this method fields stronger that 20,000 gauss, even when the sources of energy are extremely powerful and very efficient means for cooling the solenoid are employed. The use of liquid air as a cooling agent has been suggested, and this would certainly prevent overheating very effectively and would also diminish the power required, since at such a low temperature the conductivity of copper is considerably increased. But the factor of expense comes in; for it has been calculated that to produce a field of 100,000 gauss inside a coil of 1 centimetre internal diameter, one would require a source of energy of 400 kilowatts power and a supply of liquid air at a rate of 24 litres per second—scarcely a feasible proposition. Thus the production of very strong magnetic fields by any of the usual methods seems a hopeless task.

An entirely new turn has, however, been given to this branch of physical research, and possibilities, almost of a sensational nature, are foreshadowed by the publication of some experiments carried out by P. L. Kapitza in the Cavendish Laboratory at Cambridge. The paper appeared in the *Proc. Roy. Soc.* of last June, and there is little doubt that investigators not only in magneto-optics but also in such lines as high-temperature and high-pressure work, optical and X-ray flashes, and sudden powerful discharges in gas tubes, will presently be availing themselves of Kapitza's suggestions and designs.

Kapitza points out that for nearly all the phenomena which we wish to study, such as magneto-optics, paramagnetism and diamagnetism, and other effects of atomic origin, we only require to produce the magnetic field for a very short time, say the hundredth of a second, as these effects will exhibit themselves in fields of short duration as well as in permanent fields. No doubt the methods of observing the effects will have to be modified, and some thought and ingenuity will be required to overcome the difficulties raised thereby; but the enormous advantage of minimising the dangers of overheating will amply repay the work expended in this direction. In extremely strong fields, even of short duration, the scale of effects will be so much increased and observation will be all the easier.

Actually, Kapitza has produced fields of half a million gauss lasting for a small fraction of a second and has carried out

some preliminary experiments for "coiling up" α-ray tracks in a Wilson expansion chamber (described separately in *Proc. Camb. Phil. Soc.*, vol. 21, p. 511). The Royal Society paper quoted above gives an account of the appliances used for the actual production of the field. In the development of the method four separate problems had to be solved: (1) the design of a powerful source of energy; (2) the design of switches and connections for dealing with the high currents involved (of the order 10,000 amperes); (3) the accurate measurement by means of oscillographs of these heavy currents; (4) the construction of the most efficient type of coils for producing the field.

The problem of storing the energy which has to be discharged in order to obtain the magnetic field is one which might be solved in a variety of ways. A large plant of electro-generators of the usual type would be possible, but scarcely suitable when the energy is only required for a small fraction of a second. A condenser battery is another obvious suggestion, and it has the advantage that the ratio of the power at which it is charged to the power at which it is discharged is comparatively large; but it labours under the serious disadvantage that it is difficult in such a way to store a large amount. Thus to store 10,000 joules (actually required in some of Kapitza's experiments) would have necessitated a condenser battery of about 20 microfarads charged to 50,000 volts. It might be possible also to store energy magnetically, using an iron core magnetised by a current in a primary coil. On breaking the primary current it would be possible to obtain in a secondary winding with a few turns a current strong enough to produce an intense magnetic field for a brief period. This method was actually tried, but in practice it was impossible to break the primary circuit with sufficient rapidity to prevent sparking, so that much of the energy, instead of going into the secondary, returned through the spark into the main circuit. Mechanical accumulation is also a feasible method, in which the kinetic energy of a fast-spinning flywheel could be converted by connection with an electro-generator of special type and the sudden switching in of a small resistance. Expense is a serious matter, however, in such a device.

In the end Kapitza has used chemical storage. Batteries of lead-plate accumulators of a special design were employed. These were constructed to be of very small capacity, small resistance, and of a design so rigid as not to be damaged seriously by sudden discharges. These batteries could be charged from 220-volt mains at 2 or 3 amperes, and yet could be safely discharged through ·02 ohm. at a power of 1,000 kilowatts, most efficient discharge taking place with a current as great as

7,000 amperes, while twice this could be obtained by short-circuiting. Special instructions concerning the making and care of these cells are given in the paper. The remarkable amount of energy yielded by such an accumulator battery in a fraction of a second can be demonstrated by the sudden fusing of copper wire or even the blade of a Gillette razor by a rapid discharge through it, the experiment going off in a truly sensational manner with a loud report and an intense flash of light. The switch gear for producing a current of the order 10,000 amperes in ·01 second required some care and ingenuity of design also. Also special oscillographs of the Duddell type had to be made to measure the brief currents obtained in the coil producing the magnetic field. For the details and those of the special coil designed to give the greatest field with the current available the reader is referred to the paper.

There is little doubt that these researches are going to have an important effect on laboratory practice in the lines of work referred to above. Already, after a year's experience, Kapitza declares it will be quite possible to produce a field of 2,000,000 or 3,000,000 gauss in a coil of 1 mm. internal diameter. There are at present, for example, quite a number of interesting points about the anomalous Zeeman effect and its relations with the normal (i.e. theoretically predicted) effect which would be cleared up at once with the possibility of such a field well established.

X-Rays.—In the March number of the *Physical Review*, Prof. Jauncey gives a further paper on the corpuscular quantum theory of the scattering of X-rays referred to in Science Progress, April 1924 (No. 72). Jauncey applies the theory here to the scattering of polarised X-rays. The main result is a proof that in the direction of the electric vector the intensity of scattering is not zero, as it should be on classical lines. Taking the polarising angle as that angle of scattering which gives completely polarised rays from unpolarised primary rays, it appears that it is less than 90° for short wave-lengths, approaching 90° as the wave-length increases.

Compton and Hubbard contribute a paper to the April number of the *Physical Review* on a similar topic, in which the recoil of electrons from scattered X-rays is treated, first on the corpuscular and quantum hypothesis and then on classical lines, and it is demonstrated that the conclusions derived from the first postulate are much nearer the facts than those derived from the second.

In the May number of the same journal Boyce gives results of some experiments on the excitation of soft X-rays from the heavy elements, tantalum to gold, by electron bombardment at voltages between 30 and 1,500 volts, and finds fair agreement

with the predictions of Bohr and Coster concerning the ionisation potentials of orbits of the N-energy level. In the July number there are two papers, one by Crofutt on measurements of absorption co-efficients of homogeneous X-rays, and the second by Nolan on the K and L absorption and emission spectra of tungsten.

In the April *Phil. Mag.* Bradley gives the results of investigations by the use of the powder method, a Shearer X-ray tube with copper anticathode and a nickel filter on the crystal structure of metallic arsenic. In general it resembles that of antimony and bismuth, consisting of two interpenetrating rhombohedral space lattices, the amount of penetration being determined from the relative intensities of the various lines on the photographic film.

Thermionics.—In the *Proc. Roy. Soc.* for April Prof. Richardson deals with some applications of thermodynamics to the electron atmospheres in equilibrium with matter. It develops earlier theoretical papers of his, and this extension is required because recent improvements in technique are enabling some of the finer points of the theory to be tested by experiment. An interesting point of the paper deals with a universal constant connected with thermionic effects whose existence had been asserted by the author of the paper nearly ten years ago. A short paper in the *Phys. Rev.* of February by the same author also deals with this constant (viz. A in the well-known equation $i = AT^2e^{-b_0/T}$), and criticises an earlier paper of Dushman (*Phys. Rev.*, **21**, 623, 1923) dealing with the work-function in the equation. A short reply from Dushman is printed in this number. To the July number of the *Phys. Rev.* Prof. H. A. Wilson also contributes a theoretical paper dealing with similar questions, and points out how the results throw light on the vexed question of the connection between the chemical constant and size of the " region-element " in the quantum theory of the chemical constant, the conclusion being in favour of Sackur's views on this subject.

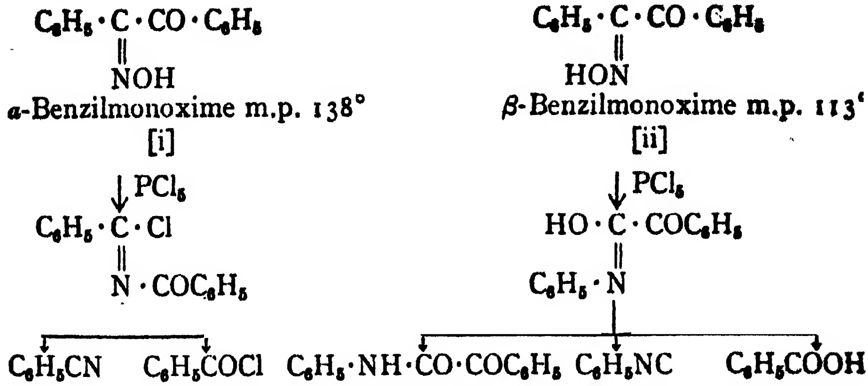
Radioactivity.—To the *Phil. Mag.* of March Drs. Kirsch and Pettersson contribute some preliminary results of experiments still in progress on the artificial disintegration by bombardment with α-particles from radium C of atoms of the elements Sc, Va, Co, and As. H-particles of a range exceeding 17 cm. of air expelled from the atoms are certainly less numerous than one-twentieth of the particles expelled from aluminium. Short-range particles are given off from beryllium, magnesium, and silicon. The evidence for the disintegration of lithium is less conclusive.

In the February number of the *Phil. Mag.* Sir Ernest Rutherford deals with the experimental results obtained by

Henderson using the photographic-magnetic field method (*Proc. Roy. Soc.*, A 102, 496, 1922), and by himself using the scintillation method (this paper) on the capture and loss of electrons by α-particles. The question at issue is how far α-particles can capture electrons in the early parts of their range. No one apparently suspected the possibility of this before Henderson's results were published, and Rutherford's experiments attempt to test Henderson's conclusions by another method and find general agreement. But in the discussion which follows the account of his experiments the author points out that many points in the theory must remain obscure and interpretation of experimental results doubtful until we have more information concerning the mechanism of the capture of electrons by charged atoms. There appears to be little doubt, however, that swift α-particles can capture an electron. α-particles which have captured two electrons and thus become neutral helium are only present to a noteworthy extent at the lowest velocities which can be examined by the scintillation method. But in a beam of more swiftly moving particles whose velocity is determined by the thickness of foil which it has penetrated and emerged from, there is a definite mixture of α-particles and α-particles which have captured one electron, the ratio of mixture depending in the main on the velocity and only to a slight extent on the nature of the foil. This ratio is thus a rapidly varying function of the velocity and almost independent of other conditions. In the same number of the *Phil. Mag.* R. H. Fowler contributes a theoretical discussion of the phenomenon based on thermodynamical and statistical considerations. He shows that the existence of this ratio probably arises from an equilibrium between the various constituents involved of the nature of a thermodynamic equilibrium, corresponding to thermodynamic conditions in which the electron density is about 2×10^{24} per c.c. (equal to that of the lightly bound electrons in the interior of an atom), and at a temperature at which electrons would have an average velocity equal to that of the α-particle. (This would be nearly 6,000,000° C. for the initial velocity of α-particles from RaC.) Capture and loss is probably a collision-mechanism and non-radiative in character.

ORGANIC CHEMISTRY. By O. L. Brady, D.Sc., F.I.C., University College, London.

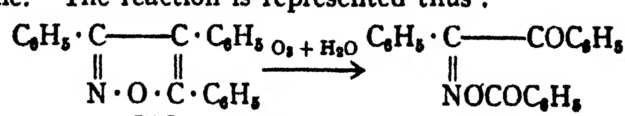
The Beckmann Transformation.—Meisenheimer and his coworkers (*Ber.*, 1921, **54** [B], 3195, 3206; 1924, **57** [B], 276, 282, 289) have made an interesting and important contribution to the study of the problem of the Beckmann Transformation.



The α- and β-Benzilmonoximes have previously been assigned the formulæ [i] and [ii] on account of their behaviour when they undergo the Beckmann change, the former giving benzonitrile and benzoyl chloride and the latter benzoylformanilide, phenylcarbylamine, and benzoic acid. The assumption was that the oximino-hydroxyl group exchanged places with the neighbouring group attached to the carbon atom ; Meisenheimer now suggests that this is incorrect and that the transformation takes place between the groups in the *anti*-position to one another thus :

$$\begin{array}{c} R\cdot C\cdot R^1 \\ \| \\ NOH \end{array} \rightarrow \begin{array}{c} HO\cdot C\cdot R^1 \\ \| \\ NR \end{array}$$

He has found that 3:4:5-triphenyliso-oxazole [iii], on treatment with ozone followed by water, gave benzoyl-β-benzil monoxime [iv], which could be hydrolysed to β-benzil-monoxime and could be regenerated by benzoylation of the β-oxime. The reaction is represented thus :



[iii] [iv]

The structure [iv] is that which, previously, would have been given to benzoyl-α-benzilmonoxime, but Meisenheimer considers that this reaction, the normal addition of ozone to the double bond, would not bring about isomeric change, and that consequently the formulæ for the benzilmonoximes must be interchanged ; it follows accordingly that the Beckmann Transformation occurs in the *trans* and not the *cis* direction.

In this connection it was necessary to investigate the benzoyl derivative of the α-oxime in order to study its stability, but here unfortunately some difficulty arises. Meisenheimer in

1921 thought that the action of benzoyl chloride in pyridine on the α-oxime gave rise, not to the benzoyl-derivative of the oxime, but to the Beckmann transformation product ON-dibenzoyl-isobenzamide [v], following Werner and Piguet (*Ber.*, 1904, **37**, 4295), since the compound on hydrolysis with sodium hydroxide gave not the oxime, as happens with acetyl-α-benzilmonoxime, but benzonitrile and benzoic acid :

$$\begin{array}{l} C_6H_5\cdot C\cdot O\cdot COC_6H_5 \\ \qquad \| \\ \qquad NCOC_6H_5 \end{array} \longrightarrow C_6H_5CN + 2C_6H_5COOH$$

[v]

This year, however, the same compound has been obtained by the action of benzoic anhydride on the α-oxime and has been found to reduce with zinc dust and acetic acid to deoxybenzoin, $C_6H_5 . CO . CH_2 . C_6H_5$ in the same manner as the α-oxime itself; it is considered, therefore, to be the true benzoyl-α-benzilmonoxime.

Meisenheimer suggests the following interpretation of the Beckmann Transformation :

$$\begin{array}{ccccccc} R\cdot C\cdot R^1 & & R\cdot C & & R\cdot C\cdot OH & & R\cdot C:O \\ \| & \longrightarrow & \| & \longrightarrow & \| & \longrightarrow & | \\ HO\cdot N & & HO\cdot NR^1 & & NR^1 & & NHR^1 \\ \text{[vi]} & & \text{[vii]} & & \text{[viii]} & & \text{[ix]} \end{array}$$

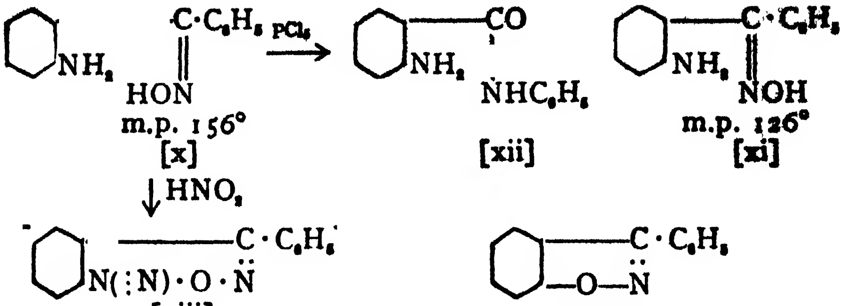
In the compound [vi] the aryl group R exerts an attraction on the hydroxyl group, displacing it somewhat, accordingly the residual affinity of the nitrogen atom on the side remote from the hydroxyl group is strengthened and in certain circumstances becomes so great that the group R^1 is attracted to it with the momentary formation of a compound [vii] containing quadrivalent nitrogen and tervalent carbon ; the hydroxyl group has little affinity for nitrogen and is therefore eliminated, naturally becoming attached to the tervalent carbon [viii], tautomeric change finally taking place with the formation of the amide [ix].

If the above conclusions are accepted, it becomes necessary to modify the formulæ of the benzildioximes, which are now given the structures :

$$\begin{array}{ccc} C_6H_5\cdot C{-}C\cdot C_6H_5 & C_6H_5\cdot C{-\!\!-\!\!-\!\!-}C\cdot C_6H_5 & C_6H_5\cdot C{-\!\!-}C\cdot C_6H_5 \\ \|\quad\;\; \| & \|\qquad\qquad \| & \|\quad\;\; \| \\ HON\;\; NOH & NOH\quad HON & NOH\; NOH \\ \alpha\ \text{m.p. } 238^\circ & \beta\ \text{m.p. } 207^\circ & \gamma\ \text{m.p. } 163^\circ \end{array}$$

Meisenheimer discusses these new configurations in the light of anhydride formation.

Some confirmation of the above views is afforded by the behaviour of the two forms of *o*-aminobenzophenone oxime to which Meisenheimer gives the formulæ [x] and [xi].

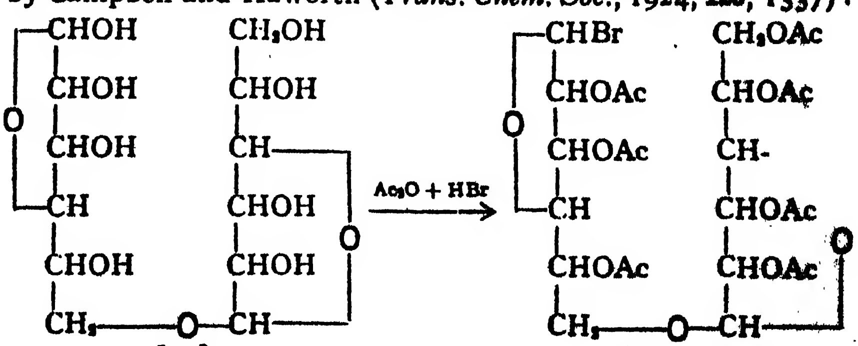


[x] [xii] [xi] [xiii] [xiv]

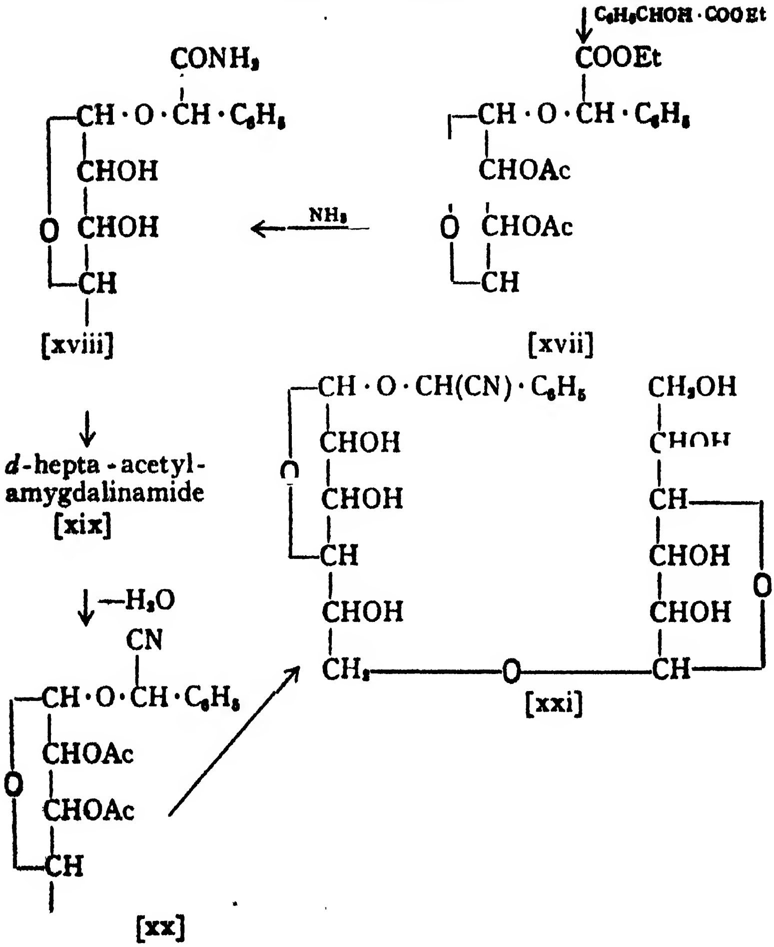
In the Beckmann change [x] gives the anilide of anthranilic acid [xii], the conversion occurring in the *trans* direction; [xi] does not give a crystallisable product. The evidence in favour of assigning a structure to [x] in which the hydroxyl group is in proximity to the amino group is that this compound, when treated with nitrous acid, very readily passes into the indoxazine [xiv], probably through the diazo-anhydride [xiii], whereas the compound [xi] does not give this reaction.

Though Meisenheimer's suggestions are of great importance, much more evidence is required before finality is reached. The work on the benzoyl-benzilmonoximes is not entirely immune from criticism, whilst the behaviour of the *o*-aminobenzophenone oximes could be explained in another way employing the older ideas.

The Synthesis of Amygdalin.—The structure of the sugar residue in amygdalin has been determined by Haworth and Leitch (*Trans. Chem. Soc.*, 1922, **121**, 1921) and the identity of amygdalin biose with gentiobiose established by Haworth and Wylam (*ibid.*, 1923, **123**, 3120; compare also Zemplen, *Ber.*, 1924, **57** [B], 698). Hudson (*J. Amer. Chem. Soc.*, 1924, **46**, 483) by a different path has come to the same conclusion. The important synthesis of amygdalin has now been achieved by Campbell and Haworth (*Trans. Chem. Soc.*, 1924, **125**, 1337):



[xv] [xvi]



Gentiobiose [xv], synthesised by Bourquelot, Hérissey, and Coirre (*Compt. rend.*, 1913, **157**, 732) by the action of emulsin on glucose, was brominated and acetylated by solution in acetic anhydride saturated with hydrobromic acid; the hepta-acetyl-β-bromogentiobiose [xvi] formed was condensed with ethyl dl-mandelate in the presence of silver oxide giving hepta-acetyl-dl-amygdalinic ester [xvii] which was converted by alcoholic ammonia into amygdalinamide [xviii]. Reacetylation in the presence of pyridine gave hepta-acetylamygdalinamide, containing one molecule of combined pyridine; this was resolved by fractional crystallisation and the d-hepta-acetyl-amygdalinamide [xix] dehydrated by boiling in xylene with phosphoric oxide giving d-hepta-acetylamygdalin [xx] which

was deacetylated by the method of Fischer and Bergmann (*Ber.*, 1917, **50** [B], 1065) giving amygdalin [xxi].

The Decomposition of Alkyl Hypochlorites.—The little-known alkyl hypochlorites have been further investigated by Chattaway and Backeberg (*Trans. Chem. Soc.*, 1923, **123**, 2999). The primary and secondary alkyl hypochlorites are very unstable and explode when exposed to bright light; even in the dark they decompose vigorously at the ordinary temperature giving aldehydes and ketones.

$$R\cdot CH_2OCl \rightarrow R\cdot CHO + HCl \qquad R_2CHOCl \rightarrow R_2CO + HCl$$

The tertiary alkyl hypochlorites are more stable, but when exposed to bright sunlight decompose to give ketones and other products not yet identified.

$$R_3COCl \rightarrow R_2CO + RCl$$

Except for the rather dangerous properties of these compounds, they provide a method for the preparation of aldehydes and ketones which may prove useful, since the hypochlorites can be obtained in almost theoretical yield from the well-cooled alcohol, sodium hydroxide, and chlorine in the absence of light.

The same authors (*Trans. Chem. Soc.*, 1924, **125**, 1097) have shown that ethyl hypochlorite is the first product in the formation of chloral from alcohol and chlorine and have re-interpreted the course of this well-known reaction.

$$CH_3\cdot CH_2OH \xrightarrow{Cl_2} \underset{[xxii]}{CH_3\cdot CH_2OCl} + HCl \rightarrow CH_3\cdot CHO + HCl$$

$$\rightarrow (CH_3\cdot CHO)_3 \rightarrow (CH_2Cl\cdot CHO)_3 \rightarrow CH_2Cl\cdot CHO \rightarrow \underset{[xxiii]}{CH_2Cl\cdot CH(OH)OEt}$$

$$CH_2Cl\cdot CH(OH)OEt \begin{array}{l} \nearrow \underset{[xxv]}{CH_2Cl\cdot CHCl\cdot OEt} \searrow \\ \searrow \underset{[xxiv]}{CH_2Cl\cdot CH(OEt)_2} \nearrow \end{array} \rightarrow \underset{[xxvi]}{CHCl_2\cdot CHClOEt} \rightarrow$$

$$\underset{[xxvii]}{CHCl_2\cdot CH(OH)\cdot OEt} \rightarrow \underset{[xxviii]}{CCl_3\cdot CH(OH)OEt}$$

The ethyl hypochlorite [xxii] first formed decomposes to give acetaldehyde, which in the presence of the hydrogen chloride also formed polymerises to paracetaldehyde; this suffers further chlorination giving *s*-trichlorparacetaldehyde which partly depolymerises to monochloracetaldehyde which combines with the alcohol present to form the alcoholate [xxiii]. The alcoholate reacts further either with alcohol to form monochloracetal [xxiv] or with the hydrogen chloride, now present in considerable quantity, to form dichlorethyl ether [xxv]; both these compounds on further chlorination give

trichlorethyl ether [xxvi]. At this stage the reaction mixture separates into two layers, the bottom consisting of trichlorethyl ether and the top of aqueous hydrogen chloride. On warming, the liquid again becomes homogeneous, the trichlorethyl ether reacting with the water to form dichloracetaldehyde alcoholate [xxvii] which is slowly chlorinated to chloral alcoholate [xxviii], the final product of the reaction.

GEOLOGY. By G. W. Tyrrell, F.G.S., A.R.C.Sc., Ph.D., University, Glasgow.

Sedimentary Rocks and Sedimentation.—Dr. W. Mackie has directed attention to the prevalence of a particular type of zircon, characterised by a purple colour of varying intensity, and by rounded or ovoid shapes, in many of the sedimentary rocks of Scotland ranging in age from the Dalradian to the present day (*Trans. Edinburgh Geol. Soc.*, **11**, pt. 2, 1923, pp. 200–13). The source of these zircons has now been identified in the Lewisian Gneiss of the north-west of Scotland. Purple zircons have also been found by Dr. Mackie in specimens of Canadian Archæan gneisses.

Dr. Mackie has also discussed in some detail the principles that regulate the distribution of particles of heavy minerals in sedimentary rocks, as illustrated by the sandstones of the north-east of Scotland (*ibid.*, pp. 138–64). A mineralogical point of considerable interest is the record of andalusite in sediments as old as the Middle Old Red Sandstone. It is stated that this mineral probably occurs more frequently than recorded, as its mode of decomposition and association hinder its recognition.

H. P. Lewis records the occurrence of detrital barytes in the Permian basal sand at Nitticarhill, Yorkshire (*Geol. Mag.*, **60**, 1923, pp. 307–13). Garnet, zircon, tourmaline, apatite, monazite, staurolite, ilmenite, and rutile occur as associated heavy minerals. These minerals occur in the Coal Measures to the west, in which barytes is found as a coating on joint planes. The source of the Permian sandstone is therefore traced to these rocks rather than to the barytes-bearing Carboniferous Limestone veins of Derbyshire, a conclusion which is reinforced by the absence of fluorspar from the sandstones.

The Verden Sandstone, described by Reed and Meland (*Journ. Geol.*, **32**, 1924, pp. 150–67), is a supposed river-channel deposit in the Permian Red Beds of south-western Oklahoma. The study of its petrological and palæontological characters (the latter regarded merely as sedimentation data) shows that the rocks were probably deposited by a river which flowed northwest from the Arbuckle Mountains. The abundant calcareous

cement of the rock was deposited mechanically as a fine limey mud. The rather peculiar lithological features of the sandstone are interpreted as due to deposition under cloudburst conditions at long intervals, such as are prevalent in most arid regions.

E. M. Kindle has compiled and collated the data relative to ice-borne sediments which have been collected by the Canadian and other Arctic expeditions (*Amer. Journ. Sci.* (5), **7**, 1924, pp. 251–85), in order to review the efficiency of sea-ice as a distributor of sediments within the Polar Basin and the North Atlantic. He has thus considered the possibility of wind transportation of sediments on to sea-ice, the relative efficiency of marine and non-marine ice in transport, the carrying capacity of ground-ice, floe- and berg-ice, and also records of life on sea-ice. Floe- and berg-ice is believed to be the most highly efficient agent concerned with the wide distribution over the sea-floor of the detritus of the circumpolar lands. Much of this material is being transported to the North Atlantic, and deposited in definite belts determined by the position and direction of ocean currents.

In a paper on algæ as limestone makers and climatic indicators, W. S. Glock (*Amer. Journ. Sci.* (5), **6**, 1923, pp. 377–408) shows that lime-secreting forms become most numerous in sub-tropical and tropical regions, and thus attain their greatest development and perform their most potent geological work in the warmer oceanic waters. The precipitation of lime in quantity sufficient to form limestone beds of any size and of ordinary purity appears to take place best where the conditions of sedimentation are not irrevocably altered by exigencies of climate, and where a large share of the resident marine organisms (as on coral reefs) assist in the construction of the deposit.

The ferruginous cherts of Notre Dame Bay, Newfoundland, described by E. Sampson (*Journ. Geol.*, **31**, 1923, 571–98), present features remarkably in common with those of Scotland in age and geological occurrence. They occur in a series of rocks ranging from Cambrian to Silurian, largely volcanic in origin, and showing "pillow" structures in great perfection. The chert appears in the interspaces between the pillows; and also as heavy jasper beds, and as thinner beds associated with acid tuffs. The bedded chert contains radiolaria as incidental fossils. The silica is believed to have been contributed to a marine basin as magmatic emanations from submarine vents or fissures. Its precipitation is believed to be due to the ions of sea-water having an opposite electrical charge to the colloidal silica; and in lesser degree to oppositely-charged colloids formed by the interaction of the magmatic with the sea-water.

P. B. Stockdale has written a dissertation on "Stylolites: their Nature and Origin" (*Indiana Univ. Studies*, **9**, 1922, pp. 97). The writer has not been able to see a copy of this work, but the following is a short summary taken from the *Amer. Journ. Sci.*, Jan. 1924. The thesis is that stylolites originate in solid limestones that are under static pressure, and that they are not formed in unconsolidated material. There is actual rock removal by differential solution due to percolating ground-waters. The solution is demonstrated by the residual clay left along the stylolite lines.

Metamorphic Rocks and Metamorphism.—P. Eskola has recently elaborated a conception of "facies" in metamorphic rocks whereby they are divided into groups, each of varying chemical composition, but characterised by a definite set of minerals which, under the P.T. conditions obtaining during their formation, were in perfect equilibrium one with another ("The Mineral Facies of Rocks," *Norsk Geol. Tidsskr.*, **6**, 1920, 143–94). Since the original paper is not accessible to the writer, Eskola's views have been taken from a summary by Eskola himself in "The Eclogites of Norway" (*Vidensk.-selsk Skr.*, I, M.–N., Kl. Kristiania, 1921, pp. 118, Intro. pp. 4–5; *see also* SCIENCE PROGRESS, Jan. 1923, p. 366), from a discussion by F. Becke (*Tscherm. Min. u. Petr. Mitth.*, Bd. **35**, Heft 5–6, 1921, 215–30), and especially from a discussion by Dr. C. E. Tilley (*Geol. Mag.*, **61**, 1924, 167–71).

Eskola distinguishes five facies (by no means all that are possible), namely, the *sanidinite*, *hornfels*, *greenschist*, *amphibolite*, and *eclogite* facies. As the rocks belonging to a given facies have originated under similar P.T. conditions, and in general have arrived at a state of equilibrium which appears to be independent of the mode of crystallisation, whether metamorphic or magmatic, each metamorphic facies may be paralleled by an igneous facies. Thus, corresponding to the five metamorphic facies given above, Eskola gives the igneous equivalent as the *diabase*, *gabbro*, *helsinkite*, *hornblende-gabbro*, and *igneous eclogite* facies.

Both Becke and Tilley criticise the use of the overworked term *facies* in this connection. Tilley has united the idea of *grade* of metamorphism with that of facies. Rocks which belong to the same facies may be said to be in the same metamorphic grade, *i.e.* are isofacial or isogradic. Thus the metamorphic zones studied by Mr. G. Barrow in the S.E. Highlands, which are indicated by the entry of certain index minerals, chlorite, biotite, garnet, staurolite, kyanite, and sillimanite, are each isogradic; and we may speak of a chlorite isograd, garnet isograd, etc., an *isograd* being defined as a line joining points where the rocks suffered metamorphism under similar

P.T. conditions. The isograd, however, is clearly only the intersection of an inclined isogradic surface with the earth's surface.

Eskola's contention that the minerals in a given metamorphic facies are in complete chemical equilibrium is combated by both Becke and Tilley, who point to numerous obvious cases of departure from equilibrium in metamorphic rocks. The lag effect on change from high-grade metamorphic conditions to low-grade (mainly from high temperature to low temperature) is enormous; and high-grade mineral assemblages are much less susceptible to further change than rocks originally composed of degradation minerals. Because of this and other considerations the natural metamorphic rocks can be fitted only roughly and approximately into a facies classification; but Eskola's conception will unquestionably be useful in the study of regions of progressive metamorphism such as the Scottish and Scandinavian Highlands.

Dr. C. E. Tilley has given a most valuable contribution to Highland petrology in his study of contact-metamorphism in the Comrie area of Perthshire (*Quart. Journ. Geol. Soc.*, **80**, 1924, 22–71). The Carn Chois diorite is there intruded almost entirely into the Ben Ledi Grit Series. The latter is of variable chemical composition, but mainly siliceous; and there has resulted from its metamorphism a wide range of hornfelsed rocks. Some of the mineral assemblages have not previously been recorded from British contact zones. There are, for example, remarkable and abundant corundum- and spinel-hornfels, a coarse fluor-violet cordierite-hornfels, and a group of hypersthene-hornfels. Calcsilicate hornfels, however, are extremely rare; but one of the Dalradian epidiorites has been metamorphosed with partial to almost complete reconstruction. Dr. Tilley describes all the mineral transformations that have occurred, and has attempted to follow out the changes in chemical equilibrium which have taken place.

G. M. Schwartz has given the results of a study of the contact-metamorphism of a basic rock (the Ely Greenstone of Minnesota) by both acid and basic intrusives (*Journ. Geol.*, **32**, 1924, 89–138). The Ely Greenstone consists of Pre-Cambrian ellipsoidal lavas and tuffs, apparently spilitic, all much altered by low-grade metamorphism. As a result of granitic intrusion this rock is metamorphosed to a hornblende schist, but without destruction of the major structural features such as the ellipsoidal shapes. The effect of the intrusion of the Duluth gabbro, however, is the formation of a pyroxenic hornfels with granoblastic texture. The difference between the two modes of metamorphism is believed to be due to the higher temperature

and relative ' dryness " of the gabbro as compared with the granitic.

Amongst the metamorphic rocks from S.W. Togoland described by T. Robertson (*Geol. Mag.*, **61**, 1924, 116–35) are hypersthene-gneisses ranging in composition from quite siliceous varieties to pyroxenites. These rocks appear to have much in common with the charnockite series of India; and similar types have been found in other West African localities such as French Guinea and Benguella.

Other notable contributions to the study of metamorphic rocks are " Ueber die Petrologie des Otravaaragebietes im östlichen Finland " by M. Saxén (*Inaug. Diss. Helsingfors*, 1923, 63 pp)., and " Die Kristalline Schiefer des Pernar Dagh in Oestmazedonien " by O. H. Erdmannsdorfer (*Neues Jahrb. f. Min. Beil. Bd.*, **48**, 1923, pp. 75–112).

Stratigraphical and Regional Geology.—A valuable Geological Survey Memoir by Mr. H. H. Read on the Geology of the Country round Banff, Huntly, and Turriff (Expl. of Sheets 86 and 96, *Mem. Geol. Surv., Scotland*, 1923, 240 pp.) contains much new matter on a part of the Highland Schist region which has been relatively little studied. The rocks are well exposed in the magnificent Banffshire coast section, and are separable into a western or Keith division, and an eastern or Banff division. The Keith division shows a high-grade metamorphic facies, its rocks being gneisses, granulites, quartzites, and schists, and its characteristic minerals garnet, staurolite, kyanite, and sillimanite. The Banff division, on the other hand, exhibits a relatively low-grade metamorphism; its rocks are phyllites, slates, grits, and limestones, with andalusite as a characteristic mineral. The two divisions are separated by a line of discontinuity, the Boyne Line, which appears to be the trace of a folded thrust plane. The Keith division is probably the north-eastern continuation of the Perthshire Schists; but the Banff division is believed to be limited to the north-east of Scotland.

The chief conclusions of Mr. J. F. N. Green's paper on the structure of the Bowmore-Portaskaig district of Islay (*Quart. Journ. Geol. Soc.*, **80**, pt. 1, 1924, pp. 72–105) are that the Bowmore Sandstone of the Survey consists in reality partly of the Portaskaig Conglomerate and its associated arkosic sandstones, and partly of the flaggy passage beds (Bowmore Flags) between the Islay Quartzite and the Dolomitic Flags. Further, the great Loch Skerrols Thrust is said to be non-existent. Mr. Green invokes but one system of folding to explain the structure; it consists of synclinoria and anticlinoria with isoclinal folds the axes of which have a low dip. The paper records a wealth of new observations impossible to

summarise here. Its main conclusions are sharply controverted by Mr. E. B. Bailey and supported by Mr. G. Barrow in an interesting discussion.

The Upper Ordovician of the south-western part of the Berwyn Hills is described by Dr. W. B. R. King (*Quart. Journ. Geol. Soc.*, **79**, pt. 4, 1923, pp. 486–507). It is shown that the period was one of shallow-water deposition on the whole, but the conditions of deposition and the type of sediment varied from time to time. Mr. W. J. Pugh has described the geology of the district around Corris and Aberllefenni (Merionethshire), in which the rocks belong mainly to the Bala Series, but with representatives of the Llandeilo and the Valentian (*ibid.*, pp. 508–45).

An excellent synopsis of the geology of the Boulonnais by P. Pruvost and J. Pringle forms one of the excursion memoirs of the Geologists' Association (*Proc. Geol. Assoc.*, **35**, 1924, pp. 29–56). The last-named author has a section on the correlation of the Mesozoic rocks of the Boulonnais with those of England. A memoir by E. Nowack on the geology of Albania (*Neues Jahrb. f. Min. Sonderband*, 1, I. Teil, 1922; II. Teil, 1923, 324 pp.) contains the results of an elaborate study whilst the author was with the Austro-Hungarian armies during the War. The rocks consist of a serpentine-schist-hornstone complex probably of Early-Cretaceous age, followed by a long Cretaceous-Cainozoic succession, all the members of which have been involved in the intricate folding of the Dinarides.

A useful sketch of the structural geology of British Malaya has been given by J. B. Scrivenor (*Journ. Geol.*, **31**, 1923, pp. 556–70). The Malay Peninsula belongs to a region of strongly-marked *coulisses* (short arcs in echelon) which sweep through the Archipelago in a semicircular band which ends up in the Philippines. Sedimentary and volcanic rocks ranging from the Lower Carboniferous to the Rhaetic were folded into the arcs in Late Mesozoic times, and the folding was accompanied by enormous intrusions of granite which brought in the tin ore for which Malaya is noted.

There are to hand two important papers on the structural geology of the Bushveldt igneous complex in the Transvaal. The memoir by R. A. Daly and G. A. Molengraaf (*Journ. Geol.*, **32**, 1924, pp. 1–35) contains significant data on the mechanism of intrusion, which will be dealt with later under another heading. But important stratigraphical conclusions have also been reached. In the Transvaal the Waterberg sediments rest unconformably on the Transvaal System; and the great Bushveldt complex of norite, pyroxenite, anorthosite, diabase, granophyre, and coarse granite is supposed to have been injected along the plane of unconformity. Daly and Molengraaf,

however, appear to have demonstrated an erosion unconformity separating the Bushveldt complex from the overlying Waterberg. This obviously necessitates an alteration in the geological time-table for South Africa, since the Waterberg sediments must have been deposited long after, and not before the emplacement of the Bushveldt igneous rocks.

The second paper is by A. L. Hall and A. L. du Toit on the section across the floor of the Bushveldt complex at the Hartebeestpoort Dam, west of Pretoria (*Trans. Geol. Soc. S. Africa*, **26**, 1923, pp. 69–97). The Bushveldt complex here rests on the highest beds of the Pretoria Series represented by quartzites with a shale near the top. With the emplacement of the main norite mass, basic sills were injected into the shale horizon and the overlying quartzite. The effect of the magmatic activity has been intense thermal metamorphism reaching down to not less than 1,200 feet below the base of the lopolith. The sills have taken up and assimilated various sediments, with the production of peculiar cordierite-rich rocks very similar to those recently described by Read from Aberdeenshire (SCIENCE PROGRESS, April 1924, p. 539).

BOTANY. By E. J. SALISBURY, D.Sc., F.L.S., University College, London.

Genetics, etc.—The polymorphic Genus *Rubus* has been investigated by Longley (*Amer. Journ. Bot.*, April 1924), who finds that cytologically the Brambles offer many points of resemblance to the Roses studied by Harrison and others. Strasburger, who examined three species of this genus, found six chromosomes as the haploid number. Longley examined one of these same species and came to the conclusion that the haploid number was seven, as was also the case in *R. occidentalis*, *R. odoratus*, *R. alleghaniensis*, *R. idæus* var. *canadensis*, etc. In all these species the pollen was mostly good with only about 4–20 per cent. imperfect pollen grains.

R. cæsius, *v. turkestanicus*, and *R. corylifolius* were found to have fourteen chromosomes as the haploid number. In *R. deliciosus* the number was 10–11, and in thirteen Blackberries, mostly exhibiting a high percentage of imperfect pollen, the somatic number was 21. In the pollen mother-cells of these forms there were present 9 bivalent and 1 trivalent chromosomes, whilst the daughter nuclei contained from 7 to 12.

R. plicatifolius, *R. hispidus*, etc., in which a high percentage of imperfect pollen is normal (64–90 per cent.), have a somatic number as large as 35 and a varying number in the individual pollen grains. Still other species have 42 or even 56 chromo-

somes as the somatic number and agree in the high proportion of infertile pollen. Probably then many of our so-called species of *Rubus* are of hybrid origin.

W. Eyster describes an interesting variant of the Maize (*Amer. Journ. Bot.*, Jan.) in which the embryo exhibits no resting stage but develops viviparously. The number of "seeds" showing this character was approximately in the ratio of three normal to one viviparous. Seeds from plants showing this feature when selfed gave an approximate ratio of 2 (normal) to 1. The results indicate that this character is a recessive, and it would appear to be linked with the factor for pale yellow endosperm and the factor for albino seedling.

The recognition of individuality in the chromosomes of insects and other animals has been satisfactorily demonstrated, but evidence for such morphological differentiation amongst the chromosomes of plants is at present very meagre. To the few examples known Taylor (*Amer. Journ. Bot.*, Jan.) now adds *Gasteria*.

An account of coupled characters in *Dianthus barbatus* is given by Lilienfeldowna in *Acta Soc. Bot. Poloniæ*, vol. ii, no. 1.

An investigation of the genus *Cratægus* by Longley reveals a similar condition to that found in *Rubus*. There are diploid species where the chromosome number is 32 in the somatic cells and in which the pollen is normal. There are triploid species in which the gametophytic chromosome number is 24 instead of 16 and also tetraploid forms in which the gametophytic number is 32. In many of the triploid and tetraploid species there is no normal pollen formation and the plants are completely self-sterile (*Amer. Journ. Bot.*, May 1924).

Self-sterility and cross-sterility in tobacco appear to depend on the slow rate of growth of the pollen tubes, which in consequence do not reach the ovules before they cease to be receptive. Edgar Anderson (*Genetics*, Jan. 1924) concludes from breeding experiments that this sterility is met with when both parents have certain factors in common, whilst the cross-fertile strains differ in respect to these same factors.

Miss Saunders showed some years ago that certain white strains of Stocks when bred together gave coloured offspring and that certain glabrous strains when bred together produced hoary offspring. The colour has been shown to depend on at least two factors, and this investigator now finds that one of these is linked with one of the factors for hoariness in all the pure-bred commercial strains studied (*Journ. Genetics*, April 1924).

Distribution, etc.—In an interglacial deposit in Poland

Kozlowska has found remains of trees and herbs mostly represented in the forests of the present day, *Tilia platyphyllos*, *Fagus sylvatica*, etc., being important features. In addition, however, there are remains of coniferous wood which is identified as *Tsuga canadensis*. This occurrence is compared with that of other American species such as *Brasenia purpurea*, *Dulichium spathaceum*, and *Fraxinus americana*, which too are only known from Europe as Interglacial remains (*Bot. Gaz.*, April 1924).

Erdtman in a recent communication to the Linnæan Society (June 5th) described the results of the examination of pollen from Scottish peat deposits. *Betula* pollen constitutes about 70 per cent. of the total, next in order of importance being *Pinus* (14·6 per cent.), *Alnus* (11·8 per cent.), *Salix* (11·25 per cent.), *Carpinus* (4·25 per cent.), *Quercus* (2·4 per cent.), and *Ulmus* (1·2 per cent.). *Fagus*, *Ilex*, *Acer*, and *Tilia*, though met with, were rare. The record of *Carpinus* is of considerable interest, as the previous records from fossil deposits in Britain (viz., pre-, late-Glacial, and Roman) were all from locations south of the Wash. The occurrence of *Fagus* is also noteworthy as indicating that the beech formerly had a much wider distribution, as a wild tree, than now, and would seem to negative the idea that the present restricted range is an indication of the recent immigration of the beech.

Ecological.—Forrest Shreve has made thermographic records of soil temperatures, at a uniform depth of 3 ins. at altitudes of 7,000, 8,000, and 9,000 ft. on both north and south exposures. In general the results show a difference at 9,000 ft. of over 18° F. as regards maximum temperatures. At the same altitude the minimum was 4·5° F. lower on the north aspect than on the south aspect. The range decreased with altitude and was always smaller on the northern slope. Comparison of the temperature changes of the soil with the changes in the natural vegetation at different altitudes leads the author to conclude that the ratio of evaporation to soil moisture is of more importance than the soil temperature in determining the plant covering (*Ecology*, April 1924).

A study of the weed flora of experimental arable plots at Rothamsted by K. Warrington (*Journ. Ecology*, Jan. 1924) leads to the conclusion that a predominant perennial type of weed flora (*Tussilago farfara*, *Equisetum arvense*, *Cirsium arvense*) is indicative of manurial deficiency especially with respect to nitrogen. Where adequate nitrogen supplies are available, annual weeds preponderate. Except in the case of pronounced deficiencies, the manurial treatment appears to be quite subordinate in its effect on the weed flora to the effect of the nature of the crop and its mode of culture.

The relation between the Calcium soluable in normal HCl and the *p*H value for a very large number of soils has been determined by Swanson, Gainey and Latshaw (*Soil Sci.*, March 1924). The relation in general appears to be a close one, provided the physical texture of the soil remains the same.

Duley, in the same journal, reports the results of liming experiments which indicate that the Calcium soluable in 0·04N. carbonated water gives a truer estimate of lime deficiency as it affects the plant than the amount soluable in weak nitric acid or the soil acidity itself.

The vegetation for Cuban coral reefs is described by Uphof (*Amer. Journ. Bot.*, June 1924), who states that the first colonisers are the succulent and halophytic *Sesuvium microphyllum* and *Phylloxerus vermicularis*. *Salicornia perennis* and *Distichlis spicata* occupy the deeper soil and cover considerable areas. Beyond the halophytic zone shrubs occur, amongst the important species being *Conocarpus erecta* (Combretaceæ), *Borrichia arborescens* (Compositæ), *Rachicallis maritima* (Rubiaceæ), *Sesuvium portulacastrum*, and *Coccolobis uvifera*, which last gives rise to extensive pure societies.

Kuhnholtz-Lordat has published an extensive account of the Vegetation of the "Dunes du Golfe du Lion" (Montpellier, pp. 308 with 3 maps and 20 plates). The variations in the vegetation are due almost exclusively to soil factors, of which the calcium content and degree of mobility are regarded as the most important. The optimum speed of the wind for dune growth appears to be about 10M. per second, and considerable significance is attached to the degree of interference of the various species. The pioneers on the foreshore are *Agropyrum junceum* and *Sporobolus pungens*, which are followed by *Psamma*, or this last may itself become the pioneer under less halophytic conditions.

On the low-lying shores liable to maritime inundation in winter, dunes may arise through the growth of *Salicornia macrostachya* and *Inula crithmoides*, to be followed by *Psamma* as the level is raised. With the dominance of the Marram grass come a number of less specialised species, such as *Medicago marina* and *Anthemis maritima*, to be followed, as the dune degenerates, by a dominance of *Crucianella maritima*. The damp hollows in the dunes, evidently corresponding to the "lows" of English dunes, are dominated by *Scirpus holoschoenus*, accompanied by *Statice bellidifolia*. On old non-wooded dunes *Rumex tingitanus* or *Sedum altissimum* become the important species and are perhaps the precursors of a Maqui-like type of vegetation. Old wooded dunes are dominated by *Pinus pinea*, to which the Cistetum of *C. salvifolius* is possibly related as a degradation stage.

Taxonomy.—A number of new species are described by F. Nabelek (in *Publ. d. l. Faculte d. Sciences d. l'Univ. Masaryk.*, 35) belonging to the following genera: *Papaver*, *Astragalus* (6), *Glaucium*, *Silene*, *Saponaria*, *Trifolium*, *Gypsophila*, *Psoralea*, *Onobrychis* (4), *Prunus* (2), *Chamænerium*, *Epilobium*, *Eryngium*, *Bupleurum*, *Pimpinella*, *Prangos*, *Rubia*, *Asperula*, *Galium*, *Pterocephalus*.

A new genus of Orchidaceæ from New Zealand is described by R. S. Rogers (*Journ. Bot.*, March), in which the labellum is similar in form to the other perianth segments, though slightly shorter. In this respect the new genus (*Petalochilus*) resembles the Australian *Thelymitra* among the Monandræ or *Apostasia* among the Diandræ. The affinities, however, are considered to be with *Caladenia* and *Glossodia*.

In the June number of the same journal R. Good describes six new species of *Utricularia* from tropical Africa and one species of *Genlisea*; also M. J. Godfery and T. and T. A. Stephenson give a key to the British Dactylorchids.

An additional species of *Amorphophallus* is described by Stapf in the *Bot. Mag.*, March.

The Grapefruit has been separated off as a distinct variety by Merrill and Lee (*Amer. Journ. Bot.*, June) under the name *Citrus maxima* var. *uvacarpa*, the East Indian Pummello or Shaddock being retained as the type.

Economic.—An important contribution to the economic botany of pastures has just appeared from the Welsh Plant-breeding Station (Rep. Series H, No. 3). Stapledon, dealing with grasslands, confirms the fact that a greater yield of hay is obtained from grassland treated as meadow than when treated as pasture, the yield of the former being from 20 to 40 per cent. greater than that of the latter. The gross yield of leaf-blades as compared with stem and leaf-sheath is larger when the cuts are few in number. Moreover, in proportion as the frequency of cutting is increased the growth attained by the root-system at the end of the season is diminished, and this has a detrimental effect upon the yield of the subsequent season. The highest yielding grasses tested were Cocksfoot, Tall Oat-grass, Tall and Meadow Fescue, Timothy, Rye Grass, Meadow Foxtail, whilst Red Fescue and Crested Dogs-tail were low in productivity.

In a paper by Fagan and Trefor Jones in the same Report the chemical composition of some fifteen herbage grasses is dealt with. Of these, the Golden Oat-grass and the Meadow Foxtail are indicated as of the best value on all grounds. The effect of season on composition is clearly shown by the data given, the true protein content being higher in 1923 than in 1922 for all but one of the fourteen species studied.

ZOOLOGY. By Reginald James Ludford, Ph.D., B.Sc., University College, London.

ic Protozoa.—The intestinal protozoa of termites, and the physiological relationship existing between them and their host, have been the subject of an investigation by L. R. Cleveland (*Biol. Bulletin*, vol. xlvi, nos. 5 and 6). It was found that the termites, if incubated at 36° C. for twenty-four hours, became defaunated, without apparently being injured. When they were then fed on their normal diet of wood, they were unable to digest it and died within ten to twenty days, although controls fed with digested wood lived indefinitely. On investigating the ability of the bacteria, fungi, and protozoa harboured by termites to digest wood or pure cellulose, it was found that neither the bacteria nor the fungi could do so, but some of the protozoa were able to perform this function. Cleveland, therefore, is able to substantiate the views of other workers who have regarded these protozoa as digesting the food of the termites. The relationship existing between these two groups of organisms is one of symbiosis, the protozoa receiving food and lodging from the termites, which receive in repayment the products of the digestion of wood.

Mitochondria.—Since the "Altmann granules" first attracted the attention of biologists, numerous have been the theories advanced to explain their nature and significance in vital processes. While they are now generally regarded as a constituent part of the colloidal mechanism of the cell, there is a school of thought which considers that these minute intercellular rods and granules are really of a bacterial nature.

During recent years, I. E. Wallin has been a vigorous supporter of this latter view, and in his latest paper describes culture experiments which he considers confirm the independent nature of mitochondria. He has arrived at the conclusion that "mitochondria are, in reality, bacterial organisms, symbiotically combined with the tissues of higher organisms" (*Am. Jour. Anat.*, vol. xxx, no. 1).

Alterations in the mitochondrial content of cells, under different physiological conditions, are of common occurrence. Some interesting new observations have been described recently. W. Cramer has found that the normal liver-cells of the mouse, when fixed by Schridde's method and stained with iron hæmatoxylin, contain densely stained mitochondria. After thyroid feeding, the mitochondria appear similar, and fine globules of fat are also visible; but after insulin the cytoplasm is considerably altered, and shows regressive changes: mitochondria are no longer visible and the ground cytoplasm has a colloidal appearance. Cramer considers that the glycogen of the liver

is not a reserve of excess carbohydrate, but an "internal secretion" of the liver-cells. Their glycogenic function is inhibited by insulin, but stimulated by the thyroid and adrenal hormones (*Br. Jour. Expt. Path.*, vol. v, 1924).

Changes in the cells of the pancreas, during inanition and refeeding, have been investigated by W. C. Ma. He found that prolonged inanition led to the mitochondria being reduced in numbers, and changed from rods and filaments to irregular granules. The mitochondrial content of the cells was never lost entirely, although the reduction was extreme. Certain of the mitochondria were also found to form fat. These changes were reversible, and after two to six days' refeeding, the cells returned to their normal form. No evidence was found of the formation of new mitochondria independently of pre-existing ones (*Anat. Rec.*, vol. xxvii, no. 2, 1924).

Cytology of the Germ Cells.—J. Bronté Gatenby and J. P. Hill have studied the ovum of *Ornithorhynchus*. As far as they have been able to observe from the material available, it appears that the two polar bodies in *Ornithorhynchus* are unequal in size, the first being the larger and undergoing division before the second is completely separated off. The ovum is polyspermatic, a condition of considerable interest in view of the general occurrence of polyspermy in the meroblastic eggs of Reptiles and Birds (*Q.J.M.S.*, vol. xlviii, part 2, 1924).

Further evidence of the transformation of peritoneal epithelial cells into germ cells in a fowl (*Gallus bankiva*) is supplied by J. Bronté Gatenby. The bird studied possessed such male characters as the comb and wattles, but its left gonad was of the ovarian type and contained an adenomatous growth. Within this ovary the peritoneal epithelium had begun to elaborate testicular tissue. The transition of the peritoneal cells into germ cells was found to take place in a manner similar to what has already been described by the same writer in Amphibia (*Q.J.M.S.*, vol. xlviii, no. 1).

General Histology: Epidermal Fibrils.—A comparative study of the epithelial fibres of the skin of mammals, carried out by B. Shapiro, has led to some interesting new conceptions of the distribution and function of these microscopic structures. Shapiro distinguishes two types of fibres in the epidermis—intracellular and intercellular ones. The former, he considers, are elaborated by the cytoplasm of the cells, and subserve the function of a supporting structure. They are distributed in strict accordance with the laws of mechanics, and their disposition in the cell is, therefore, in the direction of the resultant of all the mechanical forces acting on that cell. The intercellular fibres are regarded as ramifications of the undifferentiated cytoplasm of the epidermal cells, binding them together.

Their distribution bears no relation to the mechanical forces acting on the cell, and their function is probably a nutritive one, serving the purpose of nutritive channels between the basal and superficial cells of the epidermis, in the absence of blood-vessels in this tissue (*Q.J.M.S.*, vol. xlviii, no. 1).

Pigment.—There has been considerable discussion as to the cytological origin of pigments, as different workers have disagreed as to whether it is derived from nuclear, or cytoplasmic constituents. According to H. S. Faris, who has investigated the pigment in embryos of *Amblystoma*, the pigment that is characteristic of amphibian embryos arises in the cytoplasm from colourless granules. These are considered to be chromogen and represent a pigment complex, made of substances that arise in excess from the digestion of yolk, intermediate products of anabolic phases of metabolism and products of catabolism. This pigment is regarded as a waste pigment, comparable to the bile pigments (*Anat. Rec.*, vol. xxvii, no. 2, 1924).

That melanin is derived partly from nuclear, as well as cytoplasmic constituents, is maintained by R. J. Ludford, who has studied melanosis in a melanotic sarcoma of the horse. Pigment formation in this case seems to be a general process involving the cell proteins, often commencing inside the nucleus, and terminating in the almost complete transformation of the cell into melanin (*Journ. R.M.S.*, 1924).

Tissue Culture.—A considerable advance in the technique of tissue culture has been made by T. Lumsden. Until now it has been considered impossible to cultivate tissues continuously in serum, but Lumsden has shown that by paying due regard to the *p*H of the serum, vigorous growths may be obtained. It is essential that the serum should be sterile, and its *p*H must not exceed 7·7. Heterologous serum appears to be as effective as homologous, good cultures of young rat kidney being obtained with rabbit serum (*Lancet*, vol. ccvii, no. 5263).

The opportunity offered by tissue cultures for studying the activities of living cells has led to valuable observations being made within recent years, which have added considerably to our knowledge of cytological processes. The study of cell movements and nuclear division can be followed, stage by stage, in a manner impossible with fixed and stained cells. Special interest attaches, therefore, to recent observations of H. B. Goodrich on cell movements, and T. S. P. Strangeways on the formation of binucleate cells.

Goodrich studied free wandering cells in cultures of tissues of the minnow, *Fundulus heteroclitus* and *F. majalis*, growing in a sea-water culture medium. He paid special attention

to the fan-shaped films of protoplasm which terminate the extremities of these cells, and are probably of a similar nature to the films of protoplasm at the tips of the pseudopodia of amœboid wandering cells, described in cultures of rat tissues by the present writer. By applying the point of a microdissection needle to cells growing *in vitro*, Goodrich found that the fan-like films of cytoplasm were adherent to the cover-slip, but the needle could be inserted between the remainder of the cell and the cover-slip without causing injury to the cell. He regards these adherent portions of the cytoplasm as the motor organs of the cells, by means of which they glide along the surface of the glass. The rate of movement of certain of the cells was measured and found to be, on the average, about 6·3 microns per minute (*Biol. Bull.*, vol. xlvi, no. 5).

T. S. P. Strangeways's observations on the formation of binuclear cells were made on sub-cultures of the adult articular cartilage and embryonic choroid of the fowl, which had been growing in old plasma, previously stored in ice for over fourteen days. He found three processes led to the formation of cells with two nuclei. In the first type, the cell passed through all phases of mitosis, and at the late telophase, instead of separating into two daughter cells, the two portions united to form one cell with two nuclei. The second type was similar, only in this case both nuclei appeared in one portion of the cytoplasm, and a part of the cytoplasm was actually separated from the main portion of the cell. In the third type, two daughter cells were formed, but several nuclei appeared in each of them. This, Strangeways believes, is due to some abnormality in the separation of the nucleoplasm from the cytoplasm.

The Action of Radium Radiations and X-Rays on Living Organisms.—The use of Radium and X-rays in medical practice is, as is well known, due to the fact that cells show, at different periods of their existence, marked variations in susceptibility to irradiation. C. Packard has recently carried out experiments with *Paramœcium*, which show that its susceptibility to radium radiations (chiefly the slowest beta rays) varies with the temperature, at the same rate as do physiological reactions of various kinds. The susceptibility also varies directly with the degree of permeability of the surface layer of the cell. The slow beta rays act on the surface layer of the cell, increasing its permeability, and if allowed to act long enough cause a typical cytolysis. Cells which have a relatively high permeability are more susceptible than those having a low permeability, for the cytolytic action of the rays is quickly followed, in the former, by a cytolysis which is irreversible, while in the latter it is reversible (*Biol. Bull.*, vol. xlvi, no. 4, 1924).

H. J. Bagg and C. C. Little have investigated the hereditary structural defects in the descendants of mice exposed to X-rays. They found abnormal-eyed animals in the third and subsequent generations of the descendants of a group of mice, which had been subjected to the influence of unfiltered X-rays on five consecutive days. From animals treated in this manner, two matings, each of a different male and female, produced very similar types of abnormality, both morphologically and genetically. The eye abnormality, which exhibited a wide range of variation, was associated with marked optic atrophy and a deformity of the skull on the same side of the head as the defective eye. In many animals there was also found, along with the abnormal eye, a condition of club-foot (*Am. Jour. Anat.*, vol. xxxiii, no. 1, 1924).

"*Entwicklungsmechanik.*"—The effects of thyroid grafts on the development of the chick have been investigated by B. H. Willier. The thyroid glands of chickens, varying from two months to two years old, were removed and fragments were transplanted to the vascular chorio-allantoic membrane of the developing chick. The general results were, in the case of the best grafts, reduction in size and emaciation of the body, which are to be regarded as symptoms of hyperthyroidism. These effects are similar to those produced in amphibian tadpoles by feeding with or engrafting thyroid, and are interpreted to mean that metabolism has been increased: more particularly has there been an increase of catabolism over anabolism (*Am. Jour. of Anat.*, vol. xxxiii, no. 1, 1924).

C. Shearer has determined the oxygen consumption of the head, trunk, and tail portions of various stages of the chick embryo, and also of head and tail portions of almost fully grown earthworms, which had previously been deprived of food for three weeks. In both cases he finds that there is a gradient of oxygen consumption, highest in the head and lowest in the tail region, while for a given quantity of chick-embryo tissue the oxygen consumption rapidly diminishes as development proceeds—results which afford strong support to Child's theory of metabolic gradients.

In a further series of experiments, acetone powders were made of head and tail portions of six- to seven-day chick embryos, and of earthworms. These powders, on being made into a thin paste with distilled water, were found to take up a measurable quantity of oxygen. The interesting result came out here, that the powder made from the head region took up more oxygen than an equal quantity of powder made from the tail region. Shearer therefore points out, that whatever the metabolic gradient signifies, it is, at least in part, due to the formation of some definite substance in greater quantity in the head region

than in the tail region. The greater organisation of the head region is not alone responsible for its oxygen consumption (*Proc. R.S.*, vol. xcvi, no. B673).

The transplantation of the region of highest metabolic activity of amphibian embryos—the upper lip of the blastopore—from one embryo to another has been carried out by H. Spemann and H. Mangold. They found that such a graft exercised a differentiating function on its new surroundings, and resulted in the formation of a secondary embryonic anlage, which attained various stages of development. The degree of development of this secondarily induced embryo, which was composed partly of cell of the implant and partly of those of the primary embryo, depended upon the relative disposition of the two embryos. In cases where the primary embryo was encroached upon by the secondary one, the normal order of development of the latter was retarded (*Archiv für mikr. Anat.*, Bd. *C*, 1924).

ENTOMOLOGY. By J. Davidson, D.Sc., Rothamsted Experimental Station, Harpenden.

General Entomology.—An interesting series of papers (*Journ. Econ. Entom.*, **17**, 177–214) deals with certain aspects of statistical methods for estimating the population of and damage caused by various important economic insects; and the *Report 5th Entomological Meeting, Pusa*, 1923 (422 pp.), contains several papers of interest on various aspects of entomology. C. T. Brues (*Amer. Nat.*, **58**, 127–44) has an article on the specificity of food plants in the evolution of phytophagous insects. P. A. Buxton (*Bull. Entom. Res.*, **14**, 289–345) gives a general account of the entomology of Palestine. Malaria is an important insect-borne disease in that country, and the survey has in view primarily the establishing of the species of *Anopheles* which are important in this respect. Katherine S. Buys (*Jl. Morphology*, **38**, 485–510) deals with the general structure and history of the adipose tissue in insects. Immature forms of representatives of the chief orders of insects were studied. From the histological structure, the tissue may be divided into two main groups: (*a*) those insects in which the fat-body cells completely lose their cell boundaries early in development, the tissue appearing in the form of a syncytium, as in Ephemera, Trichoptera, etc.; and (*b*) those insects in which the cell boundaries are maintained, at least in part, until the insect is almost mature or ready to pupate, as in Orthoptera, etc. G. C. Crampton (*Jl. Entom. and Zoology, Pomona*, **16**, 33–47) discusses the phylogeny and classification of insects as indicated by palæontological evidence and comparative morphology; a table is given in which the orders of the class

Insecta are grouped into divisions according to their natural affinities; the class is divided into two sub-classes, Pterygota and Apterygota, the former being divided into two supersections, seven sections, and thirty orders, the latter into two supersections, four sections, and five orders.

Another paper bearing on phylogeny is by A. D. MacGillivray (*Proc. Entom. Soc., Washington*, **26**, 133–41), who criticises Crampton's views on the labium of certain Holometabola. The author maintains that homology cannot be determined by means of analogy of form, as Crampton has attempted to do with the labium. A correct homology on which phylogeny is based can only be constructed by a study of the generalised members of each order and by the determination of the sequence of the modification of these structures, by following through from the primitive generalised ancestor to the highly specialised condition.

A second edition of the textbook on *Medical and Veterinary Entomology* (Macmillan & Co., 1923, 462 pp.) by W. B. Hermes has appeared, having been brought up to date and a chapter on the history of the subject added. E. Martini has brought out a book on the same subject (*Lehrbuch der Medizinischen Entomologie*, G. Fischer, Jena, 464 pp., 244 figs.). It deals with a general account of the organisation and classification of arthropods (pp. 8–71), arthropods as poisonous animals (72–94), as parasites (95–269), and as vectors of disease (270–403), together with a final section on control measures (404–429). C. Houlbert (*Encyclopédie Scientifique*, serie Thysanoures, Dermaptères, et Orthoptères de France, vol. i) deals with the classification, general characters of the various groups, and characteristics of the species.

D. Keilin (*Proc. Cam. Philosoph. Soc. Biological Sci.*, **1**, 63–70) has investigated the question of the appearance of gas in the tracheæ of insects. Aquatic apneustic insects (devoid of functional spiracles) after each moult are found to have their tracheal system filled with fluid, which is, however, soon replaced by gas. It has been suggested that the gas is secretory in origin, but there is no evidence for this, and this theory does not explain the fate of the fluid which the gas replaces. The author is of the opinion that the tracheal fluid is absorbed by cells of various tissues from the intracellular capillary tracheoles; the column of fluid is thus ruptured and the space left by the retiring fluid is immediately filled with gases from the surrounding medium (blood). Data regarding the visits of insects to flowers is given by C. Robertson (*Psyche*, **30**, 158–69; **31**, 93–111). Two more parts of the admirable work of C. Shröder (*Handbuch der Entomolgie*, **3**, nos. 11 and 12) have appeared; they deal with a continuation of the general

systematics of the various orders. W. M. Wheeler's *Social Life among the Insects* (Constable & Co., Ltd., 375 pp.) is a book of absorbing interest, the social habits in twenty-four different groups of insects comprised in five different orders being dealt with.

Orthoptera.—A. C. Hollande and F. Moreau (*Archiv Zool. Exper.*, **61**, no. 3, 59–74) have investigated certain yeast-like cells found in the blood of *Stenobothrus*. When grown in cultures, these cells develop into a mould belonging to the genus *Isaria*, species of which are known to infest insects. In the insect's blood the fungus remains yeast-like and vegetative, and the cells resist the phagocytic action of the leucocytes and are very injurious to the insect; they lessen the number of leucocytes, the fat-cells lose their reserves and accumulate albuminoid crystalloids, certain muscles degenerate, and the insect eventually dies. Infection probably occurs by (*a*) ingestion of parts of plants contaminated with voided yeast cells; (*b*) by the larvæ eating spores of the external fructifying stage of the fungus; (*c*) by the larvæ being penetrated by mycelial threads proceeding from the spores. V. J. Plotnikow (*Bull. Entom. Res.*, **14**, 241–3, translated by B. P. Uvarov) makes some observations on breeding experiments carried out with *Locusta migratoria* Linn. and *L. danica* Linn. In previous experiments the author showed that the progeny of *L. migratoria* were sometimes of the *L. danica* form, the latter being previously regarded as a distinct species; and similarly *L. danica* may give rise to progeny resembling *L. migratoria*. Uvarov (1921) developed the theory that the periodicity of locusts is due to a kind of irregular alternation of a swarming phase (*L. migratoria*) and a solitary phase (*L. danica*). Experiments show that this transformation is due to very complicated factors acting on the larvæ, connected in some way with the density (crowding) of larvæ in a given space. These results are in agreement with those of P. Faure (*Journ. Dept. Agric., S. Africa*, **7**, 205–24), who showed with *Locustana pardalina* that crowding of the larvæ of the solitary phase resulted in a transformation into the swarming phase, whilst larvæ of the swarming phase when bred singly transformed into solitary forms.

Coleoptera.—H. Blunk (*Zeitschr. wiss. Zool.*, **121**, 171–391) gives a detailed account of the larva and pupa and metamorphosis in *Dytiscus marginalis* L.; the habits are fully dealt with and numerous experimental observations made. H. G. Good (*Jl. N. York Entom. Soc.*, **32**, 79–84) gives an account of the life-history of *Prionocyphon limbatus* Lec.: the larvæ live in still water near the shore of pools feeding on the broken-down epidermal cells of decomposing leaves; the females

probably lay their eggs under the leaves in late spring and early summer; the larvæ hibernate in the second or third instar; there are two moults in spring before pupation, and the pupal stage lasts from 10 to 14 days. W. Schultze (*Philippine Jl. Science*, **23**, 609–73; **24**, 309–66) has an important monograph on the Pachyrrhynchid group of the Curculionidæ.

Lepidoptera.—C. B. Antram's *Butterflies of India* (Thacker, Spink & Co., Calcutta, 226 pp.) is a useful book for identifying species; short descriptions in non-technical language and black-and-white illustrations of each species are given; the "blues" and "skippers" are not included. Another manual by H. Eltringham, *Butterfly Lore* (Oxford University Press, London, 180 pp.), is a very attractive popular book on butterflies by one who has an extensive knowledge of the subject. Ishimori Naoto (*Ann. Ent. Soc. Amer.*, **17**, 75–86) has an account of the distribution of the Malpighian vessels in the wall of the rectum of Lepidopterous larvæ; these vessels are arranged in two rows, except in the Hepialidæ, where the three vessels from each side join at their ends and rest freely in the body-cavity without passing into the wall of the rectum. D. E. Minnich (*Jl. Exp. Zool. Pa.*, **39**, 339–55), experimenting with *Pieris rapæ* Linn., found that this butterfly responds to appropriate olfactory stimuli by extending its proboscis; when one antenna was amputated, the olfactory response was reduced about 6 per cent.; when both antennæ were removed, the reduction was about 58 per cent., indicating that these organs do not constitute the only olfactory area. Some notes on melanism based on observations made in the West Riding of Yorkshire are given by B. Morley (*The Entomologist*, **57**, 109–12). This industrial area is not favourable for Lepidoptera, and moorland and lowland species are liable to change; the darkening process proceeds in three ways: (*a*) a gradual darkening year by year, resulting in total melanism; (*b*) a sudden appearance of black individuals among the type, without gradations or intermediate forms; (*c*) a course, which affects most species, resulting in every transitional form. The author expresses the opinion that the comparatively sudden and simultaneous appearance of melanism in so many places has been attended by the marked decadence of many species displaying no comparable change, and eventually it may appear that melanism is a process of deterioration preceding ultimate extinction, and that during its incidence, other species not affected by it are at the same time slowly disappearing, both results being induced by the same factors.

P. W. Whiting (*Anat. Record*, **26**, 395) has a note on the expression of instinct in gynandromorphs of *Habobracon*. One gynandromorph having male and female characters, possess-

ing minute ovaries and no testes, showed normal female instincts; another showed no sexual reactions; and a third reacted as a male but gave antennal reactions of a female.

Diptera.—The following papers are of interest to the systematist: C. P. Alexander (*Ann Entom. Soc. Amer.*, **16**, 57–76; **17**, 59–74) on species of Japanese crane-flies. C. H. Curran (*Canad. Entom.*, **56**, 133–44) on seven new species of *Rhaphium*. F. W. Edwards (*Bull. Entom. Res.*, **14**, 351–401) on the mosquitoes of the Australian region; previous papers by the same author dealt with the mosquitoes of the Palæarctic (1921) and Oriental (1922) regions. G. F. Ferris (*Entom. News*, **35**, 191–9) on the New World representatives of the bat-infesting family Nycteribiidæ. E. W. Ferguson (*Bull. Entom. Res.*, **14**, 251–63) on the Tabanid genus *Pangonia*. T. H. Johnston and G. H. Hardy (*Proc. Linn. Soc. N.S. Wales*, **48**, 94–129) on the Australian species of *Sarcophaga*. J. Villeneuve (*Ann. Sci. Nat. Zool. Ser.*, **10**, 7, 5–39) on the classification of the *Tachinidæ paléarctique*. M. Bezzi has written an important monograph on the Bombyliidæ of the Ethiopian region (*Brit. Mus. Nat. Hist.*, 390 pp.). The work is based largely on material in the B.M. and is a valuable addition to our knowledge of the African Bombyliid fauna, the members of which are of interest owing to the beauty of the adult flies and the parasitic habits of the larval stages. F. Bodenheimer (*Zeitschr. wiss. Zool.*, **121**, 393–441) deals with the anatomy and biology of *Tipula oleracea*. N. Cunliffe (*Ann. Appl. Biol.*, **11**, 54–72) continues his observations on the habits of *Oscinella frit* L. The annual cycle of this important agricultural pest consists of three generations, the first extending from the end of May to middle of August, the second from the end of July to the beginning of September, and the third from the end of April to the middle of June. Meteorological conditions over the period under observation (1919–22), although diverse, were not sufficiently so to affect materially the prevalence of the fly on the crop, which may be due to the fact that the larva is an internal feeder. The fly population would appear to be determined by the total number of host plants available, and not by cultivated plants alone. R. W. Glaser (*Amer. Journ. Trop. Med.*, **4**, 85–107) shows that the larval stages of flies are dependent upon certain accessory growth-factors which are ingested with the food; these are obtained by the larvæ from bacteria or yeasts as well as from higher plant and animal tissues, and micro-organisms present in the medium in which some larval flies live may be important in this respect. L. Lloyd and W. B. Johnson (*Bull. Entom. Res.*, **14**, 265–88) give an account of the technique they have evolved for rapidly estimating the trypanosome infections of tsetse flies, which is of value in estimating the relative import-

ance of the various species as disease carriers. J. Rennie (*Proc. Roy. Phys. Soc., Edinburgh*, **20**, 265–7) has a note on the occurrence of Polyhedral disease in *Tipula paludosa*; the properties of the bodies formed in diseased larvæ and the relation of Polyhedra to the nucleus of the fat-body cells are discussed.

L. G. Saunders (*Entom. Mo. Mag.*, **60**, 133–4) describes an unusual case of sexual dimorphism in ceratopogonine midges. Kieffer (1919) erected the genus *Apelma* with the male sex only, based on the absence of the empodium beneath the claws; Saunders has now discovered the female of Kieffer's type and finds the empodium is present in this sex but absent in the male. W. M. Wheeler (*Proc. Nat. Acad. Sci., U.S.A., Washington*, **10**, 237–44) describes two extraordinary larvæ taken from ants' nests in Panama; they are provisionally named from larval characters, as the adults are unknown. The first larva, *Microdon æolidiformis* sp. nov., is placed in a genus of which we have a British representative *M. mutabilis*; the other, *Nothomicrodon aztecarum* gen. et sp. nov., does not fit any of the recognised families of Diptera, although it appears to belong in the *Cyclorrhapha aschiza* of Brauer. H. N. Worthley (*Psyche*, **31**, 57–77) continues his studies on *Trichopoda pennipes* F., a tachinid parasite of the squash-bug, part 2 dealing with the morphology of this insect.

Hymenoptera.—E. Evrard has written a work entitled *The Mystery of the Hive* (Methuen & Co., Ltd., 369 pp.), which by its charming, simple style and philosophic atmosphere will appeal to a wide circle of readers. H. Mace has a little volume entitled *Adventures among Bees* (Hutchinson & Co., 144 pp.), which is an account of the author's experiences. A detailed work of 456 pp., dealing with the classification, habits, nests, life-histories, and interrelations of bees, entitled *Die Europaischen Bienen*, has been written by H. Friese. M. Hertig (*Minnesota Sta. Tech. Bull.*, **18**, pp. 109–40) has an extended discussion on the normal and pathological history of the ventriculus of the honey-bee, with special reference to infection with *Nosema apis*.

C. T. Brues (*Zoologica*, **3**, 427–32) describes an extraordinary hymenopteron, *Termitobrachon emersoni*, from a nest of *Nasutitermes* in British Guiana; no larvæ were found. The only other termitophilous hymenopteron hitherto recorded is *Ypsistrocerus* Cushman.

E. Bugnion (*M.T. Schweiz Entom. Ges.*, **13**, 285–313) gives a well-illustrated account of the mouth parts of *Scolia*. E. Frühauf (*Zeitschr. wissensch. Zool.*, **121**, 656–721) has studied in detail the morphology and function of the ovipositor in *Biorhiza aptera* Bosc. and *Rhodites rosæ* L. as typical of the

oak-gall wasps and *Rhodites* group respectively; the relation of the reproductive organs to this structure is discussed. Observations on the Ichneumon parasites of *Pieris* are given by C. Jegen (*M.T. Schweiz Entom. Ges.*, **13**, 278–9); 1,216 puparia out of 1,360 collected were found to be parasitised; the observation is made that males predominate from 60 to 80 per cent. in collections of the adult butterflies, the suggestion being that *Pteromalus* preferably infects those caterpillars which are destined to develop into females. A. D. Peacock (*Brit. J. Expt. Biol.*, **1**, 391–412) deals with the males and an intersex-like form of the parthenogenetic saw-fly *Pristiphora pallipes* Lep.

Hemiptera.—The factors which affect the production of winged forms and sexual forms in the Aphididæ have been investigated by several workers in recent years, and S. Marcovitch (*Jl. Agric. Res.*, **27**, 513–22) is of the opinion that the length of day is the important seasonal factor concerned. J. Davidson (*Science*, N.S., **59**, no. 1529, 364) has a note on the same question. L. B. Uichanco (*Philippine Jl. Sci.*, **24**, 143–247) has an important paper on the embryogeny and postnatal development of the Aphididæ with special reference to the history of the "symbiotic organs" or mycetom. Space will not allow of a review of this important paper. There are several papers of interest to the systematist. E. E. Blanchard (*Physis*, Buenos Ayres, **6**, 43–58; **7**, 24–45, 120–5) continues his studies on the Argentine species of Aphididæ. G. Bondar (*Sec. Agric. Indus. e Obras Pub. Secc. Path. Veg.*, Bahia, 183 pp., 84 figs.) gives a descriptive catalogue of the Aleyrodidæ of Brazil; nine genera are erected and fifty-five new species described. L. Gaumont (*Ann. des Epiphyties*, no. 5, 309–46) deals with the classification of the Aphididæ of France. C. P. Gillette and M. A. Palmer (*Ann. Entom. Soc. Amer.*, **17**, 1–59) describes several new aphids (Lachnini). J. W. Hall (*Ministry Agric. Egypt Techn. and Sci. Bull.*, **36**, 67 pp.) continues his observations on the Coccidæ of Egypt. F. V. Theobald (*Entom. Mo. Mag.*, **60**, 124–30) describes seven new species of Aphididæ from Britain.

Other Orders.—Papers of systematic interest include P. P. Calvert (*Trans. Amer. Entom. Soc.*, **50**, 1–56) on species of *Philogenia* Selys. (Odonata); P. W. Claassen (*Canad. Entom.*, **56**, 43–8, 54–7) on species of the North American Capniidæ (Plecoptera); J. Mcdunnough (*Canad. Entom.*, **56**, 98, 113, 128) on new Canadian Ephemeridæ; J. R. Watson (*Florida Agric. Expt. Sta. Gainesville, Bull.*, **168**, 100 pp.) on the Thysanoptera of North America.

K. Baldus (*Zeitschr. wiss. Zool.*, **121**, 557–620) has made an extensive study of the brain in the larva and imago of *Æschna*, *Anax*, and *Libellula*, together with experiments on the corre-

lation of habits and structure in the larval and adult forms. There is no marked difference in structure which can be correlated with the habits, and further investigation of the Neuropile, which must be considered as the seat of the complicated functions of the brain, is necessary. There is an important paper on the bionomics of *Cimex lectularius* L. by F. W. Cragg (*Indian Journ. Med. Res.*, **11**, 449–73).

Three papers of interest to students of Collembola and Thysanura are J. R. Denis (*Archiv. Zool. Exper. et générale*, **62**, 253–98), " La Faune Française des Apterygotes " ; C. Macnamara (*Canad. Entom.*, **56**, 99–105) on the food of Collembola, and J. W. Folsom (*Zoologica*, **3**, 383–402) on termitophilous Apterygota.

It is generally thought that reproduction in Ephemerids occurs after transformation to the imago, but M. A. Gros (*Ann. Sci. Nat. Zool.*, **6**, 411–13) observed the insemination of a sub-imago female of *Oligoneuriella rhinana* Imhof and subsequent oviposition and development ; the aerial life of the adult is less than a hour ; of 200 eggs extracted from a sub-imago of *Torleya belgica*, 100 developed normally. G. Hoke (*Jl. Morphology*, **38**, 347–73), dealing with the head and mouth parts of Plecoptera, shows that a study of these structures indicates a greater degree of specialisation than do Orthopterans, the latter group being hitherto usually regarded as more specialised. J. Krafka (*Journ. N.Y. Entom. Soc.*, **31**, 31–52) discusses the morphology of the head of trichopterous larvæ as a basis for the revision of the family relationships and in a further paper (*Ann. Entom. Soc. Amer.*, **17**, 97–105) deals with the morphology of the prolegs. J. Oscar (*Entom. Mittheilungen*, 1923, 227–32) records a case of facultative viviparity in *Megathrips lativentris* ; Thysanoptera are normally oviparous, but in this species some eggs undergo development within the mother and hatch out as they are liberated.

ARTICLES

THE RELATION OF MATHEMATICAL CONTINUITY TO THE QUANTUM THEORY

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1. Restrictions due to Method in Physics

There are certain kinds of scientific problems in which it is by no means a waste of time to consider how the implications of the concepts used vary according to the very different methods by which any one line of research is being pursued. Such analysis becomes important in physics, owing to the parallel development in that science of mathematical and experimental methods; pure mathematics, *e.g.* of Fourier series or Riemann geometry such as is used in recent physical theory, and experimental work, *e.g.* of electromagnetic observation and the calculations immediately connected therewith, represent such entirely different types of reasoning, subject to such entirely different limitations, that, unless logical analysis keeps pace with the growth of physics, confusion is likely to arise; *i.e.* mathematical concepts will be expected to obey the restrictions of experimental measurements, and vice versa.

The particular case of this to be considered in the present paper is the concept of functional and serial continuity. Now the criticism of continuity follows from problems leading to the use of quanta in dynamics: these have arisen from a group of anomalies in the theories of Black Radiation, Low Temperature Specific Heats, complex series spectra of simple atoms, etc., which have been provisionally solved by a group of hypotheses all involving a transfer of energy by discrete quanta; and in one case at least (Bohr's Correspondence Principle) a reason for the coexistence of classical and quantum solutions has been given in terms of the differences between statistical and individual calculation. A generalised delimitation of what kind of continuity of Action or of Energy Transfer, or of Motion itself, can be legitimate in physical

hypothesis, is, however, not to be expected until further mathematical and experimental advances have been made. Many physicists will feel that the logical analysis of concepts of continuity is useless and wasteful until these advances have been made: it is here suggested, however, that although a physical problem differs from a mathematical, in that *a priori* analysis can never solve it, there is nevertheless some use for a logical investigation as to what restrictions are placed upon the use of concepts such as Continuity or Continuous Motion by the character of the scientific methods of those who make the actual advances of physics. In the hope of contributing to this analysis, which should reveal more exactly what is implied in the main cleavage of recent physics, that separating the quantum theory from the older dynamics, some of the different continuity requirements of physical methods, experimental and mathematical, are here to be distinguished.

Current treatments of the problem of continuity in physics will be examined, and the suggestion put forward that their inadequacy for understanding the quantum problem is due to their neglect of genetic aspects of the concept. For the concept of continuity betrays itself to a considerable extent by its origin and growth. A genetic account would probably show the idea of continuity to arise originally as describing facts of perceptual experience: to pass from this by the refining of perception into the measurements of experimental science; and finally through the use of calculations regarding experimental results to be transferred to mathematics along with the transference of measurable magnitudes to act as variables in functional equations. Once away from the coarseness of macroscopic perception and measurement, the concept of continuity enters upon an entirely different phase and becomes an important link in the chain of reasoning now stretching from the principles of pure logic through the logic of mathematics, and forming an unbroken rational basis of any such process as a differential equation. Now it is the uncritical transfer back again thence to physics which breaks the logical homogeneity which mathematical continuity had achieved, and which is a source of ambiguity when we suggest that a dynamical differential equation is tied to an assumption of continuity which the implications of quantum restrictions contradict. This ambiguity will lead to error, and will not be dispelled so long as we fail to remember that an entirely different meaning has been given to the concept since it was taken from the science of measurement.

The standpoint here defended, in view of there being such a double development of meaning, is that of analysing separately the experimental use and theory and the mathematical impli-

cations of continuity; on the basis that to separate the meanings before recombining them is the only clue to the meaning of the conflict raised by the coexistence of continuous and quantum ideas in dynamics, each valid in certain restricted regions. Ultimate recombination of experimental and mathematical implications is necessary afterwards for practical purposes, since, unfortunately for the simplicity of any theory of scientific knowledge, there are not two sciences actually distinct as Mathematical Physics and Experimental Physics are logically distinct; but the above genetic summary of the development of the idea of continuity in physical science indicates that the several meanings of continuity must be rigorously separated before uncritically assuming one to be a mere coarser or more refined version of the other, or the microscopic in physics to be a mere reduced replica of the macroscopic.

2. Continuity in Experiment and Experimental Theory

The present method of analysis may best be developed from a critical discussion of certain recent treatments of the logical processes of physics. The most thorough attempt to construct an account of continuity in physics is that of Dr. Campbell (*Physics: The Elements*, pp. 539–49). The relation of mathematical continuity in dynamics to quantum requirements is, however, left as obscure as ever by the fact that he discusses physics as an experimental science and yet has to adapt purely mathematical treatments of functional continuity to a group of concepts which can have no mathematical meaning. The definition from which the treatment starts is that of Cauchy, of which a typical expression may be taken from Hobson (*Functions of a Real Variable*, p. 266). "The function $f(x)$ is said to be continuous at the point a of the domain of x, if corresponding to any arbitrarily chosen positive number ϵ whatever, a positive number δ depending on ϵ exists, such that $|f(a+\eta)-f(a)|<\epsilon$ for all positive or negative values of η which are numerically less than δ, and which are such that $a+\eta$ is in the domain of x." Dr. Campbell points out that to bring this into a solely experimental science requires arbitrary restrictions depending on the coarseness and necessary imperfection of experimental measurement. In spite, however, of the incompatibility with mathematical rigour of his restriction which says "statements that $x=x_1$ and $x=x_2$ when x_1 x_2 are within the range only assert the same experimental fact," he retains the Cauchy definition as his foundation for functional continuity by means of a distinction between "magnitude" and "real magnitude." The former is an experimental term, but the latter is not, and yet

in Dr. Campbell's treatment the latter forms the only link with a definition in experimental terms which he adapts from Poincaré as follows: "A magnitude is essentially (*i.e.* independently of functional use) continuous within a range x_1 to x_2 if, and only if, a series of systems having the magnitude can be found including x_1 and x_2 such that each is involved in a set of propositions of the form $x_r = x_s$, $x_s = x_t$, $x_r \neq x_t$, *i.e.* if a magnitude is continuous we must be able to pass from x_1 to x_2, through a series of systems each of which is equal to its immediate predecessor and immediate successor."

Now to build a logic of physics in which two steps of the same argument are the Cauchy continuity and the Poincaré continuity, passing from magnitude to function by means of both, of which the first only has meaning in mathematical rigour while the last has no meaning for mathematics at all, may serve to clear up Dr. Campbell's immediate problem; but if invoked to illuminate the issue in the relation of the quantum theory to classical dynamics, it could only serve to reveal how difficult the problem is, and to throw us back once more on the necessity of separating mathematical and experimental uses and limitations of the concept before attempting to recombine them for dynamics.

Before, however, setting down the limitations that the complete separation of experimental continuity would require, it may be pointed out that a way to the homogeneity which Dr. Campbell loses might be open along the lines suggested in a paper by Dr. Wrinch and Dr. Jeffreys (*Philosophical Magazine*, 1923, vol. xlv, p. 373); here by means of a Probability basis the theory of measurement might be enabled to deal with unique values and thus become amenable to mathematical conditions. For Wrinch and Jeffreys add to a distinction of "measured value" from "true value" (possibly corresponding to Campbell's "Magnitude" and "Real Magnitude") the concept of "adopted value." They show that "true value" of any physical quantity is not determinable uniquely from the knowledge at the disposal of the physicist, but that the "adopted value" obtained by *e.g.* Least Square treatment of measurements may constitute a mathematically unique quantity which is derived from experiment but not solely from experiment. This concept is thus freed from the incompatibility with mathematics in which Campbell's "real magnitude" is necessarily involved. It is important, in forming a general attitude to physics, to note that the greater logical homogeneity attainable by the Wrinch and Jeffreys analysis than by the Campbell analysis is actually due to the fact that the former do not limit the science as the latter does to being solely experimental; it will save time and error

to admit from the beginning of any theory of scientific knowledge that neither experiment nor mathematics alone can build physics, and that logical isolation of each, however necessary for the purpose of clearing the issue of a problem, can only prepare for a satisfactory treatment, and not make it. The recombining into the mixture of mathematics, measurement, and theory, which we call physics, must not be attempted until the separation of the different meanings has been completed: so far the discussion of the current theories has done little beyond revealing some of the difficulty of doing so.

Continuity in experimental science can only be separated from its later mathematical additions if it is regarded as a relative concept. In perception this relativity expresses the fact that, corresponding to any particular observational method (*e.g.* resolving power of a particular instrument), there must, in respect to any judgment of perception, be a considerable range of values of all the magnitudes concerned, such that no change occurs in the judgment until the values have passed outside that range. In terms of sensation and stimulus the phenomenon is expressed in Weber's law, discussed by physiologists and psychologists. The difficulty of adapting a mathematical definition to describe this fact is that it appears impossible to make a rigorous statement concerning the range of the values; in order to do so a finer observational method would have to be applied and the relative nature of the continuity concept would at once adjust itself to this finer method: this difficulty vitiates the use of the Poincaré continuity quoted above for purposes such as that of Dr. Campbell or any purpose requiring a connection with mathematical continuity.

For purely experimental science, therefore, all continuity must be defined in terms relative to a particular experimental method: *e.g.* continuity of motion in problems of Impact depends on the refinement of measurement—in a similar way ingenuity of observational method can record the discrete character of an apparently continuous stream of corpuscular rays. Physics, however, is not a solely experimental science; the examination of Dr. Campbell's attempt here has already indicated that the theories of physics cannot always be expressed in terms of experiment. The most important aspect, therefore, of the problem of this paper reduces to the question: How far can such physical theories as cannot be expressed solely in terms of measurement nevertheless make use of the Poincaré continuity by merely omitting the relativity we have suggested must always characterise its employment in experiment? It is precisely the problem of how far interpolation and extrapolation beyond the reach of actual measurement is legitimate;

in the expression used above, How far the microscopic in physics can be assumed to be merely a reduced replica of what we measure macroscopically.

Now the interpretation of the differential equations of classical dynamics, until recently very general, assumed that such extrapolation was always legitimate, and the logical significance of the quantum theory is really a setting of a limit to this extrapolation. " It is an absolutely fundamental assumption of classical mechanics that the possible states of any system form a continuous series " (Campbell, *Modern Electrical Theory*, supplementary chapter xv, p. 19), the apparent contradiction of which by a quantum hypothesis makes Campbell and other physicists demand, " If we are going to reject classical mechanics at all—and it is impossible to-day to adhere to it in dealing with any of the phenomena of radiation—we had better do so completely " (*loc. cit.*). This provides the contrary assumption that continuity cannot legitimately be extrapolated beyond the macroscopic, and that to do so is the final condemnation of dynamics ; the two sides of the quantum controversy being here seen at their extreme difference.

Exactly for what kind of theory does the legitimacy of continuity assumptions thus cease when we can no longer verify it by measurement—and it must be remembered that both continuity and discontinuity of the order discussed is far beyond all direct measurement—is thus the problem to which the treatment of experimental method leads. The *a priori* restriction on what solutions of this problem are possible must be postponed until a digression on the purely mathematical aspects of a continuity concept is briefly discussed, in order to see what new meaning may be involved when a measurement quantity is used as a variable to be differentiated and integrated. When that is done the mathematical separation can be added to the above experimental separation of the concept, and the general investigation of the use of continuity in physical science made possible.

3. Continuity in Mathematics

Mathematical physics begins at the point where magnitudes measured in experiment are inserted as variables in the functional relations of pure analysis, in particular in the differential equations of motion which sum up so much of physical theory. In the early days of such work an extension of the concept of continuity was at once required by the " infinitesimals " of the pioneers in the Calculus : the relative nature of the concept as above derived in experiment was then dropped out of

mathematics. By the use of differential equations it was dropped from physics too, until Planck's hypothesis of quanta first showed what a conflict is forced on physics by ignoring its logical history. Since the time of Weierstrass, however, pure analysis has avoided the logical inconsistency of infinitesimals and Cauchy's theory of continuity for functions has provided the basis for differential processes. But the abandonment of infinitesimals, while it allows pure mathematics to be traced from purely logical foundations by the work of *e.g.* Whitehead and Russell, gives no less anomalous a position to the continuity of mathematics when the measurable quantities of physics constitute the variables, than did the older calculus with its infinitesimals. For though functional and serial continuity are seldom compared in a single discussion, any "limit" theory of continuity for functions only receives its rational place in the development of mathematical concepts from first principles when related to the Cantor theory of ordinal continuity, which is made possible by the modern theory of transfinite numbers. Definitions of the type, "A series is continuous when it contains a median class having 'Aleph_0' terms," are involved in serial continuity, and through it in the further and not purely serial concept of functional continuity: now to use these as the rigorous expression of relations between variables whose meaning is derived from an experimental measurement is not only the extrapolation questioned in the preceding section: it is more, it is an extrapolation beyond any inductive region to infinity. "Aleph_0" is a number which does not occur in a science of measurement, and by its nature never can; its place as the first number not subject to all the inductive properties prevents its inclusion in a region where even finite extrapolation beyond the macroscopic of measurement is as yet in doubt, and infinite extrapolation meaningless. Poincaré's well-known objection against the use of infinity in science may not be universally valid, but it is for this case supported by a recent paper (Wrinch and Jeffreys, *Phil. Mag.*, 1921, vol. xlii, p. 371), in which the use of infinite classes is declared invalid if our only information about the members of the classes is empirical.

4. Continuity and Quantum Theory

With purely experimental and purely mathematical definitions of continuity as sharply distinguished as the preceding two sections would indicate, it is clear that the mathematical notion is not simply a refinement of the physical but involves concepts foreign to experiment. The question next arises as to whether either of these can form a legitimate continuity

basis for a science of dynamics or mathematical physics, which deals with material which is of experimental origin with a method which can have no experimental explanation. The answer to this question made in practice involves incompatibilities until we widen our theories of physical method; for we have seen in (2) that conflict with any quantum hypothesis must arise if the assumption is made that the continuity of certain magnitudes as we measure them is capable of indefinite extrapolation, and we have also seen in (3) how that indefinite extrapolation is excluded from experimental theory *a priori* by the nature of the assumptions involved in mathematical continuity; and yet, on the other hand, how is a differential equation, with its basis of continuous functions, to be constructed if experimental continuity with its relativity to perception is all that is to be legitimate for theoretical dynamics?

It is here suggested that these incompatibilities to which the study of continuity forces us are insoluble in the terms so far used of a single Physics into which mathematical and experimental methods are to be recombined; *i.e.* insoluble so long as we assume mathematical and experimental physics are capable of talking about the same thing. The recombination required after isolating the continuity concepts can no longer result in a homogeneous physics, but must recognise a logical separation into several sciences. This separation is of course a separation in Theory of Science rather than in Science—physics will always advance by a mixture of such separate "sciences"; but it is suggested that the separations must be kept in mind whenever a mathematical theorem is used to predict an experimental result or an experimental result given mathematical form in a differential equation. Now Dr. Campbell and others have been attracted to the comparative simplicity of a science which can be built logically on one homogeneous method, and reduced all their definitions either to the experimental or the mathematical, but the preceding discussions suggest that either of these courses must end in greater heterogeneity still.

But if the only way to retain mathematical continuity and yet avoid the conflict whose origin was above discussed is to abandon all attempts to provide a single logical development for physics, we must recognise frankly that the subject-matter of microscopic and macroscopic dynamics and experimental measurement cannot be regarded as one in the sense of being reducible to the same scientific terms: their apparent identity before the quantum theory came was due to a fortunate group of accidents to which it is illogical to make further appeal. It is suitable to express this in Dr. Bohr's recent

statement that we may have to admit the impossibility of making a strictly mechanical model of the atom, *i.e.* microscopic space by time variation may mean something entirely different from the "motion" we measure macroscopically. In this suggestion Dr. Bohr foreshadows a far more radical advance than the usual view of classical dynamics as being a statistical account of problems in which many individual quanta are involved. For such reconciliation with quantum theory on unqualified statistical grounds assumes that the individuals are merely smaller "bundles" of the same thing that we measure macroscopically, while the above considerations frankly admit that of the nature of the microscopic in time and space concepts our measurement physics can tell us absolutely nothing.

It may be profitable to attempt to classify the several "sciences" which the difficulties of continuity thus require us to recognise as the logical implication of the quantum theory: their scope, requirements, limitations, appear through the implications of the analysis of the preceding discussions, and must be separately accounted for in whatever solution is devised for such questions as the conflict of certain new views with a concept susceptible of so many different meanings as is the concept of continuity.

The two extremes first separated out above form the basis for these "sciences": namely, experimental continuity and mathematical continuity, which are entirely independent, though as concepts psychologically derivable from a common genesis in experience. Experimental continuity was seen to be meaningless unless relative to some particular conditions of perception, and unprovable if extrapolated beyond those conditions. Subject to this limitation it is definable in terms of Poincaré's expression and Weber's law. Mathematical continuity, the other extreme, was based as a function concept on purely logical ideas, and was not in any way an extrapolation of experimental continuity; since such extrapolation, if carried out, would have to involve an infinite class, from which the dropping of infinitesimals out of pure mathematics cannot save it, to suit the modern Cantor serial continuity which is its logical foundation. Now we can recognise a science of pure measurement and a science of pure demonstrative inference which would make use of these two extreme continuities respectively: the latter would constitute what is sometimes called Rational Dynamics. Most of physics is, however, not solely a science of measurement nor solely an analytical dynamics: the two sciences defined by these two simple extremes of continuity are therefore of small importance compared with the other two now to be discussed. Most

of physics consists of the theories and hypotheses required by experiment: there arises therefore from these hypotheses a third "science" which might be called Experimental Theory, and is logically distinct from the reasoning processes immediately connected with experiment itself.

This third aspect of physics, to which most of what comes under the denomination of that science belongs, is characterised by an experimental continuity extrapolated beyond actual measurement, but not beyond hypothetical measurement; *i.e.* not to a stage supported by infinitesimals of the older calculus or transfinite numbers of the modern theory of functions. It is, therefore, never either demonstrably valid as relative experimental continuity of the Poincaré type would be, nor *a priori* invalid as mathematical continuity of the Cauchy type would be, but forms a temporary hypothesis of continuity until superseded either by refinement of observational method or by the contradiction of its consequences. Of the former mode of superseding, an example is the defeat of the early continental view of cathode rays as a radiation by the English view of them as a stream of discrete corpuscles: of the latter mode of superseding an example is the introduction of Planck's "h" in its first appearance. Now on this view no conflict is here raised by the quantum hypothesis of discontinuity; the neutral character of Experimental Theory as regards validity or invalidity of continuity enabling us to relegate the chief logical difficulty of quantum physics to what is strictly another science: the distinction is clearly seen when Radiation and Specific Heat theories are analysed according to their logical process instead of according to their historical sequence or mathematical connection, showing the introduction of "h" belongs to Experimental Theory while the other great step, the substitution of $\frac{k}{e^{k} - 1}$ for a constant in the equations, belongs, together with the legitimacy of the Equipartition of energy and the legitimacy of all differential processes, to Mathematical Theory, in which also the whole problem of quantised orbits and Bohr's theory of spectra must be classified.

Thus, in the light of this division of physics into the separate sciences determined by the structure of its reasoning, the discontinuity of the quantum theory denies, not classical dynamics, but an hypothesis of continuity which had no *a priori* claim and no basis but analogy and the apparent consistency of results predicted by its means. The continuity of classical dynamics belongs logically not to this Experimental Theory problem, but to the fourth science now to be discussed.

For besides Experimental Continuity, Mathematical Con-

tinuity, and the extrapolated and logically neutral continuity of the third "science" we have called Experimental Theory, the methods used by physicists involve a need for a fourth science, to be determined by whatever continuity is legitimate for the differential equations of mathematical physics. Here arises a difficulty far greater at first sight than that of the purely physical quantum hypothesis of Planck and Einstein concerning Action, and it is perhaps this difficulty which led Campbell and others to make the demand quoted above for the complete rejection of the validity of classical dynamics. For continuity is here not logically neutral, as it was in Experimental Theory, since it is not merely an extrapolated experimental continuity but the rigorous mathematical continuity with its non-empirical basis which seems required.

But the solution seems to lie in the vital importance of remembering for what purpose this mathematical rigour is needed. Now Campbell's revolt against classical dynamics indicates that the need for continuity in forming differential equations implies a similar continuity for the empirical quantities to which the variables of Rational Dynamics become attached in the logical transition from pure mathematics to physics. But there seems no ground for an implication covering such an enormous hiatus in method: and, once the distinction between what is needed for the building of an equation and what is needed for the application of the equation is emphasised, the breakdown of classical dynamics appears as the breakdown of a false transfer of continuity from the method to the material of physics.

Now such a transfer is equivalent to a false transfer from one "science" to another "science" within physics; for the fourth science does not contain any logical element which is not present in one of the other three sciences, and this fundamental distinction within itself of method from material represents the distinction between the elements due to the sciences of pure mathematics and of experimental theory which are both present in mathematical physics.

It should now be more clear what happens when mathematical physics is built up, and what dependence it can have on the continuity restrictions and validity of experiment, mathematics, and experimental theory. Methodologically, mathematical physics is built on continuity of complete mathematical rigour, as in the case of Rational Dynamics and the differential equation systems of pure analysis: this continuity is perfectly legitimate as a methodological convenience on exactly the same ground that n-dimensional and non-Euclidean geometrics are legitimate, and no quantum hypothesis whatever can disturb their validity. But when the foundations of

this continuity were discussed above they were seen to be entirely separate from the empirical concepts on which any dynamics of the world of physics (as distinct from Rational Dynamics) is based. The subject-matter of mathematical physics is therefore not of the same conceptual nature as its methodology—the two sets of concepts involving ideas as impossible to correlate as measuring scales and transfinite numbers. When, therefore, the measurable variables of Experimental Theory are substituted into these equations a step has been taken from one science to what is really an entirely different science, and the distinction of the two sciences becomes the distinction between concepts of a non-empirical methodology and concepts of empirical subject-matter; the methodological continuity of the one must on no account be transferred to the physical material of the other. Thus, while the existence of a differential equation always implies full mathematical continuity in all its rigour, differential equations nevertheless have to be built without any reference to the subject-matter which is to give physical as distinct from mathematical meaning to their variables, and hence the functional continuity of the equations gives no ground whatever for deciding whether the hypothetical magnitudes of Experimental Theory which they represent are or are not continuous beyond what can be measured. This latter decision lies not in mathematics—which is independent of subject-matter—but in the success or failure of experimental predictions made after inserting the quantum condition summarised in Sommerfeld's $\int pdq = mh$.

Mathematical Physics, as the fourth of the "sciences" separated, thus shows itself as a superposition of Experimental Theory on pure mathematics: the latter providing the method of exploration and discovery, but the former the only final test of validity.

The distinction of logically separate "sciences" in physics, which thus seems the only clue to the real nature of the relation of classical dynamics to quantum theory, is illustrated by considering the place occupied by the various quantum conditions which are generalised in $\int pdq = mh$. The difference of what is here called Experimental Theory from the Mathematical Physics, which is a compound of the former with pure mathematics, is shown in the fact that quantum physicists construct differential equations as much as did the older workers, and on as rigorous a continuity basis: the restriction of quantising a motion is then brought in afterwards and superimposed on that continuity. This superposition is not a mathematical farce, but a recognition of the fact that the differential equation

represents a certain group of concepts forming logically one science, while the expression involving "h" represents a theory of microscopic quantities which is for the time being given terminology analogous to that used in experimental dynamics of the macroscopic, and which we have therefore classified as Experimental Theory. The former group of concepts is tied to continuity, but the latter is not.

Further illustration of these distinctions might similarly be found in considering how far mathematical and non-mathematical concepts are involved in Equipartition and Least Action: a methodological reason might be apparent why, of these two generalisations, the latter seems valid as a guiding principle of physics while the former fails. But this would be an application beyond the scope of the present paper.

The outcome of this discussion is, then, a suggestion that the distinction of the "macroscopic" from the "microscopic" in physics, which has been a commonplace these recent years, really covers a distinction into several very different sciences; this distinction, though unimportant and unnecessary in most of the investigations by which the mixed science of physics advances, nevertheless is not without use when widespread conflicts such as that between classical and quantum dynamics arise, and might enable us to recognise in differences of logical structure the only way of understanding how far the methods of physics make such conflicts to be real or illusory.

RECENT DEVELOPMENTS IN THE THEORY OF SOLUTION

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DURING the eighteenth century the majority of chemists inclined to the opinion that a vaguely defined affinity determined the course of a chemical reaction, and it was not until 1801 that Berthollet suggested that the relative quantities of substances as well as their affinities were worthy of consideration: "Toute substance qui tend à entrer en combinaison agit en raison de son affinité et de sa quantité."

In 1867 Guldberg and Waage propounded their famous law of mass action which stated that chemical change was proportional to the product of the active masses of the substances. The application of this law to reversible reactions leads to the conception of the equilibrium constant which is defined by the equation

$$\frac{a_1 \times a_2 \times a_3 \ldots -}{a_1^1 \times a_2^1 \times a_3^1 \ldots} = K$$

where $a_1, a_2, a_3 \ldots$ and $a_1^1, a_2^1, a_3^1 \ldots$ are the active masses of the substances entering into the chemical reaction

$$A_1\ A_2\ A_3 \ldots = A_1^1\ A_2^1\ A_3^1.$$

The law of Guldberg and Waage has been verified experimentally for a number of cases. Notably Bodenstein has confirmed the law very completely for the reaction

$$H_2 + I_2 = 2HI.$$

There are, however, important exceptions. For instance, Lapworth and Jones have shown that the equilibrium "constant" of the reaction

$$C_2H_5OH + CH_3COOH = C_2H_5COOCH_3 + H_2O$$

is not constant, but varies with the amount of hydrogen chloride present as a catalytic agent.

If an attempt is made to apply the law of mass action to

the dissociation of electrolytes its breakdown is complete. If we measure the degree of dissociation of an electrolyte by means of the conductance ratio corrected for viscosity,

$$\frac{\lambda_a}{\lambda_\infty}\left(\frac{\eta_a}{\eta_\infty}\right)^n = a$$

and apply the mass law, we obtain the expression

$$\frac{a^2}{v\,(1-a)} = K$$

where a is the degree of dissociation at a dilution v.

Even in the case of a weak electrolyte such as acetic acid, the " constant " K varies from 1·4 in normal solution to 1·84 in a $\frac{1}{1000}$ normal solution. But the breakdown of this law is best illustrated for solutions of potassium chloride.

Molal concentrations	" K "
·001	·046
·01	·148
·1	·528

The conception known as the solubility product has been of considerable value in a great deal of theoretical work. It depends upon the application of the law of mass action to the ions of sparingly soluble substances, and is contained in the statement that so long as a solution is saturated the product of the concentrations of the two ions concerned is a constant. At saturation the active mass of the undissociated molecules is constant and therefore by the mass law

$$\frac{Ca^{-} \times Ca^{+}}{Ca_{+}} = K$$

and collecting constants $Ca^{-} \times C = K^1$

K^1 is known as the solubility product.

Accurate solubility measurements, however, have shown considerable deviations from this law.

Yet another indication of the abnormal variation of the active mass is given in the enhanced catalytic power of the hydrogen ion in the presence of a neutral salt which may contain the same anion as that associated with the hydrogen.

Very little progress had been made in the construction of a comprehensive theory of solution till G. N. Lewis in America commenced a series of researches which had for their object the formulation of exact thermodynamic laws which should describe the behaviour of the ions and undissociated molecules of all electrolytes. He has succeeded in showing that the actual spatial concentrations measured in moles per litre or moles per 1,000 grs. of solvent with which we are accustomed to deal are not necessarily proportional to the " active mass "

of Guldberg and Waage and van 't Hoff. In a great many text-books active mass is identified with concentration and it is treated as a very surprising matter that laws deduced for a strictly thermodynamic quantity should not hold when applied to a purely mechanical quantity. The idea of spatial concentration has very little meaning for the student of the theory of solution, and indeed it will be shown that in the case of strong electrolytes it is not capable of any rigid logical definition. What really concerns the theoretical chemist is the influence that a substance can exert upon others. He is thrown back upon the active mass, a purely thermodynamic quantity, and must adopt thermodynamic methods in measuring it.

The fundamental quantity in Lewis's theory of solution is called the activity a[1]. This is strictly proportional to the active mass, and so in the case of an electrolyte such as sodium chloride we may write

$$\frac{a+ \times a-}{a_2} = K$$

where a and a_2 are the activities of the positive and negative ion and the undissociated molecule respectively. At infinite dilution we may assume that the activity becomes identical with molality. Furthermore, since we are dealing with conventional quantities we may adjust our values so that K becomes unity, and hence

$$a_\times \times a_- = a_2.$$

The activity of the sodium chloride is thus equal to the product of the activities of the two ions.

At infinite dilution

$$a_+ = a_- = a_2^{\frac{1}{2}}$$

and under all circumstances the geometrical mean of the ion activities $a\pm$ is equal to $a_2^{\frac{1}{2}}$.

If we divide the mean activity of the ions by the molality[2] of the electrolyte we obtain a quantity $\frac{a\pm}{m} = \gamma$, which is known as the activity coefficient or thermodynamic degree of dissociation, which becomes equal to unity at infinite dilution.

[1] Activity is in fact the "active mass" of Guldberg and Waage. It is defined in the equation

$$F_0 - F_1 = RT\log_e\frac{a_0}{a_1},$$

where F_0 and F_1 are the free energies of a substance in two states at constant temperature and a_0 and a_1 are the corresponding activities.

[2] A "molal" solution of, say, sodium chloride, is one which is made by dissolving one "mole," or molecular weight in grammes, in 1,000 grammes of water.

If, however, we are dealing with an electrolyte which dissociates into a number of ions greater than two these equations are slightly modified. For a very dilute solution of barium chloride of molality m we have

$$a_+ = m,\ a_- = 2\,m \text{ and } a\pm = [(m)(2m)^2]^{\frac{1}{3}} = 2^{\frac{2}{3}}m$$

in order to make the activity coefficient equal to unity at infinite dilution we must write

$$\gamma = \frac{a\pm}{2^{\frac{2}{3}}m}.$$

The modern school of chemists in America who owe their origin to G. N. Lewis have chiefly been occupied in measuring these activities and showing their dependence upon stochiometric concentration and the presence of substances with common ions.

If we wish to measure activities or their ratios there are four main avenues of approach:

(1) Electromotive force measurements.
(2) Distribution and vapour pressure measurements.
(3) Measurements of osmotic phenomenon.
(4) Solubility measurements.

By far the most useful and straightforward of these is the electromotive force method.

If a cell be constructed of the type

A. Ag|AgCl KCl(C_1) || KCl(C_2)AgCl|Ag

or

B. Ag + AgCl KCl(C_1) | KHg| KCl(C_2)AgClAg.

The electromotive force developed is

$$E_A = \frac{RT}{F}\ \frac{ln\, a_1^+ a_2^-}{a_2^+ a_1^-}$$

$$\text{and } E_B = \frac{nRT}{F}\, ln\, \frac{a_1^+ a_1^-}{a_2^+ a_2^-}$$

where a_1, a_1, and a_2, a_2, are the activities of anion and cation at concentrations c_1 and c_2 respectively. By making the assumption that $a_1^+ = a_1^-$, provisionally these equations become

$$E_A = \frac{RT}{F}\, ln\, \frac{a_2^2}{a_1^2},\ E_B = \frac{nRT}{F}\, ln\, \frac{a_2^2}{a_1^2}.$$

If we construct a series of cells in which KClc_1 if kept constant and the other half varied KCl(c_2), KCl(c_3)KO/(c_4), etc., it follows that the electromotive forces E_2, E_3, E_4, etc., stand in the relation

$$E_2 : E_3 : E_4 :: lna_2 : lna_3 : lna_4.$$

If, then, we fix the value of a_1 arbitrarily it is an easy matter to calculate a_2 and a_4.

By this means the mean activities of the ions are measured. The measurement of individual ion activities involves certain other assumptions, and will be dealt with later.

The distribution method depends upon the fact that for a substance distributed between any two phases the activities are related by the expression

$$\frac{a_x}{a_y} = K.$$

If, therefore, we can find a solvent in which the activity of the substance in question is proportional to its stochiometric concentration and can measure its distribution between this solvent and water, we can then calculate the activity of the substance in the aqueous phase. This method has not been largely employed since non-dissociating media fail almost completely to extract strong electrolytes from their aqueous solutions. Measurements have been made, however, of the distribution of picric acid between benzene and water.

It is possible, in the case of a volatile solute, to obtain a measure of its activity by means of partial vapour pressure measurements. The partial vapour pressure, provided that it is not too great to justify the application of the gas laws, is proportional to the activity of the solute in the liquid phase. Measurements of this type have been made by Bates and Kirshman, on hydrochloric, hydrobromic, and hydriodic acid. They were unable, however, to carry their work below a dilution of 3M since at these concentrations the vapour pressure becomes exceedingly small. These measurements have been extended to lower concentrations by Rideal and Dunn.

It is also possible by means of the thermodynamical relationships of partial molal quantities to calculate the partial vapour pressures of one constituent if those of the other are known. By this means the partial vapour pressure of sulphuric acid and hence its activities have been calculated from the measurements of Brönsted of the pressure of water vapour over sulphuric acid solutions.

The calculation of activities from exact measurements of osmotic phenomena, such as freezing-point depression, has been accomplished by Lewis, and for details of these calculations reference must be made to his book *Thermodynamics*, chap. xxvii.

The agreement between the three methods outlined above is excellent for solutions of sulphuric acid, the only case which has been completely tested. See Fig. 1.

It is thus possible to measure the thermodynamic concentrations of substances by several methods different in principle which lead to concordant results. It is also apparent that

these thermodynamic quantities are not related in any simple manner to actual spatial concentrations.

The extension of the theory to solutions containing more than one solute has been equally successful. The enhanced catalytic power of hydrochloric acid in the presence of neutral chlorides, where, according to the theory of ionic suppression, a decrease would be expected, was for a time very difficult to explain. If, however, we construct concentration cells in which concentration of hydrogen chloride is kept constant and

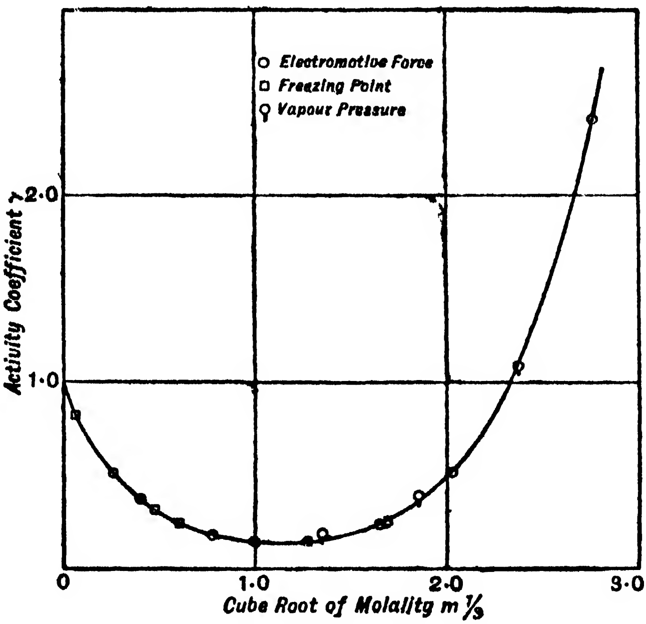

FIG. 1

various neutral salts are added, we find that the hydrogen ion activity is enhanced.

Harned has measured the electromotive force of a series of cells in which the concentration of HCl was kept at 0·1M in the presence of varying concentrations of LiCl, NaCl, and KCl, and has shown that for dilute solutions the activity coefficient is dependent only upon the total concentration of electrolyte. His results are given in table:

Total m.	Pure HCl.	HCl in LiCl.	HCl in NaCl.	HCl in KCl.
0·1	·81	·81	·81	·81
0·2	·78	·78	·78	·78
0·5	·76	·78	·76	·75
1·0	·82	·86	·80	·75
2·0	1·02	1·09	·94	·84
4·0	1·84	2·02	1·47	1·17

In concentrated solutions it is not possible to calculate the activity coefficient by any simple rule, since the specific action of the neutral salt is marked.

It is worthy of notice that the relative effect of added ions in increasing the activity of the hydrogen chloride is parallel with their degree of hydration Li > Na > K.

The work of Brönsted upon the influence of added salts upon the solubility of a solute is too complicated in detail to outline, but he has succeeded in elucidating a number of phenomena which has hitherto received no explanation.

The activity coefficient of slightly soluble bodies in the presence of other electrolytes may readily be determined. In a saturated solution the activity of the solute in the presence

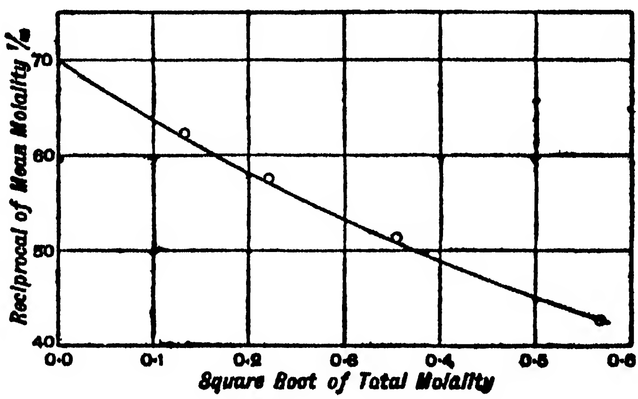

FIG. 2

of a solid is fixed since the thermodynamic potentials in the two phases must be equal.

The activity coefficient is defined by the equation

$$a = \frac{a\pm}{m\pm}.$$

The activity coefficient therefore always remains inversely proportional to the mean molality of its ions. In the case of addition of a solute with no common ions the molality becomes equal simply to the solubility. If, then, we plot the reciprocal of the solubility against the total molality as in Fig. 2, we can find the proportionality factor by extrapolation. Thus, for thallous chloride in presence of a number of solvents the extrapolated line cuts the zero concentration access at ·703, so that if we divide our values of $\frac{1}{m\pm}$ by this number, we get the activity coefficient for thallous chloride in the corresponding solution. Thus Lewis quotes the following figures :

In a 0·05 solution of potassium chloride the solubility of thallous chloride is 0·0059. Thus $m = 0{\cdot}0059$, $m_- = (0{\cdot}0059 + 0{\cdot}050) = 0{\cdot}05590$, and $m = (0{\cdot}00590 \times 0{\cdot}05590)^{\frac{1}{2}} = 0{\cdot}0181$. Dividing by 703 we see that the activity coefficient of thallous chloride in the mixture of total molality $m = 0{\cdot}05590$ is $\frac{1}{0{\cdot}0181 + 70{\cdot}3} = 0{\cdot}784$.

The following table shows a few of the results obtained:

Total m.	In KNO_3.	In KCl.	In HCl.	In $TlNO_3$.
0·001	0·970	0·970	0·970	0·970
0·01	0·909	0·909	0·909	0·909
0·02	0·872	0·871	0·871	0·869
0·05	0·809	0·797	0·798	0·784
0·2	0·676	0·613	0·630	0·546

From this we have no hesitation in affirming that if we could get a super-saturated solution of TlCl at a concentration of ·02 moles per 1,000 cc. its activity coefficient would be ·871. In attempting to formulate a rule for the activity coefficient in mixtures of different valency types, Lewis has been led to an important generalisation. He defines a new term, the ionic strength thus: in any solution of strong electrolytes, let us multiply the stochiometrical molality of each ion by the square of its valence. The sum of these quantities divided by two (since we have included both positive and negative ions), we shall call the ionic strength. In dilute solutions the activity coefficient is the same in all solutions of the same ionic strength. Thus rule holds with considerable accuracy.

Hitherto we have only dealt with the mean activities of the two ions. The individual activities of the ions may be computed if we make certain assumptions. McInnes has assumed that in the case of two univalent salts the activity coefficient of the common ion are identical at the same concentration. It is reasonable therefore to suppose with Lewis that in the case of the individual ion also the activity coefficient of any ion is dependent solely upon the ionic strength. McInnes suggests that in, the case of potassium chloride, in which the ions have nearly the same weight and mobility, we may consider that at all concentrations the activity coefficients of the two ions are identical. By this means we are able to assign values to the potassium and chlorine in any dilute electrolyte of known ionic strength. In the case of sodium chloride, knowing the mean activity and also that of the chlorine ion we can calculate that of the sodium ion. The way is thus opened up to an experimental determination of the individual activities of the ions.

The problem of strong electrolytes has recently been ap-

proached from a totally different view-point by Ghosh, Milner, McInnes, Brönsted, and others. Their fundamental assumption is that all strong electrolytes are completely ionised even at fairly high concentrations. Ghosh has attempted quantitatively to develop this theory to account for observed conductance phenomena. He assumes that the ions of a totally dissociated electrolyte are arranged regularly in a solution by virtue of the electrostatic forces developed between them. Upon this assumption he develops upon a statistical basis the number of ions which are sufficiently removed from the influence of ions of opposite charge to function as "free ions," and to carry an electric current. He obtains what at first sight appears to be an extraordinarily good agreement with experimental values.

His work has been examined critically by Kendall, Partington, and others, who have shown that not only are his two fundamental postulates mutually incompatible, but his numerical agreements are due in a very great number of cases to arithmetical errors and are in the main fictitious.

McInnes bases his view of complete dissociation on the failure to extract any measurable quantity of a strong electrolyte from aqueous solution by non-dissociating media and upon the small values obtained by Bates and Kirshman for the partial pressures of the halides of hydrogen above their aqueous solutions. It is doubtful, however, whether this evidence may be accepted since solubility data are immensely in favour of the aqueous solutions, and this effect is still further aggravated by the probable strong hydration of the salt in the aqueous phase. Hydration, again, is sufficient to account for the small vapour pressures.

Some recent work by Schreider and Brayley upon the transport numbers of sodium and potassium ions in mixed sodium and potassium chloride solutions bears upon this point. They find that under certain circumstances practically no sodium was transported at all. Since it is not likely that a sodium ion could remain almost stationary under the influence of an applied EMF, they conclude that there must be a movement of sodium at almost equal rates in opposite directions. This can only happen by means of the transport of complexes of the type $NaCl{\cdot}Cl^-$, and this necessarily entails the existence of undissociated molecules of sodium chloride.

The upholders of the total dissociation theory appear to have overlooked the work on the optical activity of different salts containing the same active ion and the variation of the rotation with concentration. This work indicates very clearly that dissociation progresses with dilution.

The Present Position of the Theory of Strong Electrolytes.—

It has been established quite definitely that neither the activities or active masses of ions nor of the undissociated molecules of a strong electrolyte are proportional to their concentrations or molar fractions.

Under the circumstances it is not surprising that the conductance ratio $\frac{\lambda_a}{\lambda_\infty}\left(\frac{\eta_a}{\eta_\infty}\right)^n$ does not give a value for the degree of dissociation which shall justify the application of the Ostwald dilution law $\frac{a^2}{(1-a)v} = K$. We should probably not be far out in assuming that we could calculate the actual space concentration of the ions from the conductance ratio.

We are far from understanding completely the factors which determine the thermodynamic concentrations and cannot agree with W. C. McC. Lewis that it is determined solely by the ratio, number of ions : number of molecules of free water. This ratio does, doubtless determine the values to a large extent, and in this connection it is noteworthy that the influence of alkali chlorides upon the activity of hydrogen chloride increases in the same order as their hydration. It is certain that hydration is an important factor in the determination of activity, but it is equally certain that it is not the only factor. The theory of total dissociation, in spite of its great and attractive simplicity, can hardly be upheld in face of numerous objections which have been already outlined, but there are certain elements of truth in it, and the statistical treatment of conductance phenomena will probably play a considerable part in the theory of solution in the future.

G. N. Lewis is of the opinion that the term degree of dissociation applied to an electrolyte is incapable of any rigid logical definition. He states his case somewhat as follows :

Let us consider the equilibrium in the vapour phase of a mixture of monatomic and diatomic iodine. Each molecule of iodine on the average after formation from the atoms will traverse a zigzag path of some length and a reasonable time will elapse before on the average dissociation once more occurs. If by means of an ultra-microscopic instantaneous photograph we could count the number of free and combined iodine atoms, the result of our count would probably be identical with the degree of dissociation obtained by physico-chemical methods.

Ionic reactions, however, are extremely rapid and before an ion has emerged from the influence of its complementary ions it is probably within the sphere of influence of another. Thus the time of dissociation is comparable with the average life of a free ion, and even our instantaneous photograph would be unable to discriminate between free and combined ions. For, first, it would be necessary to decide how far removed

from its complement an ion must be before it is considered to be dissociated. Such a decision would be arbitrary and according to our decision so would the degree of dissociation vary. " Until a problem has been logically stated it cannot be experimentally solved, and it seems evident that in such a case as we have just been considering that, just as we should obtain different degrees of dissociation by different choices of limiting distance, so we should expect to find different degrees of dissociation when we came to interpret different experimental methods."

Whether we agree with this point of view or not, we cannot but recognise the fact that, as far as the chemist is concerned, it is the chemical or thermodynamic concentrations which are important. The detailed theory of the conductivity of electrolytes remains an interesting field for the application of the theory of electrostriction and statistics.

THE NUCLEIC ACIDS

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The problems connected with the composition of the cell nucleus have attracted the attention of bio-chemists for the past half-century, *but*, like similar attacks on most other complex living tissues, have not up to the present been completely solved. The chemical structure and physiological behaviour of the nucleic acids is not only interesting but fascinating, and the study of them has resulted in the isolation of a number of new purine and pyrimidine bases and directly or indirectly has brought to light many important facts. In this summary an attempt is made to indicate our present views regarding the structure and function of the nucleic acids. "Nuclein" was the name originally given in 1868, by Miescher, to a constituent of the nuclei of both animal and vegetable cells. He recognised that it was a nitrogenous, phosphorised organic substance in union with a protein. The non-protein part of the complex has since been recognised as a definite entity, "nucleic acid." Miescher's original method was to subject pus cells to the action of dilute sodium sulphate solution, and he obtained in this way an insoluble grey powder, which on treatment with dilute sodium carbonate solution and acidification with acetic acid gave nuclein, a white flocculent precipitate which gave reactions indicating the presence of phosphorus and protein. Hoppe-Seyler in 1871 obtained a nuclein from yeast cells, and Kossel ten years later prepared another from the red corpuscles of bird's blood. Miescher, in his study of salmon in their ascent of the Rhine, found that spermatozoa heads were free from albumin and globulin and were made up exclusively of a chemical individual—a salt of an organic base (protamine) and an organic acid (nucleic acid) containing phosphorus. In the four years 1896 to 1900 Schmiedeberg in a series of experiments confirmed these observations. Some time previously, however, Piccard in 1874, acting on Miescher's advice, isolated from salmon sperm guanine and a substance (probably adenine, then unknown) which he took to be hypoxanthanine. By submitting nucleins to the action of hydrolytic agents, Kossel

discovered hypoxanthine (1879), a trace of xanthine (1880), subsequently guanine (1882–4), and finally adenine (1886). This discovery of the purine bases furnished the following guide for further investigation :

(1) It gave character to nucleic acid and provided a definition of the substance by which it could be distinguished from proteins and other animal and vegetable products.

(2) It furnished a method of distinguishing true nucleins from pseudo-nucleins which are not constituents of the cell nucleus and do not yield purine bases.

(3) It made possible a study of the distribution of nucleic acid in the body without the actual separation of the cell nucleus from the protoplasm.

(4) It refuted the claims that nucleins were merely compounds of protein with metaphosphoric acid.

(5) It established a chemical connection between the cell nucleus and urinary uric acid.

In 1889 Altmann described a method of preparing protein-free nucleic acids from both yeast and animal tissues, and five years later Kossel and Neumann devised a means of isolating nucleic acid from the thymus gland. The death-blow to nuclein was finally dealt when Neumann in 1899 showed how nucleic acid could be readily obtained in good yield from all its sources.

Research has shown that certainly two distinct nucleic acids exist in nature : one derived from the nuclei of animal cells and known as thymus nucleic acid (the acid obtained from the thymus gland), and the other of vegetable origin, and, since yeast cells were principally the raw material, the acid is commonly called yeast nucleic acid. Schmiedeberg, in 1900, was the first to isolate thymus nucleic acid in a sufficient degree of purity for analytical purposes. He obtained it as the copper salt by a method which eliminated adherent carbohydrates. The isolation of pure yeast nucleic acid was not accomplished until eight years later by Levene. The method now usually adopted for the preparation of both acids is as follows : the finely minced thymus or compressed yeast is treated with very dilute boiling caustic soda. The mixture is filtered, and the cooled filtrate is made faintly acid with acetic acid and then concentrated. The filtrate at 40°c is slowly poured into a large volume of absolute alcohol containing a trace of concentrated hydrochloric acid. The nucleic acid is thus precipitated as a white flocculent mass which is filtered off from the mixture, washed with alcohol and ether, and finally dried at room temperature. Both nucleic acids are white amorphous powders, strongly acid in character and insoluble in alcohol and ether. They are very sparingly soluble in water, but are

readily dissolved by alkali. Their aqueous solutions are optically active and in weak acid solution they are precipitated by protein. When pure they do not give the colour reactions of the proteins. Thymus nucleic acid is distinguished from yeast nucleic acid in that it is not precipitated from aqueous solution by acetic acid and that an aqueous solution of its sodium salt gelatinises on heating and subsequent cooling. Thymus nucleic acid was shown to possess the composition represented by the molecular formula $C_{43}H_{56}O_{30}N_{15}P_4$, while yeast nucleic acid, according to Levene, has the formula $C_{38}H_{55}O_{32}N_{15}P_4$.

The next question to consider is their chemical structure, that is, how the several elements are linked together in the molecule. Kossel and Neumann (1893–4) studied the action of mild hydrolysis of thymus nucleic acid by means of dilute mineral acids and found both guanine and adenine together with a more resistant, ill-defined substance which they termed thymic acid. Drastic hydrolytic agents, *e.g.* 40 per cent. sulphuric acid, destroy the purine bases and give rise to the following products : (1) cytosine, (2) thymine (both pyrimidine bases), (3) lævulinic acid, (4) formic acid, (5) ammonia, and (6) phosphoric acid.

The lævulinic acid was identified and was rightly regarded as a secondary product from a hexose group in the nucleic acid molecule. Thymine was easily isolated, recognised, and its formula determined ; cytosine was much more difficult to obtain, but was finally characterised beyond all doubt. The ammonia and formic acid were presumed to be formed by the decomposition of the purine bases. Hence the purines are more firmly held in the molecule and require drastic treatment in order to dislodge them from their combination.

The many discrepancies in the analyses of the nucleic acids from various animal sources were cleared up when, in 1904, Bang isolated from the nucleo-protein of the pancreas a nucleic acid of simple composition which yielded on decomposition three substances, viz. phosphoric acid, a pentose, and guanine (a purine). This acid is a representative of a "*mononucleotide*" and is termed "guanylic acid." It was obtained in a pure state —as the potassium salt—by Walter Jones and Richards in 1914 by the addition of alcohol to its aqueous solution. The pentose was identified as *d*-ribose, and the following structural formula for guanylic acid has been evolved :

$$\begin{array}{l} HO\diagdown \\ \quad O{=}P-O-\underset{\text{Ribose residue}}{C_5H_8O_3}-\underset{\text{Guanine residue}}{C_5H_4N_5O} \\ HO\diagup \end{array}$$

The exact point of union of the pentose and guanine is still uncertain.

Another mononucleotide, viz. inosinic acid, although it does not enter into the structure of either of the nucleic acids, is worthy of mention. It was obtained from meat extract as the crystalline barium salt by Liebig as long ago as 1847, and was later shown by the action of dilute acids to consist of phosphoric acid, *d*-ribose, and hypoxanthine linked together as indicated above for guanylic acid, hypoxanthine, however, taking the place of guanine in the latter.

Walter Jones in 1908, continuing Schmiedeberg's work, established many valuable hypotheses, and in the same year Steudel gave an equation for the decomposition of thymus nucleic acid by oxidation and hydrolysis, in which guanine, adenine, thymine, cytosine, hexose, and metaphosphoric acid were formed.[1] It is now generally agreed that thymus nucleic acid consists of four mononucleotides similar to guanylic acid, all linked together. Each nucleotide is made up of phosphoric acid, a hexose, and a base, the carbohydrate residue being intermediary, linking the acid to the base. The mononucleotides, guanylic acid, and inosinic acid are not found as such in the nuclei of the cells, but are to be looked upon rather as metabolites of the true nucleic acids and are derived probably from the vegetable food consumed by the animal. Owing possibly to the labile attachment of the purine groups and the instability of the hexose residues, the mononucleotides of thymus nucleic acid have not so far been obtained in a crystalline form. The furthest advance has been the isolation of the barium salt of hexose thymidine-diphosphoric acid, the most complex degradation product of nucleic acid which has been crystallised. However, each of the four mononucleotides which constitute the nucleic acid molecule can be split off by the action of dilute acids, and by the action of more drastic hydrolytic agents at elevated temperature each mononucleotide loses phosphoric acid, leaving a residue of the base and the carbohydrate. The latter combination is termed a "*nucleoside*," guanosine (guanine and *d*-ribose) in the case of guanylic acid. By hydrolysis with acids the nucleoside is further degraded into the carbohydrate and the base. The carbohydrate obtained from thymus nucleic acid was stated by Feulgen (*Zeit. physiol. Chem.*, **100**, 241, 1917) to be glucal ($C_6H_{10}O_4$), a reduction product of glucose to which Fischer in 1913 attributed the structure $CH_2 \cdot OH \cdot \underbrace{CH \cdot CH_2 \cdot CHOH \cdot C}_{O} : CHOH$; but in his recent book *Chemie und Physiologie der Nukleinstoffe nebst Einführung in die Chemie der Purinkörper* (Berlin, 1923), Feulgen brings

[1] $C_{43}H_{57}O_{30}N_{15}P_4 + 8H_2O + O_2 = C_5H_5N_5O + C_5H_5N_5 + C_5H_6O_2N_2 + C_4H_5ON_3 + 4C_6H_{12}O_6 + 4HPO_3$.

forward evidence showing that the sugar gives reactions of an aldose and its behaviour shows marked differences from a hexose. The two pyrimidine bases isolated from thymus nucleic acid are thymine and cytosine, and the two purine bases are adenine and guanine.

Work on yeast nucleic acid has been concentrated since 1913, and Levene and his colleagues with the aid of hydrolytic agents have proved that it contains equivalent molecular proportions of guanine, adenine, cytosine, and uracil linked up with phosphoric acid through sugar residues. Thus *both* nucleic acids consist of four mononucleotides, but in yeast nucleic acid uracil replaces the thymine of thymus nucleic acid. By hydrolysis with ammonia in an autoclave at 122° C. Dorfmüller (1915) decomposed yeast nucleic acid into the four constituent mononucleotides obtained in crystalline form by Levene (*J. Biol. Chem.*, **41**, 483, 1920). By neutral hydrolysis under pressure at 175° C. Levene and Jacobs obtained from yeast nucleic acid the following four crystalline nucleosides:

Guanosine ($C_{10}H_{13}N_5O_5$), identical with the nucleoside from guanylic acid.

Adenosine ($C_{10}H_{13}N_5O_4$).

Cytedine ($C_9H_{13}N_3O_5$).

Uridine ($C_9H_{12}N_2O_6$).

Boiling mineral acid decomposes these nucleosides into a pentose (*d*-ribose), which was isolated in crystalline form and identified, and the base. Incidentally, the isolation of *d*-ribose gave Levene the opportunity to synthesise an hitherto unknown pentose.

The question now arises as to the manner in which the four nucleotides, both in thymus and in yeast nucleic acid, are linked together by the loss of three molecules of water. Until recently Kossel's suggestion that linkage occurs through the phosphorus atoms was accepted. On this view the nucleic acids would be acid anhydrides, *i.e.* complex derivatives of pyrophosphoric acid. Levene now considers the union to be between phosphoric acid of one nucleotide and the sugar residue of another, which would represent the nucleic acids as esters. Walter Jones, whose views have to some extent been adopted by Thannhauser, imagines anhydride formation between the sugar complexes only. On this view the nucleic acids would be regarded as complex ethers. As no definite solution has yet been found, for the sake of comparison, Levene's formula for thymus nucleic acid and W. Jones's formula for yeast nucleic acid are given in the footnote on pp. 248–9.[1]

The arrangement of the basic groups is of course arbitrary to some extent, but in the case of thymus nucleic acid, as thymine and cytosine can be obtained with one sugar and two

phosphoric acid groups attached, both bases cannot be in the same half of the molecule.

The nucleotide structure, which has, as we have seen, been proved beyond doubt, is at once remarkable. Such a complex would hardly have been arrived at from purely chemical speculation, and synthesis of the nucleic acids is eagerly awaited. It is certain that such compounds play a prominent part in the chemistry of living tissues.

From their studies on rats Osborne and Mendel (1912) showed in a striking manner that certain protein groups must be present in the food if normal increase in body weight is to follow, but the growing body makes no such demand on purine metabolism. That an adult mammal can synthesise a purine ring was shown by Benedict in 1916. In his experiments with dogs Schittenhelm (1910) found that nucleic acid, taken in with the food, is not altered or absorbed by the stomach, but is split up to some extent in the intestine, giving a small amount of purine but chiefly mononucleotides or nucleosides. Working with tissue extracts *in vitro*, it has been shown that further degradation is accomplished by the spleen, pancreas, liver, and the kidney. Very little is known about the ultimate fate of the pyrimidine bases, except that the body makes use of them in some way, but only when in the combined form, *i.e.* as nucleic acid ; if pyrimidines in the uncombined state are taken in with the food, they are excreted in the urine unchanged. However, the fate of the purines has been very thoroughly worked out. Adenine under the action of the enzyme adenase is converted into hypoxanthine, which is further acted upon by hypoxanthine oxidase, producing xanthine. Xanthine is in turn transformed by xanthine oxidase into uric acid, which in the lower mammals is converted by uricase into allantoin. It has been found, working with tissue extracts *in vitro*, that uricase is absent in

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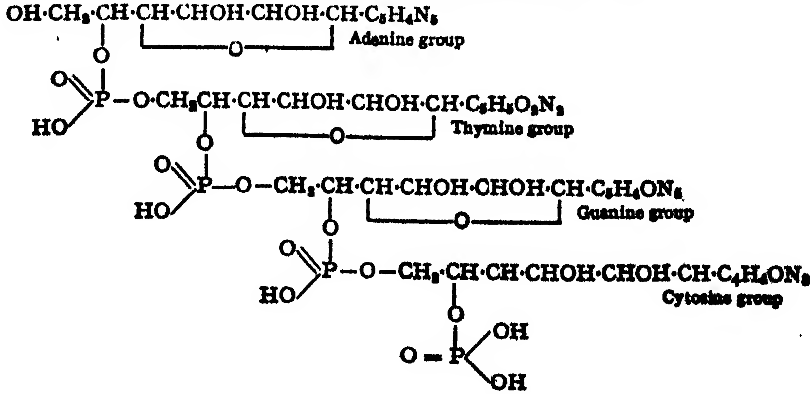
[1] *Levene's Formula for Thymus Nucleic Acid*



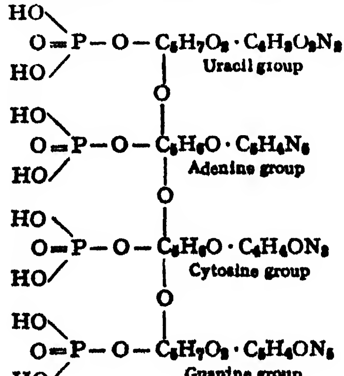
the higher apes, the Dalmatian coach-hound, and in man, and thus uric acid and not allantoin appears in the urine. Wiechowski (*Biochem. Zeitsch.*, **19**, 368, 1909) admits that if uricolysis occurs at all in human tissues, the action is extremely small compared with other animals, and when present appears to be reversible or at any rate counteracted by other enzymes. This seeming absence of the uricolytic enzyme in man is at present inexplicable.

Although not directly concerned with the chemistry of nucleic acids, attention may be drawn to some recent work, carried out in the laboratory of King's College, on the action of nucleic acids on the coagulability of the blood (*Proc. Royal Soc.*, B, vol. 96, 1924). Pickering and Hewett found that rapid intravascular injection, or addition *in vitro*, of thymus or yeast nucleic acids, inhibits the coagulation of the blood shed from cats and rats which have been deprived of hepatic activity, *i.e.* secretion of "antithrombin." Also an increase of carbon dioxide in the circulating blood of these animals decreases or annuls the anticoagulant action of nucleic acid. Furthermore, these authors noted that serial intravascular injections of thymus nucleic acid produce at first hypercoagulability, followed by tolerance, culminating in immunity to the anticoagulant action of nucleic acid. Immunisation against the anticoagulant action of thymus nucleic acid was shown to be produced with material free from protein.[1] Pickering and Hewitt suggest that inhibition of clotting by nucleic acid may be due to union with plasma components, forming a more stable complex than that which exists in normal circulating blood.

It has been suggested also that the precursor of lactic acid

[1] *Cf.* Ford (*Journ. Infect. Diseases*, vol. 4, 1907, 541) and Acree and Syme (*Journ. Biol. Chem.*, vol. 2, 1907, 547) on the production of immune phenomena by certain glucosides.

Jones's Formula for Yeast Nucleic Acid



in muscle is sugar in the form of a hexosephosphoric acid, and it is obvious, if this be so, that the metabolism of carbohydrates is bound up with a phosphorus combination such as the nucleic acids. In support of this it may be pointed out that the accelerating influence of phosphates on fermentation is well known.

Although much advance has been made with the chemistry of the nucleic acids, at present there is no convincing evidence connecting them with any particular stage of cell metabolism. Much work must needs be forthcoming to demonstrate the function of these highly interesting phosphorus complexes and to elucidate also the rôle they play in the many-sided activities of these microscopical laboratories we term cells. The whole question is as yet " wrapt in mystic silence and in gloom."

THE PHYSICO-CHEMICAL THEORY OF TITRATION AND ITS APPLICATION TO BIO-CHEMICAL PROBLEMS

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Physiology may be said to comprise the sciences of bio-physics and bio-chemistry, and these in turn may be described as the application of the laws of physics and chemistry respectively to explain the functions of living matter. It might appear from this distinction that the bio-chemist has little need to be concerned with the principles of physics, that he should be preoccupied with the chemical composition of, and chemical changes occurring in, living matter, rather than in the physico-chemical phenomena associated therewith. It has been urged that metabolism—the fundamental basis of bio-chemistry—cannot receive its ultimate explanation in terms of physico-chemical formulæ, that it consists essentially and obviously of the reactions of descriptive chemistry. Be this as it may—and it should be pointed out that these very chemical changes are themselves governed by and are under the control of physical and physico-chemical conditions—it is perhaps not out of place to emphasise the value to bio-chemical science of the precise and elegant methods of physical chemistry, since they are capable of throwing light on all manner of problems, qualitative and quantitative, which seem to baffle solution by the older methods of analysis. In other words, even if it be granted that the physico-chemical *explanation* is less adequate than the purely chemical, the physico-chemical *method* may yet be recognised as a valuable tool for arriving at purely chemical knowledge.

In the realm of physics some of the most brilliant of recent work has been undertaken for the purpose of solving physiological problems. In order to elucidate the chemical reactions which occur in muscle during exercise, A. V. Hill has employed, in conjunction with mathematical deduction, some of the most delicate physical measurements ever carried out. Hartridge and Roughton, making use of novel physico-chemical methods,

have recently thrown fresh light on the chemical change which occurs during the oxygenation of the blood in the lung. The work of Barcroft, Sörensen, Michaelis, Bayliss, Hardy, and others might also be referred to, did space allow.

In pure chemistry the modern Theory of Titration has developed from the work of Noyes, Bjerrum, and others. Its application to analytical operations has vastly increased the scope of the volumetric method, which has gained considerably also in precision and refinement. But little attempt has hitherto been made to apply the titration theory to bio-chemical problems, owing largely no doubt to the complexity of the chemistry of living matter. The main object of this paper is to describe in outline some of the results [1] of the application of the theory to amphoteric electrolytes—a class of substances which includes the *proteins* (and their various degradation products), which are the most typical components of living matter. The writer has been able to show on theoretical grounds that both the basic and acidic groups in amphoteric electrolytes are capable of estimation by a variety of acidimetric and alkalimetric methods, and has presented experimental data to demonstrate that a high degree of accuracy is possible when the titrations are controlled by conditions demanded by theoretical considerations. Perhaps the most important practical application is to the determination and identification of amino-acids—the stuff of which proteins are built. The results obtained by such methods are much more nearly quantitative than those resulting from actual separation and weighing of the amino-acids or their derivatives—a general method of attack in the past, which has yielded only very incomplete information.

Incidentally it has been found possible to solve certain outstanding problems of chemical reactions and relations by arguments based on the physico-chemical theories of Mass Law and Titration. For example, I have been able to prove the incorrectness of the oft-suggested hypothesis that the peptide linkage $-CO-NH-$, in dipeptides combines with acid or alkali with accompanying enol-lactim rearrangement.

Theoretical Considerations.—In volumetric estimations by the old-fashioned method acid is added to alkali, or vice versa, until a change of colour is produced in an empirically chosen indicator, *i.e.* until the " end-point " is attained. The

[1] L. J. Harris, " The Titration of Amino- and Carboxyl-groups in Amino-acids, Polypeptides, etc.," Parts 1 to 3, *Proc. Roy. Soc. B.*, vol. xcv, pp. 440–484 (1923); Parts 4 to 6, *Proc. Roy. Soc. B.*, vol. xcv, pp. 500–522 (1923). Use of the Quinhydrone Electrode for the Estimation of Amino-acids and of Acid and Basic Functions, *J. Chem. Soc.*, vol. cxxiii, pp. 3294–3303 (1923). " The Basic Dissociation Constant of Valine," *Biochem. J.*, vol. xvii, pp. 693–5 (1923).

theory of titration, on the other hand, takes account not only of the end-point, but also of the change in hydrogen-ion concentration (which is a measure of "acidness") during the whole course of the titration. The usual procedure is to plot observed p_H (*i.e.* minus logarithm of hydrogen-ion concentration) against amount of acid or alkali added. In titrating an acid with a strong base the end-point is determined by the strength of the acid, *i.e.* by the magnitude of K_a. The weaker an acid the more alkaline will be its titration end-point. Thus, the addition of one equivalent of soda (1) to an equivalent of hydrochloric acid produces a neutral solution, (2) to an equivalent of acetic acid, an alkaline solution, (3) to an equivalent of boric acid (a very weak acid), a highly alkaline solution. Similarly, the weaker a base, the more acid is its titration end-point. It is on considerations such as these that one can titrate separately the constituents in mixtures of acids or alkalis, *e.g.* estimate both acetic and sulphuric acids in a mixture of the two.

In discussing the theory of titration in relation to amphoteric electrolytes I have dealt especially with the amino-acids resulting from protein hydrolysis, and have been able to show that the general theory holds with considerable accuracy, provided a solution containing amino-acids is regarded as a mixture of bases and acids with the same dissociation constants [1] as those of the amino- and carboxyl-groups (or basic and acidic radicles) present.

It is impossible to describe here in any detail the theoretical basis of the technique that has been devised for estimating amino-acids. It may be said in brief that in most cases an amino-acid behaves as though it were a mixture of a very weak base with a very weak acid. Therefore to "neutralise" the whole of the weakly *basic* radicle in the amino-acid the titration must be carried to a very highly *acid* end-point, and vice versa. In neutral solution such amino-acids do not appreciably possess the properties of either acid or base. Since the various amino-acids, which in different amounts go to build up the numerous proteins, often differ from one another in the strengths and sometimes in the number of their respective basic and acidic groups, therefore, it is possible from titration data to discriminate between the various amino-acids, or types of amino-acids, present in a mixture and hence to obtain quantitative information as to the composition of the particular protein from which the mixture of amino-acids was derived.

[1] Bjerrum has recently put forward the theory that the generally accepted *dissociation constants* for amino-acids are not the "true" but "apparent" dissociation constants. Whichever view prove correct, the results described below remain from a practical point of view unaffected.

The hydrogen-ion concentration resulting after the addition of each increment of acid or alkali may be determined in one of three ways: by the hydrogen-electrode technique, by the use of coloured indicators, or in acid solutions by the use of the quinhydrone electrode (Biilman). The last-named method has much to recommend it by reason of its accuracy combined with great simplicity.

Results Obtained.—We must now summarise the main results obtained in the series of papers referred to above.

(1) The strength of a solution of a given amino-acid [1] may be estimated by titration of its weakly acid or basic groups to highly alkaline or acid end-points (applying the necessary corrections for the effect of added titrant on the *solvent*, for which purpose formulæ, curves, and constants are provided).

(2) Alternately, the strength of an amino-acid solution may be determined by titrating to a fixed, predetermined p_H value—not necessarily the end-point; the amount of acid or alkali required (after subtracting the "blank-corrections") being, from mass-law considerations, directly proportional to the amount of amino-acid present.

(3) The total amino- and carboxyl-groups present in a mixed solution of amino-acids may be determined.

(4) Estimation of individual amino-acids or groups in mixtures: using the quinhydrone electrode, I have been able to estimate (1) an amino-acid in presence of mineral acid or alkali, and each of the following alone or present in mixtures of "monoamino-monocarboxylic" acids; (2) glutaminic (and aspartic) acid; (3) lysine and arginine, or histidine, etc.

(5) The technique is of service in controlling the course of hydrolysis of proteins, *e.g.* in estimating the amount of amino-acids, etc., set free during the course of digestion; and,

(6) In controlling the purity of a given amino-acid (as during the course of its separation and preparation).

(7) By the choice of appropriate indicators many simple estimations of amino-acids may be effected. For example, histidine may be estimated by simply titrating with standard HCl, to a colour-change with the indicator thymol-blue.

(8) The methods of estimating amino-acids by titrating the *carboxyl* with alkali in presence of alcohol (or of formol) have been investigated from the standpoint of the theory of titration; and by introducing the use of appropriate indicators under conditions demanded by theory, the accuracy of such methods has been vastly increased.

(9) A simple method has been devised for estimating *amino*-groups in the presence of alcohol by a back-titration to the indicator methyl-red. The extreme accuracy of the methods

[1] The amino-acid solution itself being, most likely, of neutral reaction.

described in the last two sections is evident from the following table :

	Estimation of — COOH and other acid groups.		Estimation of — NH_2.	
	C.c. of N/10 NaOH required, titrating in alcohol to thymol-phthalein.		C.c. of N/10 HCl required in back-titration to methyl-red.	
	Found (corr.).	Theoretical.	Found (corr.).	Theoretical.
1. Glycine	5·0	5·0	5·0	5·0
2. Alanine	4·95	5·0	4·95	5·0
3. Valine	2·4	2·5	2·4	2·5
4. Leucine	4·9	5·0	4·9	5·0
5. Cystine	5·05	5·0	4·9	5·0
6. Phenylalanine - hydro-chloride . . .	10·05	10·0	5·05	5·0
7. Tyrosine	6·4	5·0 to 10·0 (6·4 for 28 per cent. of phenolic dissn.)	6·3	5·0 to 10·0 6·4 for 28 per cent. of phenolic dissn.)
8. Tryptophane	5·0	5·0	5·0	5·0
9. Aspartic acid	9·75	10·0	4·6	5·0
10. Glutaminic acid hydro-chloride	5·9	6·0	1·95	2·0
11. Histidine hydrochloride, H_2O	2·0	2·0	1·0	1·0
12. Lysine picrate . . .	2·5	2·5	2·5	2·5
13. Mixture of glycine, leucine, valine, glutaminic-HCl	5·8	6·0	3·9	4·0

(10) For use with mixtures containing a series of groups whose separate titration curves are so similar as to overlap, a method is described for deducing the " resultant titration-curve." Conversely, a " resultant titration curve " may be resolved and the various constituents present estimated.

(11) *Proteins* are capable of being estimated by a similar technique, since each protein possesses a constant " corrected neutralisation curve " which is practically irrespective of dilution and which is characteristic of that particular protein.

The following are some of the chemical problems solved other than actual analytical estimations :

(12) The determination of the K_a and K_b values for ampholytes, from titration curves ; and the correction of formerly accepted constants.

(13) Proof of the inaccuracy of the theory that peptide linkages in dipeptides react with acid or alkali.

(14) A method for calculating the p_H of any mixture of various amino-acids in presence of acid, alkali, etc.

(15) The calculation of the effect of dilution, and of normality of acid or alkali present, on the p_H of an amino-acid mixture.

(16) The theoretical continuation of a simple titration-curve by extrapolation from the early experimental part of the curve.

(17) The explanation of contradictory observations by different observers, *e.g.* on the " alkalinity " of histidine.

Enough has been said to indicate that a large sphere of usefulness awaits the physico-chemical method in its application to bio-chemical problems. In the future there can be little doubt that the titration theory will be put to use for estimation of various constituents in urine, blood, and milk, and other " biological fluids." Further, a great field of research remains open in the application of electrometric and other physico-chemical methods for the quantitative control of such estimations as involve specific reactions between a reagent and the substance to be estimated, or for precipitation reactions. The possibilities of these methods for elucidating qualitative and theoretical problems in bio-chemistry are no less extensive.

SEX-DETERMINATION IN BIRDS

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THROUGHOUT the whole range of living creatures, both plant and animal, the fundamental difference between the male and female organism is almost everywhere apparent, and its consequences are among the most far-reaching and universal in the whole field of biological science. Sex plays so important a part in the life of man, in every sphere of human thought and conduct, that we cannot wonder that the problems which it presents have been of foremost interest from the earliest times and have produced almost as much speculation and research as the central question—"What is Life itself?" Both problems remain as yet unsolved, and it may be, as the late Professor Doncaster has suggested, that neither can be completely solved apart from the other. Recent research has, however, revealed so much concerning the causes which underlie and determine sex that I cannot help feeling that we are on the threshold of a far fuller understanding of this aspect of life.

In the present paper I propose to deal chiefly with recent advances in our knowledge of the determination of sex in birds.

Towards the close of the last century the trend of scientific thought was in favour of a metabolic theory of sex-determination, such as that expounded in 1889 by Geddes and Thompson [8]. They believed that "the deep, constitutional difference between the male and female organism, which makes of the one a sperm-producer and of the other an egg-producer, is due to an initial difference in the balance of chemical changes."

With the present century and its wonderful advances in the field of heredity, consequent on the rediscovery of Mendel's principles, the weight of thought naturally swung over in favour of a Mendelian explanation, and the sex chromosome theory was evolved. An enormous amount of evidence in favour of it has been accumulated, chiefly from the study of sex-limited inheritance and the cytology of the chromosomes.

The sex-chromosome theory postulates that in the majority

of animals the sex- or X-chromosome is present in duplicate in the female, and is single or accompanied only by a small Y-chromosome in the male. Thus the female is homozygous (XX), and the male heterozygous (XY). In certain moths and in birds the evidence, chiefly genetical, seems to point to the male being the homozygous sex and the female the heterozygote. In this case it is usual to represent the male by the formula (ZZ) and the female by (ZW). As I pointed out in a previous paper [3] the fact that the mechanism of distribution of the sex-chromosome is so different in birds and moths from the majority of other animals raises at once a serious difficulty to this theory, for it is hard to imagine how two so widely separated groups could have developed a female heterozygosis for sex. Further, the phenomena of hermaphroditism occurring as they do normally in many monœcious plants and animals, and abnormally even in man himself, are difficult to reconcile with the theory.

In its unqualified form the sex-chromosome theory postulates that sex is absolutely and irrevocably determined at fertilisation by the chromosomal constitution of the fusing sperm and ovum. In the paper already referred to I have reviewed much recent work dealing with the reversal of sex in normally bisexual animals. In those cases where the reversal is supposed to have taken place before sexual differentiation is complete it has been deduced from abnormalities in the sex-ratio on the assumption that the numbers of chromosomal males and females produced would be approximately equal. In many other cases, however, the reversal took place after sexual differentiation and sometimes even after sexual maturity. The question at once arises: How, then, are we to correlate these facts, which cannot be ignored, with the sex-chromosome theory? Obviously we must admit that although the sex-chromosome may be an important factor, others also exist and must be taken into account before any complete theory of sex-determination can be formulated. I wish to emphasise that I fully recognise how convincing is the great body of evidence which has been cited in support of the chromosomal theory of sex and to state that I consider, in the majority of cases at least, the sex-chromosome to be so potent a factor that its presence or absence normally tips up the balance in favour of one or the other sex. In other words, the other factors are normally more or less equally balanced and only in abnormal circumstances do they become sufficiently powerful to outweigh the influence of the chromosomal constitution. Further, the phenomena of hermaphroditism and sex-reversal show that every fertilised ovum contains the potentialities of both sexes although its constitution may be such as to pre-

determine its development in one or the other direction. Having outlined these general conclusions I will proceed to a more detailed description of sexual phenomena in birds and a discussion of their bearing on the problems at issue.

To the late Professor Whitman is due the credit of being the pioneer worker on sex-reversal in birds. After his death Doctor Riddle [13] continued the work, confirming his results and adding much that was new and of considerable importance. Working on pigeons and doves, these experimenters found that by crossing forms more or less distantly related a large proportion of male offspring could be produced. The more distantly related were the birds crossed the more male offspring resulted. For instance, when the parents were belonging to different families or genera all the eggs produced males. If such a pair of birds was overworked throughout the season, by having the eggs removed to other birds as soon as laid, it was found that all the eggs laid during the early part of the season produced males, whereas only females were produced later in the season. During the transition period between both males and females were produced, the former usually from the first egg of each pair and the latter from the second. Very late in the season the eggs laid failed to hatch, the embryos within dying at increasingly early stages of incubation. They showed further that as the birds grew older the period during which only males were produced grew shorter, and the period when the embryos failed to hatch grew longer year after year. The males produced latest in the season were more feminine in their behaviour than those produced earlier, and the same with the females.

Taking these facts into consideration, and remembering the greater vitality commonly exhibited by hybrids, these workers concluded that maleness is associated with a greater amount of developmental energy and femaleness with less, that sexual differentiation is an expression of a quantitative difference in metabolism.

This view is remarkably interesting, being almost identical with the older theory of Geddes and Thompson.

At the same time Riddle admits that certain chromosomes are associated with sex under normal and stable conditions. In other words, he believes that the effect of the sex-chromosomes, normally resulting in the production of one or the other sex, can under certain conditions be outweighed and reversed by other factors which are not normally decisive.

In recent years Riddle has attempted to show that a metabolic difference does exist between the male and female. He has worked chiefly on the blood content and with some success, but without very decisive results. He has shown that

the percentage of blood-fat is higher in the laying hen than in the sexually inactive hen, and that it is still lower in the cock. The blood-sugar content of the male and inactive female is approximately equal, but rises in the actively functioning hen.

Sexual abnormalities in the adult fowl may be divided into three chief groups: (1) Individuals which are more or less completely hermaphrodite throughout life. (2) Males or females, in which the gonads are destroyed by disease. (3) Females, usually old birds of good laying strains, in which degeneration of the ovary, pathological or otherwise, is accompanied by the development of testicular tissue and the more or less complete assumption of the male characters.

Among fowl, birds belonging to the first group are probably the most uncommon. Many examples, however, have been described from time to time. It will be sufficient here to consider one recent case recorded by Hartman and Hamilton [9] which is in many ways particularly interesting.

The bird, a Rhode Island Red, was an apparently normal pullet for the first year. Then the comb and wattles began to enlarge and the bird learned to crow. It layed once while being stroked by the owner. Its behaviour was in some respects male, in others female. At the age of nine years it was killed. The comb was upright and male in character as were also the enormous wattles and the general carriage. There was a well-developed spur on the right leg, but that on the left was rudimentary. The plumage was completely henny. On dissection an ovotestis was found on the left side and a testis on the right. A coiled oviduct and a thin straight *vas deferens* were present on the left and a similar *vas deferens* (but no oviduct) on the right. The ovotestis contained normal oocytes up to 20 mm. in diameter. Ripe spermatozoa were present in the spermatic tubules of both the testis and ovotestis. Cysts were present in the abdominal cavity and large quantities of fat. This bird is of unique interest in that it possessed active functional ovarian and testicular tissue at the same time.

Cases belonging to the second class are common among fowl. One bird, a female, recently examined by Professor Gatenby and myself is typical of this class (Fig. 1). The head furnishings were small and normal for a female.[1] The plumage was intermediate in character but chiefly male. The spurs were well developed. On dissection a small degenerate ovary was found on the left side and a well-developed oviduct such as is normally found in a resting hen. No trace of testicular tissue was present. This case was, therefore, obviously one of pathological gonadectomy. Similar cases occur in males,

[1] The stance of this bird did not appear normal for a female.

FIG 1

FIG. 2

but in them the apparent changes are so slight that they are usually overlooked. It will be seen that birds of this description do not throw much light on the question of the determination of sex, although they may be of considerable interest in relation to the secondary sexual characters.

Recently a number of cases have been described of apparently normal females in various stages of transformation into males. These cases constitute the third of the above-mentioned groups, but it is extremely probable that some of the older records of " hermaphrodite " fowl also belong to it. Dr. Crew [4] and his co-worker, Miss Fell [5] describe a series of eight of these birds " illustrating the conversion of an actively functioning hen into an actively functioning cock." The first member of this series was a Buff Orpington with a good egg-laying record which had hatched chickens from its own eggs. At the age of three and a half years changes began which resulted in its transformation into a male. On being mated to a virginal hen two chickens were bred from it ; one was a male and the other a female. On post-mortem examination a testis and *vas deferens* were found on each side and a thin oviduct on the left. The testis on the left side was embedded in a large tumour. The plumage and head furnishings were entirely male, and externally the bird could only be distinguished from a normal male by its female carriage. Ripe spermatozoa were present in the testes. The second bird was a Rhode Island Red with the plumage more or less intermediate in character. The right spur was 1 cm. long and the left 0·3 cm. An ovotestis was found on dissection on the left and a testis on the right. Two *vasa deferentia* and two oviducts, that on the left being the larger, were present. Ripe spermatozoa were present in the testicular tissue. The third case was of the same breed as the second. Its plumage was intermediate though chiefly henny. The comb and wattles were well developed, as was the left spur ; but the right spur was rudimentary. A testis and *vas deferens* were present on the right and an ovotestis and small oviduct on the left. Spermatozoa were present in the tubules. The remaining birds of the series had the head furnishings of the male and the plumage of the female. The spurs were more or less developed. An ovotestis and an oviduct were present on the left side of each, and in one case a *vas deferens* also. One bird was a Light Sussex, the remaining four being White Leghorns ; in all the seminiferous tubules were immature.

Another case has been described elsewhere by Professor Gatenby and myself [6]. The bird was a White Leghorn (Fig. 2) which was hen-feathered, but with the head-furnishings of the cock. It crowed vigorously. On dissection the abdominal cavity was found to contain a large amount of fat.

An ovotestis was present on the left side. The oviductal apparatus was normal and no *vasa deferentia* were observed. The ovarian tissue of the gonad was fibrous and degenerate, containing no oocytes. The spermatic tubules were immature and contained no spermatozoa.

That these cases are examples of sex-transformation taking place in adult life, of the female fowl changing into the male, cannot be doubted. Simultaneously with the discovery in the British Isles of these cases occurring in nature, a French worker, M. Jacques Benoit [1] succeeded in producing experimentally similar results.

Benoit describes a female White Leghorn chicken from which the left gonad was removed when it was twenty-six days old. When four months old the chicken commenced to develop like a male. At six months its comb was as red and large as that of a normal cock. On operating a second time a testis was found to have developed spontaneously on the right side. After the removal of this organ the comb regressed rapidly and the bird assumed the appearance of the castrated cock.

In support of this experiment Benoit cites some work of Pezard's [10]. Two birds were ovariotomised in 1914, and in 1919 they had developed the sexual characters, the comb and instincts, of the male. Pezard stated that post-mortem examination showed that pathological formations had developed. He considered that these were "séminomes," and he held them responsible for the appearance of male sexual characters.

Benoit concludes from these experiments and from an examination of hermaphrodites that the right rudimentary sexual gland in the hen has the value of a rudimentary testicle and that whenever it happens to develop it gives rise to spermatic tissue. In support of this Benoit states that he finds in female Leghorn fowls of two to two and a half months old a canal stretching from the region of the right rudimentary sexual gland to the cloaca. On histological grounds he identifies this with the epididymus and *vas deferens* of the young male fowl.

While fully admitting the importance of Benoit's experimentally induced sex-reversal, I find myself sharply at issue with him in his conclusion that the right rudimentary sexual gland of the fowl has the value of a rudimentary testis. I do not think that the evidence he brings forward shows that this rudimentary gonad of the adult hen is definitely testicular, although I have no hesitation in admitting that it possesses the potentiality of developing into a testis. But this potentiality is shared by the definitive ovary itself, for in the cases of sex-reversal recorded by Crew, Fell, Gatenby, and myself,

testicular tissue developed late in life in the ovary. Further, it is well known that in many birds the right gonad of the female is developed into a functional ovary. A good instance is given by Riddle [13] in connection with his work already referred to. He says: "It has been found that some females dead at relatively advanced ages show persistent right ovaries. The right ovary in pigeons normally begins degeneration at or before hatching and is usually wholly absent from the week-old squab. In our study it soon became evident that the persistent right ovaries were found almost exclusively in birds hatched from eggs of overworked series. Further study has shown, in addition, that they arise almost wholly from the eggs of autumn, and predominantly then from the second eggs of the clutch. These ovaries have sometimes weighed half or more than half as much as the adult left ovary with which they were associated, and have been found in such birds dead at all periods from a few days to twenty-four months." Finally, Hartman's and Hamilton's hermaphrodite shows that normal functional ovarian and testicular tissue can exist side by side as components of the gonad, having presumably arisen from the same embryonic elements. Benoit's evidence that a rudimentary *vas deferens* exists on the right side of the female chick does not support his view, because the oviduct and *vas deferens* are separate morphological structures, both of which may be developed in the one individual hen and on the same side. The second example in Crew's series of fowl is a particular instance of this, for it possessed an oviduct and *vas deferens* on each side. It is therefore reasonable to assume that the rudiment of a *vas deferens* and of an oviduct exists in every case on the right side as well as on the left of the female fowl. On these grounds I conclude that not only the right rudimentary sex-gland but also the left functional ovary of the adult hen contain elements which are capable of developing into new spermatic or ovarian tissue and that these elements are probably undifferentiated germinal epithelial cells, as has been shown by Gatenby [7].

To the three classes of abnormally sexed birds, described above, must be added, in the present state of our knowledge, a fourth containing the few cases of so-called "gynandromorphs" recorded among birds. Poll [11] has described a case in the bullfinch in which the left side was female in character and sharply separated from the right side, which was male. The plumage of the right side was that of the normal male and in marked contrast to the duller female plumage of the left side. Corresponding to these external characters a testis was found to be present on the right side and an ovary on the left. A similar case in the chaffinch is recorded by

Weber [14]. In it the plumage was female on the left and male on the right, and an ovary was present on the left and a testis on the right. A case in the pheasant is described by Bond [2]. In it the male and female portions of the plumage were not so sharply separated, but on the whole the left side was preponderatingly male and the right female. A spur was present on the left, but not on the right. An ovotestis was discovered on the left, but no trace of a gonad could be found on the right side.

In conclusion, what inferences can legitimately be drawn from these facts? It is obvious that every fertilised ovum contains the hereditary potentialities of both sexes. Normally only one of these finds expression and the organism develops into either a male or a female, although every cell of its body presumably has the hereditary potentiality of the other sex. The question at issue is therefore "what causes the development of one of these alternatives, and not of the other, in normal embryogeny?" According to the sex-chromosome theory this cause is itself hereditary, so that the sex of the individual is determined at fertilisation. Recent work has shown that although this is usually the case other factors which are not hereditary are also concerned and may override or reverse the effect of the sex-chromosome and cause the organism to develop into the other sex. In birds it has been shown that this change may occur at any period in the life of the individual, but, so far, in the adult bird only cases of females changing into males, and not of males changing into females, are known. Further, the balance of sex-determining factors may be such as to produce the development of normal functional male and female tissue simultaneously in the same individual. The development of male tissue does not preclude the possibility of the development of female tissue, and vice versa. Maleness and femaleness are not mutually exclusive, and are both present potentially in every cell of the organism throughout its life, and may at any time be expressed under the influence of suitable causes. At present it is impossible to name with certainty these causes. It seems probable, as already mentioned, that the sex chromosome is an important if not the chief one. Nutrition, hormone action, and other metabolic and pathological conditions probably play their parts.

One thing is pre-eminently apparent: that although our knowledge of the causes which underlie and determine sex is in a stage of rapid growth it is still very incomplete. No line of research promises more results than the study of sex in birds, and I heartily endorse Professor Punnett's [12] words, "It is likely that many a problem in human ethics will be

brought nearer to solution by an intensive study of cocks and hens."

It is impossible for me to close this paper without acknowledging my indebtedness to Professor J. Bronté Gatenby, without whose encouragement and advice it would not have been written.

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PSEUDO-PHOTOGRAPHIC EFFECTS

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By the expression " photographic effect " is usually understood any effect in which light acts on a material substance in such a manner that a visible change is brought about in the substance, this change persisting after the removal of the responsible light-stimulus. The change may be permanent or only temporary, but it must persist for some time, so as to leave a record of the action of the light. Two kinds of photographic effect are recognised, *i.e.* those in which the light itself produces a visible change, for instance as in the blackening of a piece of ordinary photographic print-out paper, and those in which the result of the light action is " latent." This latter class requires the application of some further agency, usually chemical, *e.g.* the ordinary " developer," in order to make visible the effect of the light action.

The most familiar type of photographic effect is that employed the world over in the taking and printing of so-termed photographs. But there are many others which must be placed in the same category. A very common but undesirable form is that of the bleaching of dyes by the action of light, involving much distress to the housewife whose curtains fade on the outside of the folds, but retain their colour beneath them.

A less well-known effect is that exhibited by the visual purple which exists in the eyes of animals and man, and which is of primary importance for the process of vision. This substance is bleached on exposure to light, and it has been found possible to produce an image of a light source, known as an optogram, on, for instance, the retina of a frog's eye. The image on visual purple can be fixed with a solution of alum. This instance corresponds with ordinary photographic printing on print-out materials.

Another example of a different nature may be quoted, which corresponds with the development of the " latent image " on the photographic plate. It is well known that ultra-violet light is capable of destroying bacteria, use of this property being

made in the sterilisation of milk. Browning and Russ in 1917 performed a very pretty experiment by coating a glass plate with gelatin which was inoculated with certain micro-organisms. When this plate was exposed to the spectrum from a tungsten arc, all the bacteria were killed on the part of the plate exposed to wave-lengths less than 2960 Å.U., but those on the rest of the plate were unaffected. On incubating the plate for two days at 37° C., the living bacteria grew copiously, with the result that the plate became opaque, whereas no growth occurred where the bacteria were dead, so that in this region the plate remained transparent. A record was thus made of the action of ultra-violet light, and this must be regarded as much a photographic record as is the result of the action of light on a plate or film which bears a sensitive silver emulsion.

So much for photographic effects. But there are other effects which, without other knowledge of their origin, could be quite reasonably ascribed to the action of light. If on development of a photographic plate, which had not knowingly been exposed to light, a dark patch appeared, access of light to the plate would immediately be suspected. In most cases this would explain the phenomenon, but in other cases a quite different cause would have to be sought in which the action of light was not involved. For instance, exposure of the plate to the vapour of hydrogen peroxide would produce a marked blackening on development, and this could not be termed a photographic effect since the action of light is not the responsible factor.

Such actions as this, in which a change is brought about similar to that produced by light, are conveniently termed "pseudo-photographic effects." As in the case of photographic effects, two classes are distinguishable. If a photographic plate is treated in the dark with a very strong chemical reducing agent, such as hydrazine hydrate solution, the silver salts in the plate are reduced to black metallic silver. This is analogous to the formation of a dark image in the print-out effect in exposure to light.

On the other hand, if a plate is bathed in a solution of sodium arsenite in the dark, no visible change occurs until the plate is treated with an ordinary photographic developer, when the bathed portion develops black. Action similar to that of sodium arsenite is produced on the plate by many chemical reagents and by certain other means, such as pressure, and it is to effects of this nature alone that the term "pseudo-photographic" is usually applied. The phenomena are also sometimes referred to as "photechic effects," and in certain special cases as "Russell effects," "Colson Russell effects," "metallic radiations," "photoactivity," and "Moser rays."

These expressions are, however, in most cases synonymous, so that it is convenient to refer to them all under the one heading of "Pseudo-photographic Effects."

Ability to render a photographic plate developable is exhibited by large numbers of substances of most diverse character. The majority of these were discovered at the end of last century during the investigations of W. J. Russell.[1] Metals, fats, volatile oils, and natural organic bodies like wood, straw, feathers, blood, butterfly wings, and paper have all been shown to possess photographic activity. In many cases exposure of the material to sunlight is necessary to promote its activity, and some of these activated substances can be "sterilised" by being kept in the dark for a few days. In the case of certain metals, activity is promoted by merely scratching or cleaning the surface.

Of the metals, magnesium, aluminium, and zinc appear to be the most active, and the majority of the investigations on the effect have been conducted with zinc as the agent. Other active metals include cadmium, tin, calcium, lead, bismuth, and antimony, and the activity seems to be to some extent a function of the electro-positive character of the element.

The number of organic materials which gives rise to pseudo-photographic effects appears to be enormous. The number of active substances, however, is not so vast as would be imagined, because the activity can in many cases be traced to a common constituent of a number of apparently different bodies. For instance, turpentine, printer's ink, certain kinds of wood, resins, amber, oils of lemon, bergamot oil, copal varnish, some varieties of straw-board, and a number of other substances all possess an activity which can be traced to the presence in them of compounds of the terpene series.

Numerous investigations of the properties of the "activity" have been so fruitful that there is now little doubt as to its nature. It is capable of penetrating thin sheets of gelatin, gutta-percha, gold-beater's skin, tracing paper, and a number of other porous materials, whereas highly glazed and dressed paper, glass, mica, paraffin, and fluorspar are opaque to it. Its passage through thin sheets of aluminium foil was traced to the presence of minute holes in the metal.

The action is not propagated in a rectilinear manner, and can be swept along a bent tube by a current of air. It can also be transferred from an active to an inactive body by contact. In fact, everything points to the view that the active agency is a material substance rather than a radiation, and chemical tests indicate that it is hydrogen peroxide. The

[1] *Proc. Roy. Soc., B.*, 1898, **63**, 102; 1908, **80**, 376; *Proc. Roy. Soc.*, 1899, **64**, 409, etc.

pseudo-photographic action occurs only in the presence of moist air, and only under conditions in which hydrogen peroxide can be formed. In the case of certain freshly exposed metals it is known that hydrogen peroxide formation accompanies oxidation of the surface, and it is also formed during the aerial oxidation of terpenes, etc.

The most convincing evidence in favour of the view that peroxide is the responsible material is the fact that all the phenomena exhibited by the active bodies can be reproduced by the solution and vapour of hydrogen peroxide itself, and to a much more marked degree. Russell found that a developable impression was produced on a dry plate by exposure for eighteen hours to the vapour of a solution containing only one part of peroxide in a million of water.

It thus came to be realised that the action of light on a photographic plate could be duplicated by chemical reagents, and in the course of time a number of other chemical substances was found which were capable of producing a developable latent image on a plate. Hydrogen peroxide, however, has been most investigated in this respect, although during the past few years it has been realised that consideration of pseudo-photographic effects may be of importance for the general theory of photography, and fresh experiments have been made with other active agents.

The parallelism between the action of hydrogen peroxide and certain other reagents and light on the photographic plate is so marked that it is difficult to imagine that the actions of both are not ultimately the same, *i.e.* that both the chemicals and the light act on the same substance in the silver halide grains in the emulsion with formation of the same end-products which give rise to the developable condition of the grains.

The action of hydrogen peroxide [1] increases with the time of exposure of a plate to a solution of definite concentration, and with increase in concentration of the solution for a given time of action, giving rise on development to a density-exposure curve similar in form to the well-known S-shaped characteristic curve for light-exposure. As in the case of light, prolonged exposure to peroxide brings about reversal of the image, and the growth of density and of the slope of the density-exposure curve with time of development increase in the same way as when exposure is to light. It is also found that plates most sensitive to the action of light are most sensitive to hydrogen peroxide, and Svedberg showed in 1920 that the average sensitivity to peroxide is higher for bigger emulsion grains than for smaller ones.

The close similarity between the actions of peroxide and

[1] Sheppard and Wightman, *J. Franklin Inst.*, 1923, 195, 337.

of light on the plate have led many to believe that peroxide acts as a result of the emission of radiation, although the evidence is more in favour of a purely chemical action. The point is not yet definitely settled, however, and so the author was led recently[1] to investigate the pseudo-photographic effects of a number of other chemical reagents in which the action of radiation in its ordinary sense could not be involved.

Experiments showed that an aqueous solution of sodium arsenite (NaH_2AsO_3) reproduced the action of light on a plate as faithfully as did hydrogen peroxide. A characteristic curve was obtained similar to that produced by exposure to light, and having a well-defined reversal portion. Statistical measurements on the distribution of "development centres" among the silver halide grains in a plate, and of their topographical distribution on the individual grains, showed them to follow the same laws as were found by Svedberg[2] and Toy[3] to hold in the case of exposure to light. The term "development centre" is used to denote the points in an exposed grain at which reduction by a developer begins, the centres being rendered visible by cutting short the development in its initial stages. The distribution of development centres represents also the distribution of the latent image.

Among other chemical substances capable of duplicating the action of light on a plate may be mentioned sodium hypophosphite, ozone, stannous chloride in acid solution, persulphates, certain acids, and very dilute developers. There is one remarkable fact to be noted about these reagents, and that is that they none of them react chemically with silver bromide. This fact is of vital importance for photographic theory, for it raises the question, "If these agents do not react with the silver halide of the grains in a photographic emulsion, how is it that by merely bathing a plate in a solution of one of them, the grains are made developable?"

Consideration of this problem indicates that the chemical substances in solution could probably react with something at the surface of the grains which is not silver halide, so giving rise to the developable condition. Although ordinary silver bromide, precipitated from aqueous solutions of alkali bromide and silver nitrate, is almost immediately reduced by a developing solution, the rate of reduction is enormously lowered if the precipitation is carried out in the presence of a colloid such as gelatin, which acts as a protecting agent for the silver halide. In the presence of nuclei in contact with the silver halide in gelatin, however, the reduction is accelerated again, and in the case of an ordinary light-exposure of a photographic

[1] *Brit. J. Phot.*, 1922, **69**, 462; 1923, **70**, 717; *Phot. J.*, 1923, **63**, 237.
[2] *Phot. J.*, 1922, **62**, 186.
[3] *Phil. Mag.*, 1922, **44**, 352.

plate, it is the function of the light to provide such nuclei. Since certain chemical substances also make a plate developable, they must act by forming the nuclei necessary for development to occur.

Observations indicate that the "latent image" due to an exposure to light is distributed on the silver halide grains in the same way as that due to a chemical such as sodium arsenite, and, in fact, that both light and chemical substances which give rise to pseudo-photographic effects act on the same points in the grains. Since, as we have seen, sodium arsenite, etc., appear to act on some other material and not directly on the normal silver halide, it follows that light also acts on this same material, so giving rise to the nuclei for development. This "foreign" substance could act either by being itself more sensitive to light and chemical reagents, so giving the nuclei directly, or else by promoting catalytically the sensitivity of the silver halide to light and chemicals. It seems most probable that the latter is the case, the sensitivity-promoting substance being adsorbed on the surface of the grains.

The nuclei in exposed grains probably consist of metallic silver, although nuclei of other elements, such as gold, also act as development centres if they can be formed on the grains. The power of a nucleus to act as a centre for development appears to depend on its size, and it is probable that for a given developer under defined conditions it is necessary to attain a certain minimum size of nucleus before development will take place. This has its analogy in other well-known chemical phenomena. For instance, Ostwald found that a certain minimum size of nucleus was necessary to bring about crystallisation from a super-saturated solution of a salt. Zsigmondy investigated the influence of colloidal gold particles in bringing about deposition of gold from gold-reduction mixtures, and found that here again a certain minimum size of gold nucleus was necessary before deposition would occur. In this case the size was $2\mu\mu$, or a particle of 10^{-16} mg. weight.

Accepting Zsigmondy's value for the minimum size of gold nucleus, and taking $1 \cdot 66 \times 10^{-24}$ gm. as the weight of an atom of hydrogen, it is found that this nucleus must contain about 300 atoms of gold. It seems quite probable, then, that the development nucleus of silver in a grain of a photographic plate would be of this order of size. Anyway, it must be a group of silver atoms confined within a certain limited area, and not merely an isolated neutral silver atom.

If this is the case, then the sensitivity of a silver halide grain in an emulsion will depend on the amount of energy required to give rise to the necessary group of silver atoms. It should be possible to have, say, 100 neutral silver atoms

existing on a grain, spread uniformly over its surface at relatively large distances apart, without the grain being developable by a given developer; but if these 100 atoms were grouped together in one cluster in one particular part of the lattice surface, the grain would then be developable.

It has been pointed out earlier in this paper that on prolonged action of certain chemical reagents the image obtained on development reaches a maximum density and then decreases with further treatment. That is, reversal sets in, comparable with that observed when exposure is to light. In the case of hydrogen peroxide, at any rate, some workers have taken this as strong evidence that the action of peroxide is due to evolution of radiation of the same nature as light. Recent experiments, however, have shown that reversal due to peroxide and sodium arsenite is quite different in origin from that due to light.

Peroxide and arsenite have been found to exhibit a dispersing or peptising action on the latent image, and it is extremely probable that reversal of the image on prolonged exposure to peroxide or arsenite is due to peptisation of the nucleus material formed during the first stages of the action. Developability will increase as long as the rate of formation of nuclei exceeds the velocity with which they are removed; but if, owing to the presence of a limited amount of material capable of forming nuclei, the rate of destruction exceeds that of formation, reversal will set in.

It appears that in the reversal region, at any rate, the analogy between the actions of peroxide and arsenite and of light breaks down, unless it can be shown either that light itself exerts a dispersing action on the latent image, or else that it gives rise in the plate to something of a chemical nature which can perform the same function. Light has in certain cases been shown capable of dispersing solids in contact with liquid media such as alcohol or water, but on the other hand it is also known to produce a coagulating effect on colloidal particles, so that it is not clear without direct experiment what result could be expected. A number of experiments have been carried out recently, however,[1] which indicate that light itself does not disperse the latent image in a photographic plate, neither does it indirectly bring about such a dispersion by intermediate formation of peptising agents. The presence of the latent image alone is a sufficient condition for reversal by such fogging agents as hydrogen peroxide and sodium arsenite, whereas in the case of reversal by light, the presence of the silver halide in conjunction with the latent image is necessary.

It has been pointed out that it seems probable the sensitive

[1] *Phot. J.*, August 1924.

material which functions in exposure of a plate to light is the same as that which reacts to give development nuclei on treatment with fogging agents. If this were so, it would be expected that a systematic classification of all the chemical reagents which can bring about plate developability should give some indication as to the nature of sensitivity and of the material to which it is due. Attempts at classification, however, based on grouping the substances according to their chemical types, have failed. It was pointed out some years ago by Schaum,[1] who attempted a classification, that plates are made developable by so many chemical reagents that it is easier to consider the inactive substances than those which affect the plate. This statement is rather too sweeping, although there seems to be no class of chemical reagent which is incapable of exerting some effect.

Let us consider some of the difficulties of classification. A first glance over the list of fogging agents reveals a number of very active substances which are all mild reducing agents. These are sodium arsenite, stannous chloride, sodium hypophosphite, and diluted metol-quinol developer. Without further inspection it might be concluded that this indicated that the sensitive material was of such a nature that it was reduced by a mild chemical reducing agent. Examination, however, also reveals the fact that nitric acid, sulphuric acid, and solutions of persulphates give the same effect as the mild reducers. This would require the sensitive material to be readily oxidised, so giving the same products as are obtained by reduction—a very improbable result. There are numerous other reactions which bring about formation of a latent image which also indicate an oxidising action, but the evidence is so conflicting and the types of fogging agents so diverse that it is impossible to ascertain the chemical nature of sensitivity from a study of them.

It is conceivable that different fogging agents could act in different manners in giving rise to the developability of a plate, but this is not readily intelligible, since the results obtained are all similar to one another. Also it must be borne in mind that any explanation which applies to the action of fogging agents will in all probability have to apply to the action of light as well. The suggestion occurs that chemical reagents could possibly make the silver halide grains developable by undoing in some way the protective action of the gelatin, but it is not clear from this why grains most sensitive to light should also be most sensitive to fogging agents, unless the action of light is to break down the protective action of the gelatin. Certain workers have suggested that the action of light on the plate is to burst a sheath of gelatin which surrounds

[1] *Z. wiss. Phot.*, 1904, **2**, 205.

the grains, but the evidence supplied is very frail, whereas evidence is strong that absorption of light by the silver halide itself plays the major part in latent-image formation, indicating that developability is due to action in the grain and not on the adsorbed gelatin.

A study of pseudo-photographic actions has thus been of considerable value in modifying our views of the nature of the sensitiveness to light of a photographic emulsion, although the actual cause of sensitivity is still very obscure, and can only be ascertained by careful and systematic study of emulsion manufacture under precisely defined conditions. The chief stumbling-block is the gelatin, that material of indefinite and variable composition, the use of which is essential for the preparation of emulsions of high speed coupled with other suitable properties.

ART AND MEDICINE IN CHINA

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The action of the State Department in taking the leadership in making treaties and agreements regarding oriental matters, compared with its reluctance to become concerned with European affairs, is significant. It means, fundamentally, that we are faced by a great task, which we may not side-step, namely, to find some friendly method of adjustment between the teeming millions of the Orient and the less densely populated Occident. Those who live along the Pacific coast of North America, in the Hawaiian Islands, in Australia, in South Africa, and in other places where the two civilisations are in sufficiently close contact to lead to competition appreciate the seriousness of the problem. Already it has been brought to their attention by riots and by bloodshed. Coincident with this realisation, and to some extent because of it, there is developing in our minds a keen interest in the Far East not shared by our forefathers. But the Chinese, who will dominate in the Orient, study much more carefully our civilisation than we do theirs. If reason is to be the order of the day, and we are not to enter the negotiations blindfolded, more knowledge we must have. Information of all kinds we need. Not the least important is a sympathetic understanding—as far as is possible—of the influences which have shaped the development of Chinese culture.

This may be approached from several angles, but for us in medicine a valuable point of departure is afforded by the clear and searching account given by Garrison and Streeter [1] of the interdependence of art and medicine in Europe. When we turn to the Chinese, with the data which these authors have supplied concerning the evolution of Western medicine, we find that nature has performed a gigantic experiment by virtue of which entirely different conditions have been imposed

[1] Garrison, F. H., and Streeter, E. C., "Sculpture and Painting as Modes of Anatomical Illustration," printed as an appendix to Choulant's *History and Bibliography of Anatomic Illustration* (The University of Chicago Press, 1920).

upon a very gifted race. These conditions have produced in China results which it may be interesting to pass briefly in review because it is possible that they may afford side-lights upon the development of medical science in both the East and the West which might not be so readily detected by considering each separately. Since "Medicine is the mother of sciences" [1] and the sciences are themselves the framework of civilisation, a discussion of this kind may be rationalised as a part of the larger problem before us.

I. Association between Art and Medicine in Europe

It will be helpful, before entering upon a discussion of conditions in China, to recall the fact that in the West, at a very early date, a tendency manifested itself in favour of realism in the accurate portrayal of the body. For the Greeks, nudity was a "festal costume" upheld by tradition and sanctioned by the State. The human body, moulded in the image of the gods, was for them a creation of divine beauty to the study of which many of them gladly devoted their lives as artists, professional gymnasts, and physicians. At the Olympic Games, before the eyes of the whole nation, a premium was placed upon good physical development. Sudhoff [2] aptly remarks that, for the first time in the history of civilisation, and to an extent never again approached, universal training was adopted "with a view to the harmonious development of all the physical faculties, and to the attainment of the greatest measure of strength, dexterity, and self-confidence, of physical perfection and beauty."

The consequences in Europe were many and far-reaching. The Greek artists and their Italian successors of the Renaissance were men of keen perceptions, who, not content with superficialities, delved deeply into the hidden mysteries of our bodily organisation. We owe more of our civilisation to Giotto, Donatello, and their followers than we are wont to acknowledge. We read that ". . . the smallest mortuary chamber, cubicle, or side-chapel in the charnel-house sufficed the artist—a cellar, a burial-pit—it mattered not. . . ." [3] They acted as the standard-bearers of the medical profession, and so profound became their learning that the great Vesalius permitted himself to exclaim:

"As for those painters and sculptors who flocked about me at my dissections I never allowed myself to get worked up about them to the point of feeling that I was less favoured than these men, for all their superior airs."

[1] Welch, W. H., *Collected Papers and Addresses*, 1920, vol. iii, p. 86.
[2] Sudhoff, Karl, *Ann. Med. Hist.*, 1918, vol. i, p. 111.
[3] Garrison and Streeter, *ibid.*

This growing together of art and medicine was not a chance affair, but developed into a definite business relationship. It is significant, as noted by Garrison and Streeter, that the painters became early (A.D. 1303) incorporated with the great "Guild of Physicians and Apothecaries."[1] "Being beholden for their supplies of pigments to the apothecaries and their agents in foreign lands." "This guild relationship endured for more than two and a half centuries, furnishing innumerable points of magnetic contact between science and art."

The artists of the Renaissance opened the road to progress in still another way. By decorating cathedrals and basilicas with beautiful paintings throughout the length and breadth of the land, they secured the good-will, and in some cases the tacit consent of the Church to the practice of human dissection upon which their art depended. What might have been organised opposition on the part of the all-powerful Church of Rome they converted for a time into a kind of passive acquiescence, except when the clergy were forced into action by the fiery eloquence of Savonarola and during other periods of reaction.

In reference to the supreme masters, Leonardo, Michelangelo, and Raphael, we cannot do better than quote the words of Garrison and Streeter as follows:

"What ardours and endurances for science, what trials in the fiery furnace, had these three not passed through—Leonardo in particular! Florentines well remembered how, in the year 1505, the city had gone into entire submission before Leonardo's divinely drawn cartoon for 'The Battle of the Standard,' and the competing cartoon by Michelangelo, 'The Surprise, by the Pisans, of Florentine Soldiers bathing in the Arno.' "One of these cartoons was placed in the Medici Palace and one in the Pope's Hall; and while they could be seen there they were the school of all the world," wrote Benvenuto Cellini. "So decisive was the display, by these establishers of dissection, that there was no room henceforth for faulty drawing of the nude figure in action."

The steady income of accurate information thus secured regarding the human body has been most liberating. It has been largely responsible for the gradual exit of dogma and superstition, and has paved the way for the doctrine of evolution. It has enabled us more correctly to orient ourselves in the universe about us, and has stimulated clear thinking and with it the desire to find out things for ourselves—for experimentation—which stands at the root of the

[1] La Sorsa, *l'Arte dei Medici, Speciali e Merciai* (Molfetta, 1907). E. Stanley, *Guilds of Florence* (London: Methuen).

development of the natural sciences.[1] The action of the artists in constantly holding up before the people the beauty of the healthy human form as an ideal worthy of great effort to attain has, we believe, exercised a profound influence in the development of medical science.

II. Divergence of Art and Medicine in China and some of its Consequences

But in China it has been quite otherwise. Pictorial art developed gradually in the form of line-drawings closely related to writing (itself of ideographic origin), in fact somewhat as graphic fables. "Not to represent facts, but to suggest a poetic idea (often perfumed, so to speak, with the reminiscence of some actual poem) has ever been the Chinese artists' aim."[2] Many artists were great scholars, and many of the scholars were experts in calligraphy and painters of repute (as, for example, was the poet Tu Fu,[3] so that a close association between art and philosophy was inevitable; but at no point was there any vivifying and mutually helpful point of contact between art and science.

Fostered by an imperial court, which outshone Greece and Rome in splendour and magnificence, and which created an eager market for their talents, the naturally gifted Chinese artists achieved wonderful success in painting, in bronzes, and in ceramics. But they did not turn their attention to sculpture except as a means of demonstrating their technique and digital skill, choosing for materials jade and other substances extremely difficult to work with, and revelling in intricacy of detail and bizarre effects, to the sacrifice of realism and accuracy. According to Holmes:

"With a passion unmatched in Europe till Wordsworth's day, the Sung artists portrayed their delight in mountains, mists, plunging torrents, the flight of wild geese from reed-beds, the moonlit reveries of sages in forest solitudes, the fisherman in his boat on lake or stream. To them also, steeped in the Zen philosophy of contemplation, a flowering branch was no mere subject for a decorative study, but a symbol of the infinite life of nature. A mere hint to the spectators

[1] Astronomy and the "exact sciences" may have developed along a slightly different path, through the observation of inanimate nature, the laws governing falling bodies, the swing of the pendulum, etc.; but, like the "natural sciences," they were undoubtedly fostered by a similar spirit of inquiry.

[2] Holmes, C. J., "Chinese Art," *Encyclopædia Britannica*, 11th edition, vol. vi, pp. 213–16 (1910).

[3] Cranmer-Byng, L., *A Lute of Jade* (London: John Murray, 1917).

is often all that they rely upon; proof of the singular fulness and reality of the culture of the time. The art of suggestion was never carried further."

Ma Yuan, Hsia Kuei, and the Emperor Hwei-tsung were the acknowledged leaders of this particular school. The powerful combination thus effected of art, literature, and philosophy, under the patronage of the "Son of Heaven" himself, fortified a national ideal that had been slowly forming for many years, which, in its meditative aspect and in its detachment from worldly affairs, was not, we may suppose, displeasing to the Taoist and Confucian priests, if indeed they appreciated the trend of events. Unfortunately, the movement gained momentum, became irresistible, and created the impression that the body is of no importance, that it is something to be hidden, of which almost to be ashamed [1]—a point of view which also fell in line with the teachings of the followers of Buddha. Thus a strong and robust constitution, with the suggestion of physical brute force, came to be considered to be inconsistent with the profession of a scholar and a gentleman. So that, to this day, Chinese students are apt to cultivate a studious droop of the shoulders and to wear spectacles fitted with plain glass merely to create the impression of learning.

This unconscious emphasis, by those who shaped the opinion of the nation, upon the rigid Confucian code of behaviour and the subordination of the body and all its concerns is exemplified by two practices which would be revolting to the senses of a people imbued with a realisation of the beauty of the human form.

For many years young girls have been made to suffer agonies during the growing period when their feet are tightly bound to keep them small and to produce a dainty, pointed shape. It is said that in some districts the sufferers are kept in out-houses so that their cries will not break in upon the "monumental calm" of the rest of the household. But I have been unable to verify this statement. The economic loss to the nation of having a large proportion of the population crippled for life is considerable. And this is not the whole story, because the impossibility of taking adequate exercise has had a most debilitating effect. Fortunately neither the Manchus nor the Mongols have adopted the custom, but

[1] Chinese physicians prescribing for women are rarely, if ever, permitted to make physical examinations, or even to see the patients. In some cases (according to Juliet Bredon) they are allowed to feel the pulse at the wrist, upon the variations in which they place great store, or they may make use of a little statuette, about six inches in length, upon which the sufferer marks the location of her trouble.

numerically they are greatly in the minority. The pity of it is that, in spite of the efforts of "Young China," this mutilation is being continued, except in a very few localities where foreign influence has been felt for a century or more. No thought seems to be given by the Chinese to the crumpled and distorted bones within the silken slippers. Another practice, but one less familiar to Westerners, and possibly less serious in its effects, consists of tightly binding the breasts of girls before marriage with a view to giving the body a straight, up-and-down appearance.

What appears to us unsightly in limbs curiously warped and misshapen by inhuman toil, and in the parade of disfiguring diseases through the streets, makes apparently no impression upon the æsthetic senses of the Chinese, at least no impression sufficient to arouse in them an impelling desire to change the conditions which bring about these wrecks of humanity. For their own sake they will throw a few coppers, thereby taking merit unto themselves and lending to bodily deformities a definite monetary value.[1]

The physicians, hedged in by a *modus vivendi* which frowns upon any innovation tending to disturb the *status quo*, do not feel called upon to defy all conventions in order to advance their science. Instead they follow the path of least resistance, doubtless believing that in the end the good will prevail, and content themselves with but few observations, made under peculiar and interesting conditions.

For instance, we read in Hsieh's *Review of Ancient Chinese Anatomy*,[2] that Liang Shao Pao in A.D. 960, "sent his medical officer with an artist to make pictures during an execution of thieves, probably by the rather slow slicing process"; and, again, that in the days of Chia Ching, A.D. 1796, a terrible epidemic raged among the children in the town of Chang Li Hsien, and many died. A certain magistrate, named Wang Chui Jen, visited the public cemetery and found that, since the children were buried in very shallow graves, the hungry dogs were able to uncover the bodies and devour them. Wang's curiosity was so stimulated that he went daily to the cemetery and observed over thirty complete bodies dismembered by the dogs. He was thus enabled to test out old theories and to make important new observations which formed the basis for his book, which he called *A Correction of Faults in Medicine*. What a contrast between this dignified old mandarin who stood aloof in his silken robes waiting for the dogs to help

[1] To loosen up the donations a touch of gruesomeness is sometimes added by the use of a liberal supply of chicken's blood, as described by Doctor Wu Lien Teh (*National Med. J. of China*, 1920, vol. vi).

[2] Hsieh, E. T., *Anat. Rec.*, 1921, vol. xx, p. 97.

him, and contemporary artists and doctors in Europe who had such a thirst for knowledge that they spent night and day dissecting with their own hands, and did not scruple to resort to the dangerous practice of "body-snatching" in order to secure an abundance of material ! [1]

If Ma Yuan, Hsia Kuei, and the Emperor Hwei-tsung had lived a few years later and could have visited the town on the Arno, as Marco Polo visited the court at Peking, it is possible that, while with true oriental courtesy admiring the perfection of technique of the Florentines, in their heart of hearts they might have felt the subjects ill-chosen and might have looked in vain for the colourful imagery and the poetic suggestiveness which they themselves knew so well how to convey.[2] Certainly the palaces of the Medici and the ruins of Imperial Rome would have seemed to these Sung masters rather small and commonplace as compared with their native China. A world of difference existed between their motif and inspiration and that of the leaders in Italian art. If it were not so the human body might not still remain for their countrymen a citadel of superstition and dread which few have attempted to storm by direct observation, and in consequence of which the whole realm of living things, with rare exceptions, might not for them still be peopled with all manner of spirits and ghostly influences.[3]

We find ourselves in agreement with His Excellency Li Yuan-hung, Ex-President of China, in his statement that ". . . dissection in anatomy alone has removed a great superstition regarding the dead, and thus opened the highway to scientific progress," [4] only, in our opinion, a change for the better has hardly yet set in. China is held back by a combina-

[1] In reference to the town of Tenghsien, visited in 1922 (?), Harry A. Frank writes as follows: "Sometimes Tenghsien buries its children, like those of its paupers who do not belong to the beggars' guild, in such shallow, careless graves that the dogs habitually dig them up again. These surly brutes sat licking their chops here and there on the outskirts of the town, among discoloured rags of what had once been cotton-padded clothing scattered about little mussed-up holes in the ground."—*Wandering in Northern China*, p. 303 (The Century Co., New York, 1923).

[2] When Lord Macartney came to Peking "bringing with him several pictures as presents from George III, the mandarins in waiting were shocked by the shadows, and they gravely asked if the originals of the portraits really had one side of the face darker than the other: the shaded nose was a grave defect in their eyes, and some of them believed that it had come there accidentally."—Quoted from Juliet Bredon, original in Bushell's *Chinese Art Handbook*.

[3] To this day a "modern Chinese anatomist" will discourse upon whether or not the heart contains any blood, not troubling to look for himself (John Dudgeon, *National Med. J. of China*, 1919, vol. v, p. 50).

[4] Address to the students of the Peking Union Medical College, September 1923.

tion of circumstances, some of which, however, are intertwined and related, as we have attempted to indicate. Another consequence of the fact that medicine has been allowed to persist in China in a very primitive state without any support from other branches of learning is that its practice has never come to be considered a worth-while profession, so much so that one of the most serious problems faced by Chinese educators and by the China Medical Board of the Rockefeller Foundation is to create an *esprit de corps*, and to demonstrate that the art of healing is in fact a lofty calling capable of appealing to man's highest ideals. The reluctance of young men to come forward and to avail themselves of the opportunities offered in their own institutions as well as by foreigners in China has been so marked as to elicit editorial comment in the *China Medical Journal*. Professor Dewey has said, with reason, that we are faced by the task of " the transformation of the mind of China," the term " transformation " being used in a constructive, not a destructive, sense.

By contrast it is fairly safe to assume that one of the forces which lent dignity to the profession in Europe was this early association, which we have mentioned, between medicine and art. In this connection it is interesting to recall [1] that, in September 1276, the Conclave of Cardinals unanimously elected one, Petrus Hispanus, a Portuguese physician, to the highest office in Christendom, that of Pope, and that he served successfully under the name of John XXI as representative of God on earth and as the keeper of the keys of heaven. It was this same John XXI who received delegates from Kublai Khan. He was a fellow student with Roger Bacon. The Cardinals may have been governed by diplomacy in selecting a Pope who was neither Italian nor French, but it is certain that his training in medicine did not stand in his way. In China, to appoint a member of the lowly and rather despised cult of medicine to serve as Emperor or " Son of Heaven " would have been sacrilege unthinkable.

III. Some Factors which may underlie this Divergence

It is significant in this discussion of art and medicine in China that one naturally drifts into the use of the present tense. It means that to this day there has been comparatively little change in the point of view and ideals of the general mass of the people. The same inhibiting factors are in force that were evident thousands of years ago. Indeed it is this immutability and inertia which the Chinese point to as a

[1] Riesman, David, " A Physician in the Papal Chair," *Ann. Med. Hist.*, 1924, vol. v, pp. 291–300.

characteristic of their civilisation. They take pride in the knowledge that their culture and their race have stood firm during the rise and fall of the Roman Empire, and have absorbed and assimilated many a foreign invasion. But why, in all this time, has there been so little progress in the direction taken in the West?

We have indicated several circumstances which may have, among others, tended to retard the development of the natural sciences—hence we cannot agree entirely with Bertrand Russell that the Chinese civilisation "never contained anything hostile to science"[1]—but these influences are merely the results of their attitude toward life, which, in turn, is dependent upon elusive and fundamental forces which must have taken their origin before the country became densely populated, and many centuries prior to the introduction of Buddhism.

The Chinese discovered coal, made glass and several metallic alloys, constructed compasses, manufactured paper, and perfected printing at an early date, but they failed in large measure to profit by these instruments for advance. Mention may also be made of the vexed question of the discovery of gunpowder. More than 1,500 years ago massage was recognised as a therapeutic measure, 1,200 years ago inoculation for smallpox was initiated, and as early as the seventh century A.D. sheep's thyroids were administered for cretinism[2]; but no discoveries in medicine or in the allied sciences were pushed to their logical conclusion. How phenomenal that this nation, composed of individuals endowed with intellectual powers of so high a standard, should remain impassive and apparently uninterested!

It is not to be expected that an explanation will be found in one factor or in any group of factors. Even to approach such a problem is difficult, to project ourselves into the remote past, imperfectly recorded, of a civilisation so divergent from our own and accurately to evaluate influences then at work is almost beyond us. As Juliet Bredon has said, "We can more easily imagine ourselves taking afternoon tea with Lorenzo the Magnificent, or even with Cæsar Borgia, than with Yung Loh or Chien Lung."[3] We are inclined to consider China a vast museum of surpassing interest, because its contemplation gives us momentary release from the rather feverish excitement of our twentieth-century routine. We are led unconsciously to look for the unusual, and thus perhaps to be a little narrow

[1] Russell, Bertrand, *The Problem of China* (The Century Company, New York, 1922).

[2] Hume, E. H., "The Contributions of China to the Science and Art of Medicine," *Science*, 1924, vol. lix, p. 345.

[3] Bredon, Juliet, *Peking* (Kelly and Walsh, Ltd., Shanghai, 1920), p. 112.

in our point of view, and to gloss over influences which are not new to us and which may operate in the development of all civilisations. We are faced by a mass of contradictory evidence, generalisations are extremely difficult to establish without innumerable qualifications, and the more we know of China the less safe do they appear, so that we may attempt to synthesise only warily and with caution, each person naturally emphasising data concerning which he is least ignorant, for it is all a question of degree of ignorance among foreigners.

In this brief discussion we shall have to limit ourselves first to hypothetical internal forces, like the endocrines, and then pass to external influences, of which the environment is the most important and the simplest to consider.

We owe to Sir Arthur Keith the suggestion that variations in the glands of internal secretion are important, if little known, factors in shaping the trend of differentiation.[1] To a reduction or alteration in the activity of the thyroid gland he attributes the arrest in the growth of " Mongols " (under which heading he includes the Chinese) which is manifested in the basal parts of the skull, " with the result that the root of the nose appears to be flattened and drawn backward between the eyes, the upper forehead appears projecting or bulging, the face appears flattened, and the bony scaffolding of the nose . . . is greatly reduced." Common experience tells us that in Europeans we meet with a different series of characteristics. " The sharp and pronounced nasalisation of the face, the tendency to strong eyebrow ridges, the prominent chin, the tendency to bulk of body and height of stature," are, in his opinion, indicative of a relatively high rate of pituitary activity. Sir Arthur Keith also explains the relatively beardless face and the tendency to an almost completely hairless body of the Mongol as a manifestation of abeyance on the part of the interstitial glands. He believes that " we Europeans owe the fairness of our skins to some peculiar virtue resident in the suprarenal bodies." But he does not dwell upon the influence of the endocrines in moulding the temperament of individuals and of races, although behaviour can only be interpreted as an outward and visible sign of an inward and physiological mechanism.

It is a seductive line of thought that relative hypofunction (if we may use this term) of the thyroid, pituitary, and interstitial glands may explain the restfulness and complacency amounting almost to apathy sometimes noticeable in the

[1] Keith, Sir A., " The part played by the Endocrine Glands in the Evolution of Man," *Lancet*, 1921, vol. xi, p. 588. " The Evolution of Human Races in the light of Hormone Theory," *Johns Hopkins Hospital Bulletin*, 1922, vol. xxxiii, pp. 155, 195, and earlier papers.

Chinese, but we dare not rely too firmly upon this theory in framing any tentative explanation of the facts before us until we can learn more of the actual physiology of the glands at the present time, and this will not be a simple matter. Our best laboratory tests are only qualified to reveal pronounced variations in activity, so that wholly negative observations would not preclude the possibility that we are faced by relatively slight variations which manifest themselves only after action through thousands of years.[1] But, granting that these differences existed long enough in the past to produce physical (and mental ?) characteristics which have become hereditary, though the exciting causes may have since disappeared or may still persist and be operative to-day, the question remains as to how they arose in the first place. Anthropological evidence tending to show that the Chinese have noticeably changed during the historic period is wholly lacking.

Let us turn now to the environment. We cannot but regard man as an animal gradually becoming social, a creature of his surroundings, blessed or cursed by the operation of little-known hereditary laws. This environment is directive, probably more so than the endocrines and perhaps fundamental to variations in them. Our outlook upon evolution is changing. It is possible to discern in the founding of departments of geography in one after another of our leading institutions a sign of the times, indicating that we are destined to hear much more about the influence of the geographic features of our surroundings upon the differentiation of national and personal traits.[2] But environment is not all-embracing. The problem is complex, and we gain nothing by minimising it. While the mechanism of hereditary differentiation in the simplest animals, with which we may experiment at will, remains so obscure, how can we expect to make any progress with man! In order even to orient ourselves we must gain a little appreciation of the trend of recent developments in endocrinology, in geography, and in genetics, as well as in the older sciences of anthropology and sociology.

In this paper we may not attempt more than to stress the environmental factor by trying to reconstruct the conditions in Eastern Asia during the formative period of Chinese character, that is to say, before and during the Shang dynasty (1766–1122 B.C.). It may be difficult for us to share Elliot Smith's opinion

[1] We have accurate data to the effect that the systolic blood pressure of normal Chinese is lower than the normal for people in Europe and America, and it is suggested that *one* factor may be deficiency in the suprarenal and other hormones (Cadbury, W. W., *Arch. Int. Med.*, 1922, vol. xxx, p. 362).

[2] For a detailed discussion of the "Climate and Evolution," see W. D. Matthew, *Ann. New York Acad. Sci.*, 1915, **24**, p. 171; and Hervy W. Shimer, *Science*, 1924, **59**, 199.

(as applied to China) that there is "sufficient information to justify the conclusion that many of the fundamental conceptions of Indian, Chinese, and Japanese civilisation were planted in their respective countries by the great cultural wave which set out from the African coast not long before the sixth century B.C."[1] In fact, the evidence at hand seems to point to the opposite conclusion, namely, that Chinese civilisation is almost wholly of endogenous origin.

Elliot Smith is an exponent of the theory of "convection" according to which culture is thought to have spread from a common centre (Egypt). The opposite idea of "convergence," that primitive races tend by the observation of similar natural forces to develop similar ideas, is expressed by Garrison[2] in regard to medicine, and by Chavannes[3] in respect to Chinese art. The difference between the points of view is one of degree of interpretation only, because differentiation must necessarily proceed in both ways. But the date of the cultural wave described by Elliot Smith is perhaps too recent to have been really effective in China, impinging upon a race already far advanced in philosophic thought. Confucius, living between 551 and 479 B.C., described himself as "a transmitter, not a maker, one who loved and believed in the ancients." The legendary figure Lao Tzu looms up at a still earlier period, but at a time when there is every reason to believe that the people had already developed their distinctive temperamental characteristics.[4] It is true that a few isolated traces of contact with Egypt remain to us. For example, certain rock carving in Shantung caves are mentioned by Fenollosa.[5] Giles[6] refers to some old sculptures collected for a mausoleum by the Wu family in A.D. 147, which are still the subject of controversy. Douglas feels that they show unmistakable Egyptian influence which, however, Paleologue and Chavannes deny (Giles, p. 12). Traces of Egyptian influence are mentioned by Barbezieux,[7]

[1] Elliot Smith, G., *The Influence of Ancient Egyptian Civilisation in the East and in America* (Manchester: The University Press, 1916).

[2] Garrison, Fielding H., *An Introduction to the History of Medicine* (Philadelphia: W. B. Saunders Co., 1917).

[3] Chavannes, Édouard, *Les Mémoires Historiques de Se-Ma Ts'ien.* (Paris: Ernest Leroux, 1895).

[4] The first date in Chinese history which may be definitely compared with ours is that of a solar eclipse observed in both the East and the West in 776 B.C. (Hirth, F., *Ancient History of China.* Columbia University Press: 1911).

[5] Fenollosa, E. F., *Epochs of Chinese and Japanese Art* (New York: F. A. Stokes, Co., 1913.)

[6] Giles, H. A., *An Introduction to the History of Chinese Pictorial Art* (Shanghai: Kelly and Walsh, 1918).

[7] Barbezieux, G., *Contribution à l'Étude de l'Histoire de la Lèpre* (*Janus*, 1914, p. 19).

De Zwann,[1] and others. And I have myself reported some anatomical diagrams possibly of Egyptian origin.[2] According to Cordier,[3] references may be found to the Chinese in the writings of Virgil and Horace. A scholarly treatise dealing with the relations between Rome and China was published some thirty-nine years ago by Hirth.[4] But the most fascinating and realistic description of the reaching out of the Chinese toward the West over the great central Asian plateau is given by Mervin.[5] It is based upon the trade in Chinese silks, which apparently assumed considerable proportions in the Han dynasty, and it is cast in the form of an historic romance. In succeeding centuries China expanded a little further under the T'angs, but soon withdrew into herself and lost effective touch with Europe, so that the contact with the West was merely ephemeral, confined only to very small minorities, and wholly insufficient to noticeably modify the separate evolution of civilisation in the Orient and the Occident.

The Shang dynasty is so remote that we have no historical records to guide us, because authentic history in China does not compare in completeness with that in Egypt, but it is safe to assume that the population could not have been dense in the sense that we see it now, so that Professor Dewey's explanation of Chinese behaviour in terms of the "psychology of a crowd" could not then have been operative, and probably he did not intend to apply his theory to that period. The explanation presupposes a complex and stable social organisation in which any action of an individual tending to cause a wholesale readjustment is frowned upon and discouraged. But with steady increases in number of a race famed for its fecundity, this group sentiment in all probability became a powerful factor in creating a conservatism more rigid year after year, and tending ever more and more to fix and perpetuate any tendencies taking their origin at an earlier time. That cultural and artistic ideals did become ingrained in Chinese culture very early is clearly illustrated by the wonderfully graceful sloping roofs of Chinese architecture which have since undergone little if any modification, and are said to be reminiscent of the tents of a people who lived in the open, and who had not yet developed a community life in cities. The magnificent bronzes in the museum of the Forbidden City in Peking were conceived and made a little later, during the Chou dynasty, and have

[1] de Zwann, J. P. Kleinweg, *Nat. Verh. v. d. Hollandsche Maatsc. d. Wetensch. t. Haarlem, derde Vers.*, 1917, p. 7.

[2] Cowdry, E. V., *Anat. Record*, 1921, 22, p. 1.

[3] Cordier, Henri, *Histoire Générale de la Chine* (Paris). Quoted from Russell, *Problem of China*.

[4] Hirth, F., *China and the Roman Orient* (Leipzig and Shanghai, 1885).

[5] Mervin, Samuel, *Silk* (New York: Bobbs-Merrill, 1923).

served as models for generations of artists who have reproduced their graceful proportions with slavish accuracy. Perusal of Osborn's book on *Men of the Old Stone Age*,[1] illustrates how faithfully advances in art reflect the progress of civilisation.

Whatever their ultimate origin, it is conceded that the Chinese[2] have lived in the valley of the Yellow River for thousands of years and have there developed many of their temperamental characteristics. It is to be noted, in this discussion, that we are using the designation "Chinese" advisedly, in its narrower sense. It is not intended to include the Manchus, Mongols, Turki, and Tibetans, who find recognition in the five-barred flag of the Republic, nor the Miao, the Lolos, the Shans, and other small tribes which inhabit the more inaccessible parts of the country. The Chinese themselves have from early times been a relatively homogeneous race[3] feeling quite sufficient unto themselves and shut off by great geographical barriers from the rest of the world, except to the north, where they built the "great wall." They have pursued a policy of isolation, not of intercourse with other nations, all of whom they have regarded as barbarians and altogether beneath their notice.[4] This spirit is illustrated in Chien Lung's reception of the first British ambassador, sent by George III, which reads in part as follows:

"Swaying the wide world, I have but one aim in view, namely, to maintain a perfect governance and to fulfil the duties of the State; strange and costly objects do not interest me. I . . . have no use for your country's manufactures. . . . It behooves you, O King, to respect my sentiments and to display even greater devotion and loyalty in future, so that, by perpetual submission to our Throne, you may secure peace and prosperity for your country hereafter."[5]

Had the Chinese inherited a less secluded land, such as the valley of the Nile or of the Euphrates, the course of their history might have been different. In other words, the immodifiable character which they have shown may be partly

[1] Osborn, H. F., *Men of the Old Stone Age* (New York: Charles Scribner's Sons, 1918).

[2] Willians, E. T., "The Origins of the Chinese," *Am. J. Phys. Anthrop.*, vol. i, 1918. Hrdlicka, Ales, "The Anthropology of Asiatic Peoples," *Anat. Suppl. China Med. J.*, July 1920.

[3] However, according to Shirokogoroff, the Chinese are a complex of four anthropological types (extra volume of the J. North China Branch, Royal Asiatic Society, reviewed in *Nature*, 1924, 113, 367).

[4] The expansion of the Empire during the T'ang dynasty (A.D. 618–907) to the point where it made itself felt as far as the Persian Gulf was due not so much to military aggression on the part of the Chinese, as to the development of trade and commerce.

[5] Quoted from Bertrand Russell (*Problem of China*). Original translation is given by Backhouse and Bland in *Annals of the Court of Peking*.

due to the lack of insistent outside stimuli for change rather than to any intrinsic racial attribute.

The Chinese have not been blessed with large lakes, inland seas, or island dependencies, which would foster navigation and lead them directly into conflict with the forces of nature, although they are capable of making good sailors, as was shown at a later date by the achievement of a famous eunuch Admiral during the reigns of the Ming Emperors, Yung Loh and Hsuan Te, in sailing " in command of a fleet of armed junks to India, Ceylon, and Arabia, down the African coast to Madagascar, and up the Red Sea as far as Jiddah, the port of Mecca." [1] During the period which we have in mind they were more or less completely shut off from the sea by the ' wild tribes.' " Much of the land in which they first settled (or from several strains became amalgamated into an independent nation) was a large and fertile plain which enjoyed a climate of a very unusual continental type. In few parts of the world is the succession of seasons more regular, the orderliness of nature more manifest. Winter is followed by spring, spring by summer, with surprising regularity. Under these conditions there may have developed (as in India) an idea of the futility of human initiative, that the good times and the good seasons would come inevitably and of their own accord, that it is not good to try to improve upon nature. In the old days, before the disappearance of the forests, many of the tributaries of the Yellow River must have been streams of crystal clearness. It must have been a wonderful land, one flowing with milk and honey, one in which nature was so bounteous that comparatively little effort was needed to secure, not only the necessities, but also the luxuries of life, and one in which ample time was given for contemplation—an environment which would lead to a peculiar placid tolerance, a tolerance of religion, of everything, except initiative, tending to disturb conditions already so well adjusted.[2]

In surroundings such as these it would be but natural to assume that the people generally were willing and content to adopt a policy of non-interference and to drift with the tide (particularly if the activity of the three endocrines which we have mentioned was even slightly depressed) ; but we are not obliged to make this postulate concerning the whole race. If we try to turn back the pages of recorded history elsewhere we find that great philosophic and religious ideas have not always originated in the minds of the masses, although their

[1] Quoted from Juliet Bredon.

[2] Perhaps the clearest and most searching glimpses into Chinese character are given by Mr. Somerset Maugham in his essays *On the Chinese Screen* (New York: George H. Doran, Co., 1922), particularly No. 38, " The Philosopher."

maintenance and propagation may be conditioned by the receptivity of the people as a whole, or by the efforts of a small band of disciples. It only requires a single kindling spark in the imagination of a great leader. It was certainly so with Gautama Buddha, and also to some extent with Mohammed, and with the Prophet of Nazareth. In ancient Cathay we know that many philosophic ideas were advanced, but it seems clear that the stage was set for the germination of only one, and that of a particular kind. Is it any wonder that the teaching ascribed to Lao Tzu and his great disciple Chaung-Tze calling for a policy of non-interference and stressing the fixity of natural laws was hearkened to? Listen to him in the eloquent words of Giles's [1] translation:

"Leave all things to take their own course, and do not interfere.

"Practise inaction; occupy yourself with doing nothing.

"Keep the mouth shut, close the gateways of sense, and as long as you live you will have no trouble."

At the same time he breathed a gospel of kindliness and of self-effacement:

"He who exalts himself does not rise high.

"He who overcomes others is strong, but he who overcomes himself is mightier still.

"Good words shall gain you honour in the market-place, but good deeds shall gain you friends among men.

"To the good I would be good; to the not-good I would also be good, in order to make them good.

"Requite injury with kindliness.

"What makes a kingdom great is its being like a down-flowing river . . . or like the female throughout the world, who, by quiescence, always overcomes the male. And quiescence is a form of humility."

In a languorous atmosphere of this kind, steeped in the fatalism of the East and reinforced by the influence of Buddhism, it is not surprising that discoveries leading in some measure to a control of natural forces should have been received with disarming apathy. In science, as well as in philosophy, the credit for many advances must be given to far-sighted leaders usually gifted with strong personalities, although the labours of thousands of more modest individuals may by no means be ignored, for without them much of the essential groundwork could not have been built. From among the immortals in Western science let us consider Darwin and Pasteur. Darwin was spurred on by the feeling that his theory of evolution would revolutionise human thought, and by the knowledge

[1] Giles, Lionel, *The Sayings of Lao Tzu*, "The Wisdom of the East Series" (London: John Murray, 1917).

that the eyes of the intellectual world were upon him. In China, as we picture it, it is doubtful whether his views would have elicited any reaction whatsoever in the minds of his contemporaries. Certainly the wind would have been taken out of the sails of his eloquent exponent, Huxley, if none could be induced to listen. Brilliant minds might well be sapped of their initiative, unnoticed in an unresisting atmosphere of complete neglect. In contrast, we cannot but conclude that the resistance encountered in the West was in itself highly invigorating. Expressed in other words, public opinion was, in the East, a narcotic ; in the West, a stimulant. Pasteur revealed to our senses a world of microscopic life, and his researches acted as an entering wedge destined to remove much of the mystery attached to disease. For reasons chiefly economic, but also humanitarian, his investigations were followed with eager expectancy by the world at large. He lived to triumph in a way which would be incredible for a scientist in China. In the careers of many other leading investigators, the economic factor has been an important one. I owe to Professor Overstreet, of the College of the City of New York the valuable suggestion that scientific advances were seldom made effective in China, but instead were consigned to oblivion, or were employed without foresight and purely empirically, for the reason that there were no immediate and pressing demands for their use by the population as a whole. It may thus have been to some extent a question of supply and demand. It is fundamentally this background, upon which Chinese medicine is built, with which we are concerned, instead of with the intricate details of the medical cult.

In the countries bordering the Mediterranean different races were split up into comparatively small groups by mountain ranges and by arid deserts. Their geographical localisation was as beneficial as that of China was inhibitive. They were spread along a littoral circling a great inland sea which led to the development of navigation destined ultimately to make possible the discovery of the New World. This circumstance also facilitated, from very early times, the spread of new ideas conceived under diverse conditions. The people differed from one another in language, in customs, and in religion, and there existed between them an intense competition. A less tolerant atmosphere was created at a later date by Christianity and by Mohammedism. The eastern border of the Mediterranean possessed special strategic advantages in the sense that it was the point of intersection of commerce flowing from three directions, from Africa, Central Asia, and lastly from Europe. To adopt a contemplative, *laissez-faire* policy and to let matters take their own course would

have been suicidal. Instead there developed a tendency to face things as they are, a love of realism which in art was most helpful to the growth of the medical sciences. Slowly, among the Greeks, there evolved ideas of racial hygiene of real importance to humanity. With the gradual increase of international trade, it was accordingly, in the West, a utilitarian world, in which discoveries were launched and were employed for the advancement of individuals and of nations.

In his study of *Greek Biology and its Relation to the Rise of Modern Biology*, Singer [1] remarks that "if necessity is the mother of invention, experience is her father," and that these two, rather than Greek letters and Greek philosophy, were the real begetters of the new experimental method. In China we have an example of a nation that needs the blessings obtainable by the experimental method more perhaps than any other race on earth. She has experienced disease in all its ghastliness for thousands of years, yet she has learnt almost nothing. In her case, therefore, it is competition which, in the years to come, may win the laurels of the parenthood of invention. It is competition in a national sense welding the country into a unit which will "move mountains"; the internal, domestic struggle for existence is keen and remorseless and might be progressive were it not vitiated by a horror of innovations. Perhaps this hesitancy to countenance any change likely to force a readjustment in living conditions, which we have already touched upon, may in some way be linked with the strange persistence through thousands of years of a primitive patriarchal form of society in which veneration for the habits and deeds of bygone ancestors is a strong guiding principle. Lafcadio Hearn's [2] estimate of the influence of ancestor-worship in Japan is almost equally applicable to China, where the practice originated. According to this inspired observer, "not only government, but almost everything in Japanese society, derives directly or indirectly from this ancestor-cult and that in all matters the dead, rather than the living, have been the rulers of the nation and the shapers of its destinies."

Returning to our main thesis, the point of view herein expressed in regard to some of the influences which may underlie the divergence which we have emphasised between art and medicine in China may now be summarised. But, since it is manifestly beyond our power to envisage the problem in its entirety, the following suggestions are merely tentative:

[1] Singer, Charles, *Studies in the History and Method of Science*, vol. i (Oxford: Clarendon Press, 1921).

[2] Lafcadio Hearn, *Japan: An Interpretation* (New York: Grosset and Dunlap, 1904).

(1) That geographic conditions at the time when the Chinese first settled in the valley of the Yellow River will bear careful scrutiny as possible factors in the development of a philosophy of non-interference.

(2) That this attitude became deeply rooted in the national consciousness, not on account of any inherent racial resistance to change, but rather through protection by geographical barriers from insistent outside stimuli for change. That the feeling thus created is well reflected in Chinese art, which is meditative and suggestive, seldom realistic.

(3) That the mental atmosphere evolved was not favourable for the development of the sciences for at least two reasons: First, because potential leaders, instead of receiving inspiration from their fellows, were much more likely to be simply left alone and ignored; it was the opinion of all that they had nothing really worth while to offer (and no one will deny the power of suggestion); the eyes of the nation were set in an entirely different direction. Secondly, because discoveries, once made, were at best only half-heartedly adopted, since there was no crying need for their utilisation either for humanitarian purposes in medicine or in order that the race might survive and compete for the mastery with virile and domineering neighbours pressing upon it from all sides.

IV. Outlook for the Development of Chinese Medicine and Culture

In this discussion we have been compelled to mention certain aspects of Chinese culture, which are not progressive, which may even be inhibitive, in order to offer, in an elementary way, an explanation of why the medical and allied sciences have failed to develop as they have done in the West. There is every reason to believe that the Chinese race has produced in the past, and will continue to produce in the future, men endowed with most brilliant intellects, in no sense inferior to our leaders in the West and perhaps even their masters. But it is China's misfortune that these men have almost without exception devoted themselves to philosophy and to letters, striving, as Welch [1] has emphasised, to attain truth "by the use of the human intellect alone." For reasons which we have tried to outline, they have not felt impelled to test out their theories by observation and by experiment. But the Chinese have also had great men in science, only they lived and died hundreds, nay thousands, of years ago. It is to the memory of leaders, like the mythical Yellow Emperor, Huang-ti, that the Chinese burn incense with unfailing regularity in these

[1] Welch, W. H., *Collected Papers and Addresses*, 1920, vol. iii, p. 175.

days of the twentieth century.[1] As a result, the citizens of this great Republic are to this day pitiably sick and struggle through life with 10 or 20 per cent. of the energy which is their rightful heritage. We have seen what a China riddled with pestilence and famine and isolated by vast oceans and lofty mountains from human intercourse can do, but no man can predict the potentialities of a rejuvenated China, in touch with all the world by modern means of communication, restless with abundant vitality and armed with a knowledge of the sciences.

The outlook for China is certainly not a gloomy one. Indeed, it is the fashion nowadays to harp upon the imperfections of our own social and economic organisation and to intimate that we must approach the culture of the Orient with humility, as novices ready and eager to learn. No one will claim for any nation a monopoly of virtues. Reaction against our missionary policy is increasing,[2] and perhaps rightly so. The cheerfulness of the Chinese, their patience and their constitutional tendency to settle disputes by some form of arbitration, rather than by brute force, are qualities which we may properly emulate. Among them the peacemaker is considered blessed. They have a keen appreciation of the beautiful, though it does not always coincide with ours.

An asset which may be made of real value to themselves and indirectly to other nations is the high respect which they have for scholars. Their national heroes are not generals, nor admirals, nor yet great administrators, but humble teachers and philosophers. All classes share in their reverence for learning, and it has been so for centuries. When a forceful Government is established—and none doubt that it will come sooner or later—we may witness a twentieth-century renaissance, provided that the people throughout the Republic retain this age-long respect for learning and take it as a matter of course that the needful appropriations are made—that failure to do so would be repudiating the convictions of their ancestors—and what is equally, if not more important, if men of the calibre of those who formerly devoted their lives to the classics, and considered themselves privileged so to do, will strive to enter the schools and universities, and, if successful, will be able thus to excite the admiration and envy of their less favoured countrymen. The Chinese cherish their past and are conservative as to changes, so that we may venture to hope that the more far-sighted of their leaders will labour toward this ideal and will

[1] Cowdry, E. V., "Office of Imperial Physicians, Peking," *J. Am. Med. Assn.*, 1921, vol. lxxvii, p. 307.

[2] Lowe Chuan-hwa, "The Christian Peril in China," *The Nation*, 1923, vol. cxvi, p. 144.

thereby be successful in preventing the onward march of industrialism (which is in itself beneficial and progressive) from creating, as it tends to do in the West, the feeling that teachers and professors are to be regarded with a kind of tolerant sympathy as compared with really "practical" people who invent a new kind of chewing-gum or a patent bottle-opener, thereby taking unto themselves merit in the form of dollars.

At present China stands almost alone in possessing this respect for learning, in rating the profession of the scholar the most desirable of all. What gives educationists grave concern is that Germany seems to be losing it. Unless it is regained with the stabilisation of internal conditions, she will be greatly handicapped in regaining her pre-war eminence in science. Japan is still fortunate in its possession, the Emperor himself encourages learning, and the professors, who in the Imperial universities are officials of the *cokunin* class,[1] appointed directly by him, are everywhere treated with due respect as intellectual leaders whose high duty it is to shape the ideals of the rising generations, who, in turn, alone can be responsible for the safety of the Empire.

[1] Cowdry, E. V., "Anatomy in Japan," *Anat. Rec.*, 1920, vol. xviii, p. 67.

POPULAR SCIENCE

THE ENCOURAGEMENT OF DISCOVERY

A PROCONNARY

By Sir RONALD ROSS

Statesman. We agree, then, that perhaps the most important faculty of all living things is this faculty for successful research—that is, for *discovery.* There are first the age-long efforts of plants and animals to discover suitable environments, or to adapt themselves to new environments. Then probably we reach stages of more conscious efforts, as disclosed in the nests of bees, wasps, ants, birds, and so on; and lastly we arrive at the discoveries of men—their huts, houses, and palaces, their weapons, their clothing, their herds, their vehicles, their cooking and medicines, their methods of communication and record, their arts, their education, their governments; and, finally, their inquiries into the mechanism of all things, from the stones to themselves and the stars. From the roots of the seedling seeking nourishment in the soil to the genius of Newton desiring to solve the secrets of the heavens, we find only research, research, research, and—sometimes—discovery. For you, you seek the laws of matter, force, life, and disease. For us politicians, we seek the laws which shall endow humanity with the highest possible welfare, and—alas!—the arguments which may induce humanity to accept such laws. Civilisation is the sum of a vast number of discoveries and inventions which are not known to savages or to animals.

Scientist. How would you define discovery and invention respectively?

Stat. Discovery is the acquisition of new knowledge and invention that of new process. They are complementary of each other. Art is the discovery and invention of new beauty, and science the cognition of all this domain. . . . I fear that these definitions are insecure, but they are the best which I can form at the moment.

Scien. Yes, words are very diffluent things. The gist of

the matter is that discovery and invention are at once the most important and the rarest of human works. They affect not only the present but all nations and all future times. The primitive men who discovered or invented weapons, houses, clothes, crops, fire, boats, and wheels lifted the race from the jungle and put all of us under an obligation to them from their own times until to-day, by making us dominant over the powers of earth, water, and air.

Stat. And now we come to relativity, and to wireless and aeroplanes! But surely such works can scarcely be called rare.

Scien. Not perhaps in the perspective of time, nor so much at the present, which is a moment of great discoveries and inventions. But looking through the ages we shall perceive whole tracts of time which were quite sterile, when nothing new was done, when no advances were made, when the goat-herd fed his flocks among the ruins of Athens and Rome. Behind them again there were the great deserts of the past, which probably saw not a single advance of any kind; and we have China and India to-day. The truth is that the vast masses of men are totally unconcerned with discovery and invention. They live for their own food, for their families, their pleasures, their wealth, and their ambitions. Even their religions are personal bargains with God. They wage barren wars, destroy libraries, temples, palaces, cities, and sometimes whole civilisations, in the interests not of humanity but of themselves or of their superstitions or ambitions or hatreds or pride. Their demagogues, politicians, prophets, prelates, patriots, and kings are too often of the same order—if insincere, labouring not for humanity but for themselves, or, if sincere, often wholly mistaken. Such men interrupt the course of true progress, disendow the future of the past, sink whole nations either in slavery or in sloth, and

> Add not a corngrain to the goodman's store,
> A word to wisdom, nor a stave to song.

I can find little consolation in what is called history—mostly a record of futile storms and earthquakes, of destruction, and devolution. True history is that of discovery and invention.

Stat. Are all the soldiers, politicians, and kings evil, then?

Scien. By no means. I said "too often." There have been and are millions of good men who do their best for their fellows in whatever station of life they find themselves. They are the salt of the earth; but their work is by its nature generally local and temporary. The well-ordered estate, the local hospital, the cleanly town, and the prosperous nation are the results of their labours; but alas! the conqueror, the fanatic, the demagogue, or merely the cold hand of time, may destroy

all these things. My contention is that discoveries and inventions are still greater benefits because they are given not to one time and people, but to the world and for all times. Pericles conferred prosperity on Athens for years, but her philosophers, poets, and sculptors have elevated men for centuries. We can classify all human efforts under three headings, and their negatives—namely self-service, state-service, and world-service. All animals and plants exhibit the first, and bees and ants the second also ; but it is only men who rise to the third—and that very slowly. We can imagine that our primeval ancestors lived only for the first. Then the vast ages of commencing and advancing civilisation have added the second class ; and now lastly—now that we have measured and mapped the world—more of us are turning to the third class of effort. May it prosper ! Discovery and invention are the spear-points of the advance.

Stat. We humble politicians work, then, only in the second class—or, more often, I fear, in the first class.

Scien. No, sir. The politician works in the first and second classes, it is true ; but you statesmen work in the second and third classes. That is the difference.

Stat. Thanks for the compliment, which I hope is deserved. But I have heard it argued that all the discoveries and inventions in the world have not advanced the happiness of men one jot.

Scien. Those who say so should return to the jungles without clothes, weapons, or habitations ! That an invention is largely used proves that it is useful.

Stat. I have also heard it argued that science does but give men more perfect weapons wherewith to destroy each other.

Scien. It is not the fault of science that men misuse it. The most valuable medicines can also be used as poisons, and one might as well maintain that all medicines should be abandoned because criminals sometimes employ them.

Stat. After all, the old hand-to-hand battles were far more fatal than those of to-day. When you forced your enemy down you were generally obliged to finish him off, lest he should get up again. It is said that at Chevy Chase nearly all the combatants were killed, and that at Agincourt few of the defeated army survived ; but in the recent war the mortality was, I have read, seldom more than 5 *per centum* on both sides. But a stronger argument against science is that it requires factories and that factories create slums.

Scien. Do not blame science for that—surely it is your office as a scientific statesman to prevent slums.

Stat. Not always so easy. But admitting that discovery

and invention are among the most beneficent things, what would you do to advance them?

Scien. What would you do, sir, to advance anything?

Stat. I should have to spend money on it, I fear. This country is now doing so on a large scale—for science. There are the universities with their laboratories, the observatories, and also numerous special, technical, commercial, or manufacturing institutions, many of which receive considerable public grants. Then there are the grants given by the Industrial Research Council and the Medical Research Council who dispense, say, £200,000 a year of public money between them. Also, I understand, several Government departments and many hospitals have laboratories. The total cost must be considerable.

Scien. But this money is given for teaching and research, not for discovery.

Stat. Surely teaching and research lead to discovery.

Scien. Do they? Only sometimes. Teaching and research are parts of the process which occasionally yields discovery. You give large sums for the process, but nothing for the result!

Stat. All these subsidised institutions and laboratories mentioned by me are daily producing discoveries which are, I understand, copiously recorded in the large scientific press.

Scien. Hum! I hope so. After all, they are doing their best. I have forgotten which of the American humorists it was who when he lectured "out west" used to post up the notice, "Please do not shoot at the lecturer—he is doing his best."

Stat. You mean that the results are poor.

Scien. Put it in this way. Suppose that a nation decided to encourage the arts by establishing a great institution with numbers of heavily endowed professorships, lectureships, and scholarships for the purpose of producing the most magnificent poetry or painting or music that the world has ever heard or seen: do you think that this institution would create many Dantes, Shakespeares, Raffaels, or Mozarts?

Stat. Probably not one. But it might help your geniuses to ripen, if nature or fortune were to provide any.

Scien. Precisely. The institution would teach teachers, not create creators. A wit has said, I believe, that "Those who can—do; those who can't—teach." There is some truth in that. What you are doing to-day is to spend great sums on the ancillary services of Discovery, but none on Discovery herself. You pamper her servants but allow the mistress to starve in the garret.

Stat. We cannot create creators. All we can do is to help them when they arrive.

Scien. That is precisely my contention. Up to the present the world has helped them very little—in fact it has often starved, persecuted, or punished them. Socrates was poisoned; Pythagoras and Plato were driven into exile; Copernicus was threatened; Galileo was imprisoned; Bruno and Servetus were burnt. Many others were allowed to die in extreme poverty. The Spaniards starved Cervantes "in order to make him work." Camoens and many others died paupers. Tycho Brahe was forced to leave his great observatory and to stop his work. Many were persecuted all their lives by jealous rivals; and many others had their work successfully pirated. Both Harvey and Jenner suffered from thieves; and almost all the greatest men were subjected to virulent criticism or to personal abuse. Columbus was sent home in chains from the continent discovered by him; and even Lister, the gentlest of men, was violently attacked. Very few indeed have ever received the smallest reward. Jenner, who was one of the few exceptions, was nevertheless so embittered by attacks upon him that he said to the Tsar in London in 1814, "I have received the thanks and the applause, but not the gratitude, of the world." In 1805 he remarked that his labours had been "less appreciated, perhaps, in this island than in any other part of the civilised world"; and even to-day his memory is vilified by cranks, mostly English. It has always seemed to me that the tragedy of discovery was most admirably pictured in the legend of Prometheus. He was probably one of the earliest men of science—who discovered how to get fire from flints and was then duly immolated by the chief of his tribe for his presumption! Another case is the great one of which we hear so often in church on Sundays; but I fear men learn so slowly that they still continue to crucify their benefactors.

Stat. Scarcely to-day.

Scien. Yes—metaphorically, if not actually. I know several cases. There is an intense, if subconscious, jealousy of intellectual achievement, especially among those who think that they possess the intellect, but who have never achieved the achievement—the Pharisees and Sadducees of the day. Swift summed up the subject in his terrible but true picture of the Struldbrugs, the human immortals whom their countrymen kept in a state of beggary. Nearly all the immortals of the arts and sciences—the men who really gave us civilisation—were Struldbrugs! Not only is it true that, as Sir Henry Taylor said, "The world knows nothing of its greatest men," but too often the world has persecuted them. One is tempted to maintain that the story of civilisation is a story of neglect and often of crucifixions. For example, the names of scarcely any of our really great men are perpetuated in our aristocracy,

who are descended mostly from traders and politicians. We are so dull a nation that we omitted to preserve any records even of our greatest poet; so that people are actually able to argue to-day that Francis Bacon or others wrote his works!

Stat. This was neglect, not persecution.

Scien. I am drawing a general but rapid picture. At least, scarcely any of the world's greatest men received any rewards or recognition during their lifetime.

Stat. Yes, world-service is mostly unpaid; state-service receives a few pence; but self-service wins the millions and the titles. Really the mass-mind, as we may call it, is still only in the simian stage. It does not recognise benefactors. It bites the hand that helps it. It kills the goose that lays the golden eggs. Men crown those who destroy them and punish those who save them—compare Napoleon and Christ. They hear the tragedy every Sunday in their churches and then repeat it all the weekdays outside, but still call themselves Christians. How can we stop this kind of thing? It seems to be ingrained in human nature. Even we humble politicians are sometimes punished for our virtues! But we were talking of the sciences rather than the arts.

Scien. They are in the same case.

Stat. Scarcely, I should say. The really great men of the arts are extremely rare—probably less than a hundred in every generation in all the arts throughout the world. I think that they must have most exceptional qualities to achieve their exceptional positions. But there must be many hundreds, if not thousands of men of science now living in this country alone; and I doubt whether even a man of science like yourself will maintain that all of these possess any unique genius for science.

Scien. Not only do I not maintain it, but I deny it. You spoke first of "the really great men of the arts" in particular and then of "men of science" in general, and argued that the former are much more rare than the latter. So they are: but you were comparing different classes of aggregates. If to the "great men of the arts" you add all the teachers of the arts, the art-critics, the literary and dramatic critics, the innumerable *expounders* of the arts, you will probably obtain a much larger number than that of all the men of science put together, including teachers, expounders, critics, and successful and unsuccessful investigators. My point is that it is only the successful *investigators* in the sciences who are to be compared with the successful *masters* in the arts. Both in the sciences and in the arts they are the supreme men: they are extremely rare in both cases; they are the "discoverers" and the only discoverers: the others, the teachers,

the expounders, the critics, the textbook compilers, are necessary and sometimes distinguished, but they are ancillary. Every great scientific discovery, like every great artist, is followed by a host of interpreters, tutors, commentators, students of detail, and recorders, who consolidate the positions won in the advance. My principal argument is that we are now spending large sums of money to encourage the ancillary services of science (and the camp-followers), but do nothing at all for the generals who win the victories!

Stat. Surely any man of science who has made important discoveries will easily obtain well-paid professorships at the schools and universities. You laugh. Well, on thinking over it, I suppose not—at least in Britain. There are such things as "local influence," "first claims," and so on—certain little backwater eddies. Then important discoveries must be usually made by men who are too old to commence academic careers afterwards, or who may be unwilling to give up research for teaching.

Scien. We must distinguish here. Certain branches of science—astronomy, physics, chemistry, physiology—now absolutely require subsidised laboratories. In these, investigation can be very conveniently combined with teaching, and has in fact been largely carried out by teachers. But in other branches of science—philosophy, pure mathematics, and much geology, botany, zoology, and practical medicine and hygiene—the leading investigations have been done, and are being done, quite apart from teaching, from universities, schools, institutions, and even laboratories, by private enthusiasts who sacrifice their energies, their time, and often their money in such pursuits, and who seldom hold or obtain academical posts, or indeed any rewards worth mention in comparison with the value of their work for mankind. In all cases, academical posts are of trifling value compared with the value of the services rendered to the world by those who achieve great discovery; and they are apt to interfere with future work by the imposition of duties which are of trifling importance compared with real research by really competent men. In other words, the world is at present largely wasting its best material. I want it to avoid such waste.

Stat. Are there not the Nobel Prizes, which are, I understand, considerable sums given only to "discoverers" in certain branches of effort?

Scien. Yes, by one comparatively small and poor nation! Fancy Sweden being called upon to pay for discoveries made by all the other nations, including the British, American, French, and German! It is very good of Sweden, but I should advise the Swedes to keep their Nobel Prizes for their own

countrymen until other nations begin to take some share in this obvious duty of paying for benefits received by humanity. One entire Nobel Prize (£7,000–8,000) does not equal one year's salary of the British Lord Chancellor, two years' salary of many judges and secretaries of state, ten years' salary of most professorships, or the annual income of thousands of country gentlemen, business men, shopkeepers, and of some lawyers and doctors. The whole annual disbursement for the five Nobel Prizes does not equal £40,000—which is less than the income of many rich individuals.

Stat. I gather, then, that you recommend some form of pecuniary payment for discovery; but I have heard it stated that men of science, at least, scorn payment for their work.

Scien. And I have heard that not one of them has ever refused it, if offered! Apparently such pretences are advanced by those who have never done anything to justify such payment. Is there any conceivable reason why the highest form of effort should be penalised by being refused payment for work done? In all other fields we pay for benefits received——

Stat. Excuse me, we have only recently paid members of Parliament!

Scien. True; but until recently members of Parliament were rich men who were glad, not only to do without salary, but to bear large election expenses; they have prospects of ministerial emoluments; and, in this country at least, receive much more honour than is ever given for discovery.

Stat. I could think of much good work being done now along many lines of work, without payment.

Scien. Certainly; but not *life*-work. There was once upon a time a king, who, having a promising young painter in his dominions, begged him to decorate the walls of his new palace. The painter, flattered by the request, gladly undertook the labour, but, relying upon the king's generosity, made no previous agreement regarding payment. When the work was splendidly finished, after many years, the king refused to pay the painter anything at all. He was quite within his rights; but what do you think of the case?

Stat. I should call the king a mean old villain, and the painter a fool.

Scien. Yes; and this is precisely the case of the British Public to-day on the one hand, and of many men of science on the other.

Stat. Why, then, do so many of them continue to do such work?

Scien. Pardon me, very few do. Some dabble for a time in "research"; others stroll along familiar academical paths, for which they receive academical salaries; and others again

are recouped by professional practice or preferment. But those who wander forth into the wilderness in order to discover entirely new lands or to die in the attempt are few indeed. Men have their families to support, and know only too well the habits of King Public. Not many of them will, like Bernard Palissey the potter, burn their furniture and starve their children and themselves in order to achieve their inventions at any cost. Yet, to be frank, it is chiefly men like him who have built up our civilisation.

Stat. It has recently been argued that every discovery inevitably follows previous ones, like terms in a mathematical series.

Scien. The History of Science laughs at such absurdities. Certainly many main propositions are followed by many easy corollaries which can be picked up even by scholastics; but who establishes the main propositions? Not the scholastics! Archimedes nearly discovered the Calculus, but nineteen centuries elapsed before Newton actually did so. It is extremely unlikely that vaccination against smallpox would have been discovered even to-day, but for Jenner's work 130 years ago; but both these great discoveries opened the flood-gates for the torrents of corollaries which followed.

Stat. Your theory is, I take it, that every "discoverer" is to the mass of other men in the same generation what the "germ-cell" is to the mass of "body-cells" in the individual—designed for the future, not of that individual, but of the species: that the progress of civilisation depends upon "discoveries" as evolution depends upon "germ-cells." But, if so, Nature arranges these matters, and how can we help?

Scien. Very simply—as Nature does—by fostering these germ-cells. To leave the analogy—which is not quite exact—your problem is to foster and help in every possible way the few men who have shown themselves capable of advancing science and art. They are really the supreme assets of the race.

Stat. You are going to suggest some form of pecuniary reward for "discovery," as we call it. But this is often won automatically. Take the medical profession, for example, which is full of rich knights and baronets.

Scien. Almost entirely the successful clinicians; the stone-cutters and the bricklayers, not the architects. What do you do for the latter—the designers?

Stat. Hum! Democratic, at least! But we are already paying largely for medical research.

Scien. The country pays, say, fifteen million pounds a year to its clinicians, not counting hospitals and dispensaries; but only about £180,000 a year for medical research. That is, every person in England, Scotland, and Wales pays

something like ten shillings a year for the treatment of his maladies, but only about one penny a year for investigation as to how those maladies are caused and may be prevented or cured. Supreme wisdom, that! The House of Commons alone receives much more. Yet during the last eighty years the "expectation of life" has been increased in this country by no less than twenty years, chiefly, I believe, owing to the labours of "discoverers"!

Stat. Small gratitude we show them for it.

Scien. No, sir—none at all. You really get nearly all the work done for you by poor men for nothing.

Stat. The only just and reasonable course is for every civilised nation to establish a Public Service Fund to pay honestly for benefits actually received. I suppose that for this country an annual rate of a farthing a head would suffice. The difficulty would be to exclude pretenders and charlatans; but this difficulty could be overcome by proper organisation, just as it is now overcome in the case of public honours. But we already have the Civil List Pensions.

Scien. Mere doles for the deserving poor, given by a minister on petition—not payments by the State for services rendered.

Stat. Well, then, a farthing rate would yield over £40,000 a year—a paltry sum compared with our expenditure on Old Age Pensions and Unemployment—not to mention Education. Would this sum suffice, do you think?

Scien. Certainly, if given only for what we call "discovery." The whole subject was exhaustively examined by the British Science Guild in 1920—here is a copy of its scheme. The Guild recommended a system of small state-pensions of £500 or £1,000 a year each for important but unremunerative discoveries and inventions already achieved—which are only too rare.

Stat. This is a system of payment for results, not for expectations. Personally, when I buy boots or clothes, I alway pays for results, not for expectations! At present the country pays science only for expectations—which are not always realised. Cannot discovery be brought within the Patent Acts?

Scien. No, not without impeding research.

Stat. Do you think that payment for discovery would really encourage it?

Scien. You would not long continue to get your boots or clothes if you did not pay for them.

Stat. Have you any other argument to add?

Scien. Yes—that payment for discoveries is a debt of honour which the world owes to the men who have conferred such great benefits upon it.

Stat. Thank you. That is the strongest argument of all. I conclude that a pound spent in this way will do more good in the world than a hundred pounds spent in any other way; and I will do my best to help you.

Scien. The world should thank you if you succeed. At present it does much for talent but little for genius. Yet

> . . . talent does but reap the crop
> That genius sows and grows;

and history tells us that the reapers are always many—but the sowers, few. The only honest way to encourage discovery is to pay the men who achieve it.

NOTES

Sir Malcolm Watson (R.R.)

On June 25 last Dr. Malcolm Watson received an honorary degree from his university, the University of Glasgow; and on July 10 he was knighted by His Majesty at Buckingham Palace. Seldom have public honours been so fully deserved. They have indeed been long overdue. The work for which Malcolm Watson is now famous has been that of giving practical effect during nearly a quarter of a century to my work on malaria and mosquitoes completed in 1898–9. He was almost the first of all British subjects to recognise that the new knowledge could be utilised for preventing malaria on a large scale. He was then serving in the Federated Malay States at Klang, and set himself at once to mosquito-reduction as suggested by me in 1899 and subsequently. The malaria rate at Klang fell as if by magic, and shortly afterwards a similar miracle occurred at Port Swettenham, where a bad outbreak of malaria was occurring. Since then he has occupied himself in keeping down the disease in a large number of plantations in the States and, with the help of several medical officers and other officials, has overcome difficulty after difficulty. The details will be found in his fine book *The Prevention of Malaria in the Federated Malay States; a Record of Twenty Years' Progress* (John Murray, 1921). The task has been much complicated by several factors. It was found that the measures which reduced the malaria in the "flat land" increased it in the "hilly land," and vice versa—a phenomenon due to the peculiar habits of the malaria-bearing mosquitoes in the two kinds of area respectively. About ten years ago Singapore took up the subject with the help of Watson; and recently he has been temporarily engaged on certain estates in India for the same purpose. The magnitude of Watson's work has hitherto not been appreciated in this country, where most of the malaria workers have been very pessimistic as regards the possibility of reducing mosquitoes anywhere. By sticking to his guns, however, Sir Malcolm has achieved as great a victory in the Federated Malay States as did Gorgas and the Americans at Panama—with this difference, that Watson did not receive the immense advantage of great State subsidies, but was obliged to work his way

forward, step by step, not only against the disease, but against pecuniary difficulties.

Someone has called hygiene the Cinderella of the sciences. Generally it works alone and in obscurity, while other sciences may move more pleasantly in the light of fame. One of the most curious things about the human race is that we take little interest in avoiding disease and prolonging our own lives. Anyone from a politician to a jockey is a greater man in the public eye than he who saves thousands of lives by the modest duties of "Cinderella." We would gladly see some awakening to this fact in the British public. The health of millions is of greater importance than politics or racing.

Another Miracle

Many old miracles have been recorded regarding the cure of the sick and even the bringing of the dead to life, but perhaps none was so wonderful as the miracle recorded in the Annual Report of the Medical Officer of Health for the County of London for 1923. Since 1841 the average expectation of life in London has been prolonged from 34·6 to 53·8 years among males and from 38·3 to 59·1 years among females. This really means that the average length of life has now been prolonged by twenty years, that is by more than a half of what the average length of life used to be in 1840. Many factors have concurred in giving us this wonderful result; but it has been chiefly due to the investigators of disease and to those who study the practical application of sanitary science.

The Severn Barrage

The Government have decided to proceed at once with certain preliminary investigations in order to ascertain the feasibility of the scheme for using the tidal power of the River Severn for the production of electrical power by the erection of a barrage across the river. Lord Parmoor, to whose scientific department (the Department of Scientific and Industrial Research) the work has been entrusted, has already made arrangements to put it in hand.

The Lord President has appointed a Committee, constituted as follows, to supervise and direct the work: G. S. Albright, Esq., C.B.E., J.P. (Chairman); Prof. A. H. Gibson, M.Inst.C.E., M.I.Mech.E., F.R.Ae.S.; G. W. Lamplugh, Esq., F.R.S.; Maurice Wilson, Esq., A.M.Inst.C.E.; Dr. J. S. Flett, O.B.E., LL.D., F.R.S., Director, Geological Survey and Museum.

The feasibility of the Severn scheme turns first and foremost upon the possibility of finding satisfactory foundations for a barrage. Accordingly the first stage of the investigation will involve:

(a) The study by the Geological Survey of the stratigraphical formations in the neighbourhood of the sites suggested for the erection of the barrage;

(b) Taking preliminary soundings with a view to determining the contour of the river-bed at the sites; and

(c) Preliminary measurements of the flow of water at different states of the tide.

Meanwhile, two eminent consulting engineers, Sir Maurice Fitzmaurice, Kt., C.M.G., F.R.S., and Sir John Purser Griffith, Kt., M.A.I., M.Inst.C.E., have been invited by the Lord President to submit a joint report before the end of this year as to the possibility of constructing a barrage on one or more of the sites suggested on the assumption that safe foundations

exist. The data which will be provided as a result of the geological and hydrographical investigations already mentioned will be placed at their disposal. The staff of the Geological Survey have already begun the inquiry entrusted to them, and their report will probably be ready for submission to the Committee before the end of September.

Notes and News

THE following honours were conferred on scientific men on the occasion of the King's birthday: *Order of Merit*, Sir Charles Sherrington; *Baronet*, Sir Humphry Rolleston; *G.B.E.*, Sir Josiah Stamp; *C.B.E.*, Prof. E. P. Cathcart; *Knights*, Mr. P. H. Clutterbuck (Inspector-General of Forests, India), Dr. G. T. Walker (lately Director-General of Observatories, India).

Mr. Henry Balfour, Curator of the Pitt Rivers Museum, Oxford, has been elected a Fellow of the Royal Society in place of the late Dr. T. N. Annandale, who was originally selected by the Council for election to the Society.

Sir Napier Shaw has been elected a Foreign Member of the Royal Swedish Academy of Science.

Prof. J. C. McLennan, of the University of Toronto, has been elected President of the Royal Society of Canada.

Prof. F. G. Donnan succeeds Sir Robert Robertson as President of the Faraday Society.

Mr. J. H. Field has been appointed Director-General of Observatories, India; Professor S. Chapman, Chief Professor of Mathematics at the Imperial College of Science, London; Prof. H. A. Wilson, Professor of Natural Philosophy in the University of Glasgow; Dr. H. Martin Leake, Principal of the Imperial College of Tropical Agriculture; Dr. B. B. Baker, Professor of Mathematics at the Royal Holloway College, Egham.

Among the names of the scientific workers whose death was announced during the past quarter were the following: Dr. C. W. Andrews, palæontologist; Alfred Angot, geophysicist; Dr. G. H. Bailey, chemist; Sir W. H. Bayliss, physiologist; Prof. F. H. Bigelow, meteorologist; Mr. H. Deane, palæobotanist; Sir James Dobbie, chemist; Sir William A. Herdman, zoologist; Prof. J. G. Longbottom, applied mathematician; Mr. K. J. J. Mackenzie, Reader in Agriculture, Cambridge; Prof. T. C. Mendenhall, physicist; Mr. F. Merrifield, entomologist; Prof. E. F. Nichols, physicist; Sir Sydney Russell-Wells, M.P., lately Vice-Chancellor of the University of London; Dr. R. Mullineux Walmsley, Principal of Northampton Polytechnic Institute.

The Commissioners for the 1851 Exhibition have awarded Senior Studentships for 1924 to the following: T. M. Cherry (Melbourne and Cambridge), mathematics; M. Dixon (Cambridge), bio-chemistry; Dr. R. D. Haworth (Manchester), organic chemistry; R. W. Lunt (Liverpool), physical chemistry; G. M. Morant (London), anthropology.

The Ramsay Memorial Fellowship Trustees have made the following awards of new Fellowships for the session 1924–5. A British Fellowship of £300 to Mr. S. W. Saunders for work at University College, London; a Glasgow Fellowship of £300 to Mr. Alex. Robertson, for work in the University of Manchester; a Danish Fellowship of the value of £200 to Mr. K. J. Pedersen, for work in the University of Bristol. The Trustees have renewed the following Fellowships for the same session: Dr. S. Coffey, for work at University College, London; Dr. A. Titley, for work in the University of Oxford; Mr. T. S. Stevens, for work in the University of Oxford; Dr. M. Crespi, for work at University College, London; Dr. J. Kalff, for work in the University of Manchester; Dr. H. Weiss, for work in the Davy Faraday Laboratory, Royal Institution; Dr. Edward Boomer, for work in the University of Cambridge.

The Council of the Senate of the University of Cambridge has issued a

report on the Jacksonian Chair of Natural Philosophy, which has been vacant since the death of Sir James Dewar. It appears that the endowment is too small to provide a full professorial salary, and it is therefore recommended that the occupant of the chair shall not be required to reside in Cambridge, and that he shall be appointed for one year only, but shall be eligible for reappointment for a second year and no longer.

In a reply to a question in the House of Commons, the First Commissioner of Works stated that it has been decided to erect a new building at South Kensington to house the offices of the Geological Survey and the Museum of Practical Geology.

We regret to have to place on record the tragic termination of the Mount Everest expedition this year. The final attempt to reach the summit was made by Messrs. Mallory and Irvine on June 6, when they started from the North Col Camp for Camp V, situated at a height of 25,000 ft. On June 7 they went on to Camp VI (27,000 ft.), and on June 8 were seen for the last time by Mr. Odell in a position whose elevation was afterwards determined by theodolite as 28,227 ft. It is probable that the summit was attained and that the disaster occurred on the return journey. The other members of the expedition have returned safely to England.

The Marine Biological Laboratory at Wood's Hole, Massachusetts, which was founded in 1888, is to be greatly extended as a result of donations totalling about one and a half million dollars from the Rockefeller Foundation, the Carnegie Corporation, and other sources. A new laboratory and library is to be built at a cost of 600,000 dollars.

A paper read at the annual meeting of the National Academy of Sciences (U.S.) last April by Dr. H. D. Curtis, the Director of the Alleghany Observatory, Pittsburgh, contained an account of the measurements of solar spectrum wave-lengths which are being made at that observatory. The method employed is a combination of an interferometer with a grating spectrograph, and it is claimed that the wave-lengths obtained are correct to 1 part in 5 to 8 millions. The so-called Einstein shift of the lines in the solar spectrum towards the infra red amounts to about 1 part in 500,000, so that the Alleghany measurements can detect wave-length variations ten times smaller than those predicted by the relativity theory. The results obtained show that there is a shift in the predicted direction, but that its magnitude depends on the intensity of the line observed. For very faint lines the shift is practically zero, with increasing intensity it becomes measurable, and for lines of intensity 10–15 it is twice that demanded by Einstein's calculations. Thus if the Einstein shift really occurs, there must be another factor in operation which brings the faint lines back to their normal positions. There are various causes which may shift spectral lines towards the red, but no known cause of anything shifting them in the opposite direction except velocity, an explanation which Dr. Curtis considers to be untenable in this case. At the same meeting, however, Dr. St. John, of Mount Wilson Observatory, expressed the opinion that convection currents in the sun's atmosphere are adequate to produce "minor" deviations from the theoretical displacement (it is not clear that he was aware of the results obtained by Dr. Curtis), and the matter must still be regarded as unsettled.

The Report of the Conference held at Birkbeck College last January between the Royal Meteorological Society, the Geographical Society, and the Science Masters Association has been published by Messrs. G. Philip & Son (32, Fleet Street. Price 1*s*. 6*d*. net). Very complete weather data are radiated by wireless for the Eiffel Tower (10.5 a.m., 2,600 metres) and the Air Ministry (8.50 a.m., 9 a.m., 2 p.m., 2.50 p.m., 4,100 metres), using Morse at 12–15 words per minute, so that it is possible for anyone to construct a weather chart and to make his own forecast. Such charts can be prepared in a 45-minute teaching period and, in agricultural areas, the school laboratory

may be made a centre for the rapid distribution of weather forecasts. In addition to a suggestive discussion, the Report contains a valuable paper by Mr. Dobson, of Oxford, giving an account of the advances made in meteorology during the last twenty-five years.

Mr. H. Gilbert-Carter, Director of the Cambridge University Botanic Garden, has compiled a series of 61 *Descriptive Labels for Botanic Gardens* (Cambridge University Press, 1*s.* 6*d.* net) of a most attractive character; for instance:

Family Lauraceæ

CINNAMOMUM CAMPHORA *Nees et Eberm.*

CAMPHOR LAUREL

Native of Formosa, Japan, and China. The name Camphor is applied to several white, odorous, volatile vegetable products having similar properties. This tree is to-day by far the most important source of Camphor, which is obtained by distilling the wood. The Camphor first known to the world was obtained from *Dryobalanops aromatica* Gaertn. (family *Dipterocarpaceæ*). In many parts of the world Camphor is extensively used in medicine and perfumery. The Camphor Laurel is hardy in the western counties of England, and is commonly planted by roadsides in South California.

In the Botanic Garden at Cambridge these labels are printed in Old Style Antique type on green waterproof paper made at the Willesden Paper Mills, and, to protect the ink from the weather, the labels are painted with a syrupy solution of celluloid in either amyl acetate or acetic acid. A cheaper label can be prepared by typewriting or writing in Indian ink on a good white card which is then soaked in melted paraffin wax.

The Rouse Ball Lecture for 1924 was delivered by Prof. H. Lamb, who chose as his subject *The Evolution of Mathematical Physics.* The lecture, which contains a masterly survey of the development of this subject, as distinguished from Dynamics and Astronomy, during the last century, is published at the Cambridge University Press (price 2*s.* net).

The June issue of the *Journal of the British Science Guild* (6 John Street, Adelphi, 1*s.*) contains the very interesting speeches delivered at the annual meeting of the Guild on May 22 last and eleven of the articles previously published in the *Morning Post* and elsewhere under the title *British Science Guild News Service.* These articles seem admirably suited to their purpose, *i.e.* to give to the general public some idea of the fascination and material importance of present-day discoveries and research.

The present year will see the beginning of what should prove to be a great development in forestry training and research, with the establishment at Oxford of an Institute which will be known as the Imperial Forestry Institute, a title adopted at the command of His Majesty the King.

The question of establishing a central training institution was first discussed by the British Empire Forestry Conference in 1920. This Conference felt that, owing to lack of funds and dissipation of effort, training in the higher branches of forestry for the needs of the Empire was nowhere as complete or efficient as was desirable, and therefore recommended the establishment in the United Kingdom of one institution which should undertake the higher training of forest officers and should also be a centre for research into the formation, tending, and protection of forests. An impartial Committee, consisting partly of representatives of Government departments concerned and partly of experts, recommended the establishment at Oxford of a central institution for the higher training of forest officers, for training in research, for the provision of special and "refresher" courses for officers already serving, and for the conduct of research into forest production. The

report made it clear that there was no intention of interfering with the work done by the various University Schools of Forestry, and provided the training of these was maintained at a required standard, selected students from any such schools would be eligible for admission to the central institution. These recommendations were strongly supported by the British Empire Forestry Conference in Canada and by the Imperial Economic Conference held in London in 1923. Arrangements have therefore been completed for starting the Institute in October 1924. Temporary accommodation has been arranged, but funds have yet to be obtained for the purchase of a site and the erection of suitable buildings.

Elaborate preparations have been made for the centenary celebration of the founding of the Franklin Institute, Philadelphia, in September 1924. The first scientific address is to be given by Sir Ernest Rutherford, while further addresses will be given by Sir W. H. Bragg and Prof. W. L. Bragg, Prof. E. G. Coker, Prof. Donnan, Sir Charles Parsons, and Prof. Townsend, in addition to those given by representative American men of science.

The History of Science Society, which was founded in the United States last January, has already 450 members. Its object is the encouragement of the study of the history of science and the publication of the journal, *Isis*, edited by Dr. George Sarton. Members receive this journal gratis. The annual subscription to the Society is 5 dollars, which may be sent to F. E. Brasch, the Treasurer of the Society, at the Department of Terrestrial Magnetism, Carnegie Institution, Washington, D.C.

The Second Annual Report of the Director of the Institute of Science and Industry of the Commonwealth of Australia shows that the work of the Institute is being most seriously handicapped by the absurdly inadequate grants made to it by the Commonwealth Parliament. The first steps towards the formation of the Institute were taken in 1916, and by the middle of 1917 the temporary Advisory Council appointed to prepare the ground for the permanent Institute had completed its work. At the request of the Government this Council continued its labours for a further period of three years, although without laboratories, research staff, or apparatus of its own. In spite of this handicap, much valuable work was carried out, as has been recorded in these Notes. The Act to establish the permanent Institute was passed in 1920, the Director was appointed in March 1921, and it was understood that the necessary staff and resources would be provided to permit the statutory functions of the Institute to be carried out—functions modelled somewhat on the lines of those of the English Department of Scientific and Industrial Research. However, in 1922–3 the grant was only £20,907 and in 1923–4 £21,356. These figures, too, include sums allotted to certain specific purposes, *e.g.* the prickly pear investigation and the Pan-Pacific Science Congress, so that the net totals were only £13,900 and £12,856 respectively. Since the administrative expenses amounted to £6,000 in each year, the funds available for research were practically all absorbed in the continuation of the research initiated by the temporary Council, and no new investigations have been started. It is satisfactory to note, however, that certain parasitic insects and fungus diseases have been discovered which attack the prickly pear, and provided these are as successful in the field as in the laboratory, complete control of this pest is expected. A successful method for the elimination of "blackspot" has been devised, and the investigations on pottery, paper-pulp, the blow-fly, and tannin are making satisfactory progress. Testimonials and expressions of gratitude from manufacturers and others who have been helped by the Information Bureau of the Institute form a novel, and perhaps rather undignified, feature of the Report. Their presence is doubtless explained by the stern fight the Director is making for financial support a little less unworthy of the great Commonwealth which the Institute could serve so well.

We have received a copy of the *Reports on Research at the National Bakery School*, London, from the Secretary of the National Association of Master Bakers, Confectioners, and Caterers (89 Kingsway, London, W.C.). The work, which has been carried out by Dr. C. Dorée and Mr. J. Kirkland at the Borough Polytechnic Institute, deals with such subjects as the loss in weight of loaves in baking, the weight-conserving effects of wrapping loaves in waxed paper, etc., and the results obtained should be of very great practical value to those engaged in the trade.

The Department of Commerce of the U.S. Bureau of Standards, Washington, has forwarded another batch of interesting and helpful papers. *Technologic Paper No.* 254 deals with the emissivity of paints and emphasises the low emissivity for heat radiation of aluminium and bronze paints. The heat radiated from a surface painted with any non-metallic paint (*i.e.* one which does not contain flakes of metal) is, within a few per cent., the same as that emitted from a dull black surface at the same temperature; so that it is quite immaterial, for example, whether a house radiator is painted black or with a white enamel. The radiation from an aluminium-painted surface is, however, only a quarter to a fifth of that from a black surface. A radiator covered with this paint will thus dissipate from 15 to 20 per cent. less heat than it would covered with any other substance. The difference is not great, because some 70 per cent. of the heat from a radiator is carried away by convection currents. In other cases a much greater effect is obtained, *e.g.* painting the *inside* surface of a tent, a motor-car hood, or a shed roof with aluminium paint will cut down the heat radiated inwards to a fifth of its previous magnitude. The best arrangement in such cases would be to have the outside surface as white as possible (*e.g.* clean white duck or white lead paint) and the inside coated with the aluminium paint. The aluminium may be painted on the outside, indeed it would be better there if only one of the surfaces is to be covered and the unpainted outside be a bad reflector. Thus painting the outside of a black "artificial leather" car hood reduced the inside radiation to 25 per cent. of its previous value, while painting the inside (only) reduced the radiation to 40 per cent.

Technologic Paper 253 contains a report on the *Standardisation of Hosiery Box Dimensions* by a physicist on the Bureau staff and a Research Fellow appointed to carry out investigations for the National Association of Hosiery and Underwear Manufacturers. From this report it would appear that every maker at present packs his goods in boxes of a size selected by himself—a proceeding which results (1) in confusion on the retailer's shelves, (2) in damage during transit, since the hose usually leave empty spaces at the corners or sides of the boxes. The standardised boxes designed at the Bureau overcome these troubles and save 6 per cent. by weight of the box materials—a figure which becomes more imposing when it is realised that the total weight of the boxes manufactured each year for the hosiery trade is about 165 million pounds.

Technologic Paper 247 describes in some detail an electrical telemeter devised to measure and record strains in parts of a machine which is in actual use, *e.g.* in airship stay cables during flight or bridge members during passage of a train. The working of the telemeter is based on the change in the electrical resistance of a pile of carbon plates held firmly between end-pieces fixed to the test object. The many practical difficulties which are met with in the design of an instrument working on this principle have been overcome with much ingenuity and, with the apparatus described, using an oscillograph in place of a galvanometer in the Wheatstone's bridge circuit, mechanical vibrations up to 840 cycles per second can be recorded photographically with an accuracy of about 5 per cent.

In *Scientific Paper* No. 482 Dr. Heyl describes experiments to determine whether the gravitational pull on a non-isotropic crystal varies with the

position of its axes relative to the earth. The crystals, which included examples of each of the five non-isotropic systems, varied in weight from 375 to over 1,300 gm., and the accuracy attained in the weighings was as great as 1 part in a thousand million. No change in weight with orientation of the crystal could be detected. Poynting and Gray had obtained a similar result using a quartz crystal, but their experimental method permitted an accuracy of 1 part in 16,000 only. It is interesting to note that the fact that the attraction of gravitation is independent of the nature of the attracted substance has now been established with an accuracy of 6 parts in a thousand million for water, copper, platinum, magnalium, copper sulphate, asbestos, and talc. (Eötvös, Pekar, and Fekete, *Ann. der Physik*, **68**, 1, pp. 11–66, 1922.)

One of the most absorbing books of travel recently published is *With my Wife across Africa*, by Colonel J. C. B. Statham, C.M.G., C.B.E., R.A.M.C. (ret.) (Simpkin, Marshall, Hamilton, Kent & Co., London. Price 12*s.* 6*d.*, with 3 maps, 55 illustrations, and 323 pages). The journey started from Mossamedes on the south-west coast of Africa, which was left by train to Lubango on May 14, 1923. Thence the travellers travelled "by canoe and caravan," down rivers or across deserts, until they finally reached the Zambesi near the Victoria Falls at Livingstone. The incidents are always vividly described and illustrated, and are frequently not a little exciting, while the journeys across the bush and the Kalahari Desert nearly ended several times in disaster. Not only is Colonel Statham a fine hunter, but his newly married wife (daughter of Mr. McQuisten, M.P.) is to be highly complimented on her indomitable pluck throughout the "trek." At one time they came into contact with a morose if not mad white man, who nearly caused a tragedy. The book contains many notes of scientific interest, especially in the four appendices (on the people, the game, the physiography, and the flora respectively).

Mr. A. A. Campbell Swinton, F.R.S., delivered a very interesting and amusing lecture at the Royal Society of Arts on March 12 on some of his personal recollections of notable scientific men, with photographs of many of them. It is full of points of note, especially regarding the work of David Hughes, who almost, if not quite, discovered wireless before Lodge and Marconi—a curious genius who was also one of the original inventors of type-printing telegraphs. The lecture is an addition to the neglected subject of scientific biographies.

On May 27 last the British Science Guild interviewed the President of the Board of Education on the subject of the increase of bureaucratic intervention in education. It cannot be said that much progress was made, and, as usual at such meetings, both sides seem to have retained their original opinions.

The Pickett-Thomson Research Laboratory at St. Paul's Hospital, Endell Street, London, W.C.2, has issued the first number of its Annals—a fine quarto volume of 216 pages of large print, two coloured plates, and many photographs, costing 25*s.* The editor is Dr. D. Thomson, O.B.E., who is Director of the Laboratory; and he contributes five out of the nine copious articles, especially three papers on the etiology of variola-vaccinia, measles, and scarlet fever, in which he reopens the search for the causative agents of these diseases—a vastly important matter which has been unduly neglected of late. There are also papers on Thomson's "detoxicated vaccines" and the classification of cocci by photography.

CORRESPONDENCE

To the Editor of SCIENCE PROGRESS

DR. KAMMERER IN CAMBRIDGE

From JOHN R. BAKER, B.A.

DEAR SIR,—On reading Prof. MacBride's letter of January 22, 1924, published in your April issue, I find I have the distinction of having reached (in his eyes) a "climax of absurdity" in my paper on intersexual pigs, read at the Liverpool meeting of the British Association.

Prof. MacBride says: "The climax of absurdity was reached at the Liverpool meeting of the British Association when one Zoologist, describing the occurrence of 'intersexes' amongst pigs in one of the Pacific Islands, explained their occurrence by assuming the existence of a 'factor for intersexuality.'"

I do not understand how anyone acquainted with the word "factor" could criticise my use of it in my paper. For the sake of those who are ignorant of its significance (and there is at least one among your correspondents), perhaps I may be allowed to make the following remarks.

There are observed differences between individuals of a species. In any particular case, some of the differences may depend on environmental differences, some on germinal differences. It is further found that in the majority of cases the germinal differences behave as independent unit-differences. To the physical basis of such germinal differences the term "factor" has always been applied. Nothing is implied as to the mode of origin of the differences, but a term for their existence is necessary.

In my paper I brought forward some evidence that there are germinal differences among the pigs of the New Hebrides, causing some to be intersexual, some not; and I gave some account of the mode of inheritance of the peculiarity.

There are two alternatives. Either there is a physical basis for the peculiarity, or there is no physical basis for it. I leave the second alternative to the mystic; if the first is accepted, then the term "factor" is that which, by common consent, is to be applied to the germinal difference between normal and intersexual pigs.

Prof. MacBride's letter gives the impression that I thought the peculiarity was "explained" when I had shown it to be due to a factor (or factors). Those to whom I had the honour to read my paper may remember that the greater part of it was concerned with the physiology of the development of mammalian intersexes, along the lines laid down by Dr. Crew in his important paper on the subject.

I am continuing my research on the mode of inheritance of intersexuality, and when I publish my results I will be so bold as to use the word "factor" in the sense in which it has been used in textbooks for the last twenty years, even though my action in so doing may be regarded by some as the "climax of absurdity."

Yours faithfully,
JOHN R. BAKER.

UNIVERSITY MUSEUM, OXFORD.
May 26, 1924.

TUBERCULOSIS AS AN ERADICATOR OF THE UNFIT

From CRANSTON WALKER, M.D., B.Sc.

DEAR SIR,—In the January number of SCIENCE PROGRESS, Prof. Sir Flinders Petrie published a very interesting modern instance of his Revolutions of Civilisation. The article will cause many furiously to think, and perhaps so speak. I would humbly question one small item. Sir Flinders Petrie says: "To make the world fool-proof, is to encourage the growth of fools" (p. 445). Or at any rate not to discourage. But would that matter? In a fool-proof world, foolishness is without significance. That a man is unfit for one kind of world is no necessary handicap to him in another. Indeed, there are many who are remarkably unfit for the Kingdom of Heaven, but who flourish provokingly in this world. It is almost notorious.

Similarly, and this is of more practical importance, a weakness in tuberculosis-resistance would be no drawback in a world free from tubercle-bacilli. Sir Flinders Petrie likens tuberculosis and other contagious diseases to a weed eradicator and considers every step taken against them a loss to the future (pp. 444–5). Let us consider tuberculosis from this point of view. It weeds out. Certainly. What does it weed? The tubercular. Who are they? They are those who show obviously some scathe in the battle which all fight against the tubercle-bacilli. The weeder removes a number of these before their normal time.

Some persons may suffer more than others in the tubercular battle; the reasons may be various, conceivably as follows:

1. From massive infection.
2. From specially virulent infection.
3. From removable handicaps, such as lack of sufficient food, warmth, air, light, etc.
4. From some peculiarity of physiological make-up which diminishes defence.

Such a peculiarity is believed by many tuberculosis experts to be present in a number of cases. It is believed in with vastly greater credence by many sociologists. This defence-defect may be hereditary, to some extent and in some manner; many sociologists assume its inheritance to be as certain as that of hands and feet. It is not necessarily irremediable.

With persons liable to suffer under headings 1, 2, and 3, it would not seem reasonable to eradicate the persons. The question arises concerning those with the defence-defect; they are often designated "the unfit," rather inconsiderately. But unfit for what? Only relatively unfit to meet tubercle-bacilli in personal combat. I submit that there is no evidence that this defence-defect is necessarily, constantly, or even more frequently than chance would allow, accompanied by any other defect or unfitness whatever. On the contrary, among the tubercular are to be found in greater proportion than elsewhere, persons beautiful to look upon, and, even more abundantly, persons of optimistic temperament—both, I suggest, attributes worthy of much encouragement. Nor is tuberculosis incompatible with services to mankind, even services of very high and special kinds.

It is true that tuberculosis imparts no immunity to foolishness; and foolishness, as every doctor knows, gives great assistance to the eradication-process, such as it is. But tuberculosis has no appreciable effect on the eradication of fools. There are so very many non-tubercular.

But supposing that those deficient in defence ought to be weeded out and destroyed, what kind of a weeder does tuberculosis make? A good weed-killer should be easy of application; should kill all weeds, quickly and cheaply; and should neither kill nor injure the other plants. Tuberculosis is of irregular application. It does not kill all the weeds, but only a small

proportion; others it merely injures, as by loss of limb, other crippling, by years of enforced idleness, or by diminished health. It is not rapid; it seldom kills in a few months; it spreads the process over years, often decennials. It is therefore not cheap, since the prolonged burden of the partially eradicated is, for many reasons, shouldered by the community. It does not kill at a convenient time; it mostly kills when the community has invested a good deal of capital in the subject and has only received a little in return; commonly it does not kill early enough to prevent propagation of the stock. Further, though this may be unimportant, one would like a human-weed eradicator to kill with the minimum of pain; tuberculosis might be worse, on the physical side at any rate, but the Society for the Prevention of Cruelty to Animals would hardly approve it as a humane killer.

Nor is tuberculosis innocuous to those who cannot be considered weeds. Evidence is lacking that all tuberculous persons had originally the defence-defect; and there is much evidence that normal persons may succumb to the massive and perhaps virulent infections to which they are exposed in the neighbourhood of the semi-eradicated. Also, being human, the normals in these regions are apt to suffer mentally.

The tubercle-bacilli are thoroughly bad eradicators. They are thoroughly bad in every aspect. They are utterly damned. Let us endeavour to bring them to complete destruction without misgiving.

Yours faithfully,
CRANSTON WALKER.

June 5, 1924.

ESSAYS

FIVE YEARS OF RELATIVITY: AN ABSTRACT (David Landsborough Thomson, M.A., B.Sc.)

It is now just five years since the theory of Relativity began to attract general attention. Familiar to physicists and mathematicians since 1905, Professor Einstein's greatest work was little known outside these sciences, until it received the unequivocal support of detailed observations made by a British expedition during an eclipse of the sun on May 29, 1919.

Since that date, a notable milestone in the march of science, the theory of Relativity has commanded attention and admiration in ever-widening circles. No scientific advance, since the time of Darwin's *Origin of Species*, has aroused such general interest. No doubt this is partly due to the manner in which Einstein's reasoning has triumphed alike over insidious false assumptions at the very root of scientific thought, and over such stubborn problems as that of "curved space," which no imagination can ever truly picture. Even greater is the importance of the general philosophic results of this abstrusely mathematical hypothesis.

A theory, to justify its existence, must explain known facts better than previous theories. The view was formerly held that unending space was filled with an impalpable medium called the "ether," through which the planets and the stars in their courses swam without setting up any displacement or "swirl." Waves of light and electricity were waves which travelled in the ether, somewhat as sound-waves travel in air. The famous experiment of Fizeau showed that light-waves made to pass through flowing water were not directly helped nor hindered by the current: the light was said to travel in the stationary ether. But when Michelson and Morley carried out their celebrated experiment, which was designed to contrast the path of light-waves in the "stationary ether" with the path of the earth in its onward motion through space, they obtained only negative results: the rush of the earth through the ether did not show itself. A similar result awaited Trouton and Noble, who unsuccessfully sought to show that an electrically-charged condenser, carried through space by the moving earth, would act as a "convection current" of electricity. An explanation of the first of these results was offered by Lorentz, who suggested that all objects moving through the ether became shorter in the direction in which they moved; and he set up a series of equations by means of which this shortening could be calculated. Einstein's first great success lay in deriving these same equations from much more firmly rooted and fundamental considerations, which form his Special Theory of Relativity.

To illustrate this theory Einstein and others have devised many fictitious experiments, to which another may perhaps be added. Let us suppose that a ship is riding at anchor, with its stern towards us, and that the captain on the bridge causes lights to be shown at the bow and the stern at what appears to him to be the same instant. But to us, if our eyes were quick enough to see it, the signal from the bow would appear later than the other, since the light has farther to travel by the whole length of the ship. This

already gives us a hint of the way in which the speed of light requires consideration in all such measurements. Einstein also insists on the point that we are quite as correct in calling one signal a little late, as the captain in supposing the two to be simultaneous ; it all depends on the point of view. Moreover, if the ship now starts to steam away, and again to send out light-signals from bow and stern, the situation will be slightly changed : for now a minute fraction of the time the light from the bow takes to traverse the length of the ship will be compensated for by the forward movement of the vessel. The difference in time between the two signals will now seem to us to be slightly less than before ; and, as we can readily use this time-difference to find the distance from bow to stern, the ship will appear to us to be shorter than when she was at rest. In general, these equations of Lorentz and Einstein differ from the simpler classical ones in that they do consider the velocity of light, instead of assuming it to be infinitely great. Light of course travels at so great a speed—186,000 miles a second—that the difference between the old and new is hardly ever appreciable.

The Special Theory of Relativity deals not only with lengths but with time. If we imagine a clock rapidly receding from us, it will seem to go slow, since at each second the light-waves by which we read the clock will have farther to come. Wherefore time is no more absolute than length has been found to be. After all, as Bergson has pointed out, all our measurements of time, by clocks, stars, or what not, are in reality measurements of a distance travelled. In a more complex manner Einstein shows that the weight or mass of an object increases if the object be set in motion. This argument can be rearranged to lead to the result that the energy of the motion has a certain weight of its own, which is added to the weight the object shows when at rest. That this is indeed true of every form of energy is the most important result of the Special Theory of Relativity.

But this Special Theory, which concerns itself with uniformly moving objects, is incomplete. To complete a general theory, which should include motions of all descriptions, even the most complex, took Einstein years of labour in the most abstruse mathematics, and led him at last to a wholly new picture of the universe.

Minkowski had already elaborated a picture of the universe, or " world," in which time was the fourth dimension, and throughout which the Special Theory of Relativity could be applied. Minkowski's " world " was of the nature of a four-dimensional Euclidean continuum. An example of an Euclidean continuum in two dimensions is the surface of a flat-topped table—a surface upon which Euclid's propositions can be worked out. We know that if our table be ruled out as a draught-board, we can locate any point on it by means of two numbers: so many squares along, so many squares up. It is easy to imagine a room ruled out in some such way that three numbers—length, breadth, and height—would fix any point in it. Minkowski's " world " of four dimensions might be measured out in some such way.

But Einstein held that the world of four dimensions was a non-Euclidean continuum—a much more complex matter. A non-Euclidean continuum in two dimensions is the surface of a ball, or an egg, or any uneven and hummocky surface. Euclid's propositions cannot be worked out on a rumpled cushion, nor can draught-board squares be flattened out on such a surface. The map-maker's difficulties in representing the convex surface of the world on a flat sheet of paper are well known. But Gauss had long ago found a method of locating any point on any such surface by two numbers, by an imaginary " draught-board " pattern all twisted and askew ; and the great mathematician Riemann showed that Gauss's numbers could be applied to a non-Euclidean continuum of three or more dimensions, impossible as it is to picture such a thing. Einstein's triumph lay in being

able, by means of his General Theory of Relativity, to apply the purely abstract work of Gauss and Riemann to actual problems in the four-dimensional non-Euclidean continuum of time and space which is our universe.

According to Einstein, the presence of any mass in space—for example, the sun—causes a warping or straining of the four dimensions of the surrounding space, just as the two-dimensional surface of a rubber balloon is warped by a weight resting upon it. Certain puzzling irregularities in the path of the planet Mercury were exactly explained on this view, that the planet was making its way through a time and space distorted by the presence of the nearby sun. Moreover, it was to be expected that rays of light from distant stars would be affected by this distortion if they passed near the sun —and this is what was proved so strikingly just five years ago. Pursued to its rational limits, this theory leads us to believe that the distortions of space caused by all the stars and their satellites average themselves out in such a way that the whole universe, though boundless, is of definite size. A circle, though endless, is of definite size; and the surface of a sphere, or an egg, is also without limits. Further than this, imagination cannot carry us, but the Dutch astronomer De Sitter has calculated that the diameter of the endless four-dimensional universe is some five hundred million million million miles; the most distant nebula seen in the heavens is perhaps one-tenth of this distance away.

But the theory of Relativity, which has assumed such importance in the gigantic tasks of measuring the universe, has not been unfruitful in another and very different direction, in the modern science of atoms, the most minute particle of matter. The simplest of these, the atom of hydrogen, consists of a small planet or electron revolving round a central nucleus, or sun. According to the Danish scientist Niels Bohr, the planet electron, when charged with energy, can jump from one path to another—as if the planet Mars were to leap inwards to share the earth's orbit—and at each such jump light of a definite colour is emitted. The total series of the various colours which hydrogen can emit is called its spectrum, and by means of its spectrum hydrogen can be recognised, for example in the light from distant stars. Sommerfeld has shown that the path of the planet electron may be now a circle and now an oval of similar size. But in the oval track the electron moves at varying speeds and therefore, according to the theory of Relativity, with varying weights. These minute variations of weight find expression in the spectrum as very slight changes in the light given out; and these correspond so well with the results calculated by mathematics that the theory of Relativity is said to be proved by this alone. There is, at least, much to be said for a theory which in five short years measures the boundless universe and unlocks the secrets of the atom.

SPIDERS' WEBS (Theodore H. Savory, B.A.)

THE spider's web is unique in all the animal kingdom—unique in several ways, commonly overlooked. It is the only trap, save one, that any animal builds, for only one other creature constructs an object that traps and snares its prey as does the spider's web. One of the remarkable facts of zoology is that no member of any other phylum has ever evolved the same idea, imitated the spider's web, and proceeded with convergent or even parallel evolution.

So efficient a method of obtaining food does a web provide, that a web-spider hardly ever employs a sense save that which the use of the web demands. The sense of touch is accordingly developed to an extraordinary degree of delicacy, until the relation between the spider and its web is the closest possible. The web, in fact, has become an extension of the organs of touch, and enables the spider to feel the arrival of chance visitors or

amorous mates in a wide area. And yet the attention of arachnologists is always devoted to the spider and hardly ever to the web.

The manufacture of this absolutely essential accessory to the spider's life is clearly a vital process, and it is important to realise that it is performed by a series of actions in which intelligence takes no share. An entirely excusable admiration for the beauty of the finished product, the web, must not be allowed to blind us to the fact that it is produced by a series of actions which, although they may contribute to individual experience, can be carried out perfectly on their first performance in the absence of experience. Further, they are adaptive in the strictest sense to the well-being of the individual and to the continuance of the species.

Such typically instinctive actions depend on the inherited structure of the nervous system and not on the inheritance of intelligently acquired habits. The intelligence devoted by a spider to its web is readily seen to be of a primitive order, all intelligent actions being those that demand acquired characteristics in the nervous system, the results of the relation of the organism to its environment.

For example, a spider in the corner of its web is stimulated to rush out and attack an enmeshed insect by the vibrations of the silk threads. It will attack a tuning-fork that is used to shake its web in the same way, for a certain time. On each occasion that it runs out to the fork it is performing an instinctive action, but so soon as it refrains from running out it is exhibiting intelligence. If it were capable of instinctive actions only, it should continue to run out indefinitely as often as the fork shook the web; but in its capacity to learn it shows itself, to that extent, intelligent. In an hour or so it will, however, attack the fork again.

It is, indeed, to be questioned whether the particular instinctive actions involved in web-spinning do make any contribution to individual experience. The educated spider, which refrains, even temporarily, from attacking the fork has profited by experience and shows that it has done so by modifying its behaviour. In common parlance, it is becoming wiser and improving with practice. Thus to profit by individual experience is the only evidence we possess of any inward consciousness of the experience itself.

In the act of spinning a web, a spider shows no sign of profiting by experience from birth to death. It never spins more quickly or more symmetrically or in a series of better and better chosen places. It never improves. Therefore web-spinning may be an unconscious act, an act that makes no contribution to experience and thus affords no data by which the individual spider can profit. It may seem a startling idea to suppose that a spider is all the while unconscious of the complex, co-ordinated actions which it performs in spinning a web, and the incredulous may prefer, perhaps, to find the spider merely unconscious of the connection between spinning a web and subsequently catching a fly. Be that as it may, the fact remains that we have no evidence of consciousness in either case, while we have familiar evidence of unconsciousness. It is a common cause for wonderment that a spider should spin a web in such an absurd place as the inside of a sealed museum-case, or even more absurdly in the small cages from which they have found escape impossible. It is very difficult to interpret such acts if they be supposed to be conscious.

PASSIVE RESISTERS?—SOME ASPECTS OF THE STRUGGLE BETWEEN ANIMALS AND PLANTS (H. Mace, F.E.S.)

In the universal struggle for existence, the most clearly defined is that between animals and plants, because, while the plant is able to obtain all its requirements from the abundant air and the inorganic matter contained

in the soil, the animal must procure its food from the vegetable. A few parasitic plants and some low forms of life which flourish on decaying animal matter are the only exceptions to this rule.

Whether regarded as antagonistic forms of life, or as higher and lower, one deriving its support from the other, the problem to be solved is the same. There must be an exact balance between the capacity of the animal to consume and the power of the vegetable to avoid consumption. In trying to find out how this balance is secured, we are led into intricate by-paths, and find it necessary to study the most trifling organs of animals and plants: in any special direction, as, for example, the form of the reproductive organs of entomophilous plants, there is material for a life-study. In the present paper I am merely trying to indicate the most general means by which plants escape destruction by the animals which subsist upon them.

The fact that they are fixed in one place, and have no organs of sense or feeling, deludes us into the belief that plants play a passive part, while the restless animal, continually seeking its food, seems to be the aggressive party; but facts prove that the apparent passivity of the vegetable masks a deliberate offensive, aiming sometimes at the very life of its enemy.

The most widespread protective measure adopted by plants is the storage of food-material out of reach. This plan serves a dual purpose, for it is equally effective against the rigours of weather. In biennial and perennial plants there is usually a seasonal shedding of leaves and temporary repose of the circulation. In temperate regions, low temperature, and in hot, dry countries, inadequate moisture, are the conditions which render this suspension of functions necessary. On the return of genial conditions, activity and growth recommence with a vigour corresponding to the amount of food-material stored during the previous growing period.

Roots provide interesting examples of the way this storage is carried out. Each has advantages under special conditions. The plant has to face, during the resting period, the danger of having its stored material taken by animals rendered more alert by shortage of the leaves which form their chief support. The simplest and most natural method of storage is that of the turnip, in which an enlargement takes place at the junction of the stem with the root; but anyone who has seen sheep or goats nibbling down turnips till nothing is left but the thin taproot must realise that this is not the most efficient system. Nevertheless, by providing an abundance of easily reached material, the plant to some extent safeguards the more vital root below.

There is a slight variation in such plants as the carrot, which taper gradually to a point, so that as the animal gnaws from above its task becomes increasingly difficult, until it is more to its advantage to find another root than to burrow continually at one.

A more effective form of root storage is found among those plants which are fibrous rooted. Plants with bulbous roots are only effective in deep moist soil, for there is no danger of their moisture supply being out of reach. On dry soils, especially where only periodic rains occur, these deep-seated roots would be unable to respond to surface showers.

The most widespread and persistent of all forms of plant life are the grasses, which comprise one-sixteenth of all known species, while in individuals they outnumber all other forms of plant life. This predominance is due to the direct and simple manner in which they have solved the problem of survival. Instead of storing all their food material in one monstrous packet, they divide it into innumerable portions, so that should an animal attack the root, there is always a probability of some portion surviving. This distribution also enables the storage to be effected without undue penetration of the soil, so that the plant can respond more quickly

to the influence of warmth and moisture. The rapidity with which grass, apparently burnt up completely, becomes green after a few showers is a fact of common observation.

Trees, shrubs, and other plants which maintain a permanent stem, carry only a small proportion of reserve food-material below the ground. Most of it is added to the stem in concentric layers. As each layer is added, the one below loses its vitality and is no longer available as food, while serving as a support. The soft material on the outside would be vulnerable, were it not for the protective skin which forms over it. Against most animals, this is sufficient protection, but there are some specially adapted as gnawers. To guard against these, many trees develop numerous layers of hard, unpalatable bark. The tree is, indeed, an outstanding example of the way in which the vegetable may gain complete ascendancy over the animal. Trees are the longest-lived forms of life. Not only have they succeeded, by forming the tough protective bark, in protecting their food-reserves, but by pushing out their new growth higher and still higher, they place the leaves out of reach of the normal vertebrate animal. The competition which has taken place in this direction is exceedingly interesting to contemplate. The still living giraffe is one terrestrial creature remaining to show how the animal strove to meet the situation by increasing stature, but the immense size which trees attain seems to indicate that animals reached much greater heights than the giraffe before they were beaten. There is, in fact, abundant proof of the former existence of gigantic herbivorous animals, which were probably protagonists in this struggle with the victorious tree. I do not know that it is founded on substantial facts, but I believe this simple habit of trees may account for other animal developments besides increase of stature. Watching domestic fowls jumping up to reach the leaves, one is led to wonder whether this is not a stage in retrogression from flight, which was passed in progress to it.

A common means of protection is the development of thorns and prickles. There is a fundamental difference between the two which is worth notice. The thorn is a hardened growing point of the plant, directly connected with the inner layers of the stem, while the prickle is a modified hair growing on the skin. The thorn is more specially identified with young growth, and in many plants, such as the plum and holly, thorns are produced less freely on the upper portion than the lower. Prickles are intended to protect the stem and leaves after growth has become more advanced, and the gradual evolution of the formidable prickles of the rose and bramble may be traced from the simple hairs which, in the beginning, were perhaps sufficient to protect the stems. The number of animals whose mouths are so hard that they can consume thorny and prickly plants is very limited, and the survival of these in deserts where thorns and prickles are the rule is due more to their habit of browsing lightly and on many plants, instead of eating one down to the ground, than to their indifference to the sharp weapons with which the plants are armed.

A more subtle means of protection consists in the formation in the plant of substances which are poisonous to animals. Under this head may be included, not only those which are fatal, but others which produce unpleasant effects, such as vomiting or pain, and those which are merely distasteful to the palate. Sometimes there is nothing to warn the animal of its danger. Even in the abundance of good pasture, cows will sometimes eat greedily of the fatal yew. On the other hand, there are plants which certain animals will not touch, though no harm ensues should they do so. Goats will eat the common stinging nettle, rejected by most mammals, quite readily, but they reject the dead-nettles, hedge stachys, and black horehound when pastured. If these are present in a mixture given to them in the stall, they eat it with the other material, without harm. When they

have an opportunity of selection, the smell of these rank plants appears to be distasteful. This modifies the old belief that animals instinctively avoid what is bad for them. It is clear that the plants most likely to be relished by animals stand most in need of protection, and among such, objectionable smell and taste are the most effectual. Plants such as belladonna and aconite, which are fatal to many animals, are not by any means abundant, so that the development of this property to its full extent carries no special advantage. The effectiveness of this form of protection is well seen in the buttercup, which causes blistering of the mouth and is rarely eaten by cattle. Its abundance under conditions in which scarcely any other plants but grasses and clovers survive is abundant testimony to the value of this provision.

From the protection of self to that of race is only a step, and the principles which govern the storage of reserve food or the protection of vital points operates in the production of seed. Plants of short duration specialise in rapid growth and seed production, wasting no material in storage. As soon as drought threatens, the annual hastens to produce and scatter its seed, and so secures its race, even though itself be consumed in the subsequent scarcity.

The perennial is not so readily able to produce flower and fruit, and so provide for the future of its race, at the expense of its own exhaustion, but its method of securing future generations is none the less studied and guarded. Its flowers are generally produced in great profusion, and at a season when abundance of foliage renders them more or less immune from consumption by the larger animals. Its fruits also are provided with many and varied devices, enabling them to be distributed over a large and varied area of country, affording endless possibilities of settlement in suitable environment. Wings, hooks which cling to the hair of animals and other devices are familiar, but the most interesting is that so widely prevalent among trees and shrubs, which actually turn the appetite of the animal to their own advantage. Countless seeds are surrounded by two coats—the outer a soft, fleshy, appetising pulp, readily consumed by birds and mammals, the inner hard, stony, and resistant even to the acids of animal digestion, preserving the vital germ uninjured, to be cast forth on the ground, perhaps miles from its parent.

Although the struggle between plant and mammal is still active, its most violent phase seems to have passed. The recuperative power of grass enables it to feed an endless number of herbivorous mammals, without harm to itself, while the tree has long since beaten out of the field all its vertebrate antagonists. The strongest enemy the plant now faces belongs to the invertebrate animals, and the insect, by its extraordinary capacity for multiplication, seems at times as though it would annihilate vegetation. In the height of summer foliage a mighty oak may be stripped of every leaf by an army of caterpillars, while an invasion of locusts can, in a few hours, completely destroy thousands of plants. The never-ending fight goes on, the nature of the attack changing as the attacked evolves new means of parrying it. In the long-run, the vegetable must triumph, if the animal also is to survive, for as surely as the former vanished would the other perish with it.

ESSAY-REVIEWS

CANCER: FACT AND FANCY. By ARCHIBALD LEITCH, M.D., being a review of (1) **Cancer Research at the Middlesex Hospital, 1900-1924. Retrospect and Prospect.** Edited by W. SAMPSON HANDLEY, M.S., F.R.C.S. [Pp. x + 90.] (London: Published for the Middlesex Hospital Press by John Murray, 1924. Price 3*s.* 6*d.* net.) (2) **The Elucidation of Cancer:** an article by Dr. LOUIS W. SAMBON, in the *Journal of Tropical Medicine*, June 2, 1924. (3) **Cancer: How it is Caused and how it can be Prevented.** By J. ELLIS BARKER. With an Introduction by SIR W. ARBUTHNOT LANE, Bart., C.B., etc. [Pp. 432.] (London: John Murray, 1924. Price 7*s.* 6*d.* net.)

Cancer Research at the Middlesex Hospital, 1900-1924

FOR the last quarter of a century Middlesex Hospital has had a special laboratory devoted to cancer research. Long before that, indeed since the close of the eighteenth century, it had wards for the reception of cancer cases, so that those who maintain that the study of the disease from the clinical point of view has been neglected or that purely clinical investigations are capable of contributing more to progress than laboratory researches have here an example on which to meditate. Of late years we have not heard much of the activities of the Cancer Research Department: the war put an end to the annual publication of the contributions of its staff; and though the editor of this small book insists on the excellence of the machinery available, we get the impression that, at the present time, it is scarcely functioning at high pressure and that, in comparison with other institutions engaged in cancer research, its output is restricted. It may be a question of finance, which, it is to be hoped, will soon be adjusted, for no laboratory nowadays can hope to attract to its service and retain for any length of time the right type of worker unless it can offer him something approaching a bare sustenance salary. They have not done so in the past, and the critics who roundly assert that cancer research has been sterile and who might fail to recognise in this retrospect of work done at the Middlesex a convincing refutation of their assertions should remember that the staff of this laboratory for the most part has been composed of young men who served it for short periods before passing on to more attractive or lucrative pursuits. Under such circumstances all that we can expect to find will be short or incomplete studies on isolated subjects that may or may not have important bearing on the main problems of cancer. Of course, if a discovery of prime magnitude should be made, it can be developed under such a policy, but Middlesex has been rather unlucky in this respect and seems to have lagged behind in the development of discoveries made elsewhere. Thus the discovery of the transplantability of animal tumours with all its potentialities was not exploited; indeed, such researches were opposed at first, and were only undertaken after the limitations of progress had been sensed by others. Tissue cultivation *in vitro* found no devotee at Middlesex to apply the technique for the elucidation of questions of growth or metabolism in neoplasms. Above all, the experimental production of cancer,

which is the dominant feature in research to-day and which furnishes a direct means of attack on the fundamental problems of etiology, has been left to others.

The clinical and autopsy records of cancer cases extending over a long period of years supplied excellent material for numerous statistical studies and several very useful papers were the result, dealing with age incidence, site frequency, hereditary influences, and routes of dissemination of the common forms. The wealth of material provided from the operating theatre and the post-mortem room was utilised in histological researches of very varied character. Though some of these, probably the majority, are mainly of the descriptive order and others of a controversial nature, yet there are a few of outstanding merit. The cytological observations of Farmer, Moore, and Walker attracted great attention at the time: they had rediscovered the fact that in many malignant tumours the cells departed from the somatic type in having only half of the usual number of chromosomes; to them this reversion to reproductive type furnished the explanation of the limitless proliferation of cancer, for the "unsatisfied affinity" demanded union and reproduction. Bonney was successful in demonstrating that the same phenomenon of reduction of the chromosomes occurred as frequently in tumours of low malignancy, and he clinched the matter by showing that in so-called venereal warts, which never become malignant, the same diminution of chromosomes can be found. The same worker was responsible for some fascinating studies on "precancerous conditions": from histological analyses of early cancers, and of lesions which he believed to be frequent precursors of cancer, he showed that there were profound changes in the connective tissue preceding the irruption of the epithelium. The underlying hypothesis of altered stroma as an essential to cancerous proliferation of epithelium was not new, but Bonney gave it a surer basis. The matter is still disputed. According to other observers, the loss of sub-epithelial elastic felting, the aggregations of plasma cells, and the other characters on which strength is laid, are not constant in early malignant tumours and may occur in lesions which never develop malignancy.

Perhaps the best piece of work emanating from the Middlesex laboratory and certainly the one that has attained most widespread recognition, especially amongst surgeons, is that of Handley, who edits the present booklet. He maintained that cancer, starting in such an organ as the mammary gland, spreads centrifugally by continuous permeation of the lymphatic vessels, and that there is within this increasing ring a reparative fibrosis on the part of the connective tissue which tends to destroy the cancer cells in the lymphatics and may succeed in obliterating all traces of them. With regard to the latter, it may equally be maintained that the fibrosis takes place only after the cancerous cells are dead and does not come into play whilst they are alive; and with regard to the former, that continuous permeation of lymphatics occurs only in directions against the flow of the lymph stream in contradistinction to embolic spread in the direction of flow. No one who has had opportunities of investigating the question will deny that in advanced cases a permeation of the deep lymphatic plexus (in breast cancer) can be demonstrated far down on the abdominal wall and that there is much reason in Handley's operation in removing the deep fascia extensively, but it is still very doubtful if it is more liable to be involved than the chest wall in the immediate vicinity. But, however we may differ from the author in these particulars, we must recognise the value of his work as a whole and welcome his contributions to that much-neglected, but very useful, branch of cancer investigation, surgical pathology. It is a pity that we cannot induce young surgeons in the making to devote their time to such researches, for only in this way can we hope for improvements in the surgical treatment of cancer.

Of the contributions of the Director of the laboratory, Prof. Lazarus-Barlow (who has recently retired), which have been mainly of a biophysical character, it is difficult to assess the value, for his findings have not been assimilated either by physicists or pathologists; but if he succeeds, as I trust he will, in infusing the spirit of investigation and experimental inquiry into those who work with radium and X-rays in the treatment of cancer, he will have performed a great service.

The Elucidation of Cancer

A new epoch in cancer research was inaugurated by Fibiger of Copenhagen when he published over ten years ago his first communication on *Spiroptera carcinoma* of rats. He discovered a large number of rats with cancer of the cardiac end of the stomach, a lesion which had never previously been encountered, although hundreds of thousands of rats had been examined for malignant tumours. In these gastric tumours he found a nematode of a new species to which he gave the name of *Spiroptera neoplastica* (*Gongylonema neoplasticum*). He showed that the relationship between the neoplasm and the worm was not merely an accidental association. The adult worm lived in the epithelium of the fundus of the stomach of the rat and the larval stage in the cockroach *Periplaneta americana*, which had been imported with consignments of sugar from the West Indies. By feeding laboratory rats on infested cockroaches, Fibiger succeeded in producing carcinoma of the rats' stomach. This was the first time that cancer was produced experimentally. Last year Dr. Louis Sambon had an opportunity of becoming acquainted with Fibiger's experiments, but I imagine he has not made himself familiar with the details or with Fibiger's extensive writings. Sambon, at the same time, heard from somebody that cancer was very prevalent in the Italian province of Romagna, and as he had for a long time entertained the idea of a connection between cancer and household pets, and with that end in view had studied rats, mice, cockroaches, mealworms, and bed-bugs in London, he concluded that here was the clue he had sought for, and in Romagna was the material to investigate. He brushes aside as being of no consequence the claims of experimenters to have produced cancer in animals by repeated applications of tar and other substances; in fact, the irritation theory of cancer meets with his condemnation, although Fibiger is one of its most enthusiastic adherents, and although the great success that has attended its experimental exploitation has appealed to those specially engaged in cancer research the world over. Arrived in Romagna, where he spent five weeks in field investigation, he learned from conversations with local medical men that cancer was about three times as common in that district as it was in the rest of Italy, that they believed its incidence to be increasing, that it was unequal in its topographical distribution, and that there were localities and even blocks of houses where aggregations of samples were notorious. A radiographer whom he met had particularly insisted on the prevalence of cancer in the cardiac end of the stomach, and a statistical paper by Testi and Galli showed that the majority of the cancers in Romagna were situated in the alimentary tract or in organs closely associated with it. He learned also of several instances of animals suffering from cancerous disease, dogs, cats, and barn-fowls, being found in close association with human cancer cases. Having inspected some houses in which a few people were reported to have died of cancer, he discovered that these houses and more particularly the meal-arks were invariably infested with *Blatta orientalis* and that rats and mice were everywhere. That, apparently, was all that he discovered; the rest is the merest speculation mixed up with discursive remarks on Roman history, tropical parasitology, climatic conditions, customs, and the

flora and fauna of Italy. The suggestion seems to be that the causal agent of cancer is conveyed by quite a host of different parasites that inhabit a large variety of invertebrates. For example, he states that cockroaches, meal-beetles, meal-moths, cellar-beetles, and woodlice all harbour in their body-cavity or musculature the encysted larval forms of various round and flat worms which spend their adult stage in the alimentary tract of domesticated animals and man, but the most important of these are certain Spiruroidea of the genus *Gongylonema*. Whether he has personally verified this or only suspected it does not emerge from his communication, but, at any rate, the parasites merely act as the carriers of the supposititious cancer germ. In short, his theory, as far as I can learn from his paper, is that in cancer we have to deal with certain cockroaches, meal-beetles, and possibly also dung-chafers which are capable of fostering and disseminating the young forms of Spiruroid worms of the genus *Gongylonema;* the mature *Gongylonema* gains access to the alimentary tract and more particularly the œsophagus and the cardiac end of the stomach; and here "some ubiquitous micro-organism, possibly ultramicroscopic, favoured by the activities of the worm, may invade the tissue cells and give rise to malignant growth." To anyone who knows Fibiger's work and the twenty-years-old hypothesis of Borrel, the origin of Sambon's theory will be evident. In other words, Sambon has served us a dish compounded of uncooked Fibiger and overdone Borrel dressed with speculative sauce *à la maître d'hôtel*. We have long awaited, and in all probability shall never see, the materialisation of the recondite cancer germ, ultramicroscopic or otherwise, but surely Dr. Sambon might have provided for us one solitary little demonstration of a Romagna cancer containing one of these gongylonemata, something definite and concrete on which our minds could dwell. If only he had done that, we could have forgiven him all the rest.

Cancer: How it is Caused and how it can be Prevented

There is probably no subject in the wide range of biology, certainly none in medicine, on which more nonsense has been written than on cancer, and yet no other group of diseases has been more extensively studied and explored. The ancient idea that it was a general rather than a local condition, the common use of the word as a specific and not a generic term, the failure to appreciate it as a tissue reaction quite as distinct and as general as the inflammatory reaction, the myths and cloudy speculations that have accumulated around and obscured the palpable facts, and, above all, the lack of success that, until recently, attended the efforts to produce cancer experimentally in lower animals, combined to make the subject the happy hunting-ground for uninstructed prophets with uninspired doctrines. Armchair speculations seem to possess a fascination not only for the public who are not in a position to learn the facts, but unfortunately also for many medical men, distinguished in their particular walks in life, who ought to know better. Here we have a book on cancer written by a journalist who has apparently devoured, without being able to digest, a great deal of the voluminous literature of cancer. He has been aided and advised by the well-known surgeon, Sir W. Arbuthnot Lane, as well as by others whom he hails as authorities, whose views, good, bad, or indifferent, shine alike to him as scientific truth. They urged him "to take up the investigation of the cancer problem," they informed him "that the inquiries of those engaged in research had yielded hardly any practical results, that the mystery of cancer was as great as it had been in the past, that science knew practically nothing as to how cancer was caused, that possibly an amateur might succeed where professional research had failed. When I inquired more closely into the cancer problem I found that practically all scientists, sur-

geons, and physicians despaired." In this unrelieved atmosphere of gloom he set forth on his quest, dauntless and unarmed—very much unarmed; but he has brought back the golden fleece. Sir W. Arbuthnot Lane is inclined to regard Mr. Barker's book as "easily the most important practical work on cancer existing in English or in any other language," and he begs us to listen attentively to what he has to say. We have listened, and this is what we remember of the discourse. Cancer is a disease of civilisation, unknown amongst primitive races, increasing like wildfire, and caused by the prolonged action of bowel poisons. These bowel poisons result from intestinal stasis caused by vitamin starvation and by the ingestion of faked foods. Lane has been preaching this doctrine for years: Mr. Ellis Barker introduces it to the masses. Each of the principal statements in the book is backed up by quotations, some of them at inordinate length, from the writings of medical men whom he cites as authorities. The thesis that cancer is a disease of civilisation is easily disposed of. Even if we had no positive evidence on the point, we could judge from our own statistics in this country, where doctors are common and the people seek their advice for the most trifling of ailments, how frequently on the average a case of cancer would occur. A medical man in this civilised community encounters less than two cases a year on the average. The less civilised a tribe is the less the opportunity will there be for cancer to be discovered, and any statement by a medical man who sees them occasionally that he has not come across a case of cancer does not prove that its occurrence is less common than in England. We have, however, sufficient evidence of a positive nature to lead us to believe that cancer is no less common in primitive races than in our own civilised country. It was commonly said, for example, that cancer was rare or non-existent in Greenland, and the statement is repeated in this book; but a recent survey by the Danish Government has been published by Fibiger, who shows that it is as common there as in European countries. That cancer occurs in all sorts of animals, mammals, birds, fishes, etc., is sufficient to disprove the idea that its occurrence is essentially connected with civilisation.

The second contention that cancer is increasing at an alarming rate is not borne out by mortality statistics carefully interpreted; indeed, several of the best statisticians who have gone into the question can find no evidence for believing in the actual increase of the disease. It must be remembered that the material on which mortality statistics are founded contains, as far as cancer is concerned, a high percentage of error, and any conclusions drawn from them are to be accepted with great caution.

The main contention, the theory of Arbuthnot Lane as I understand it, that the absorption of products of intestinal decomposition conveyed to various parts of the body induces cancerous proliferation, is pure speculation and rests on no proven bases. The cancers of animals, as far as we can determine, are not allied with any constipated habit nor with the presence of intestinal "kinks," nor is there anyone outside of Lane's entourage who maintains this in the case of the human subject. Even the very indirect evidence that Lane cites as bearing out his ideas is much discounted. Experimental verification of his hypothesis should long ere this have been performed by some of his followers. Personally I have injected into the lower bowel of a large number of animals for periods of nine months several of the products of intestinal putrefaction without inducing any lesion whatever, local or otherwise. Until Lane brings forward something like scientific evidence for his belief, the constant reiteration of his assertions leaves us cold and the popular promulgation of his doctrines may do harm.

But even if we granted all the contentions, we are still unable to see how cancer can be associated intimately with civilised dietary. It is surely peculiar, if modern dietetic customs were so very bad in comparison with

those of primitive races, that the average expectation of life amongst civilised races should be incomparably higher than amongst the uncivilised. Most of us would prefer to enjoy such food as our cooks and restaurants provide us, even at the risk of dying of cancer at seventy, than dine on the unappetising dishes of savages and die at forty.

THE INDIAN CHARACTER. By Sir R. Ross. Being a review of: **India: a Bird's-Eye View**, by The Earl of Ronaldshay. [Pp. xiii + 322, with 24 illustrations.] (London: Constable & Co., 1924. Price 18s. net.)

Lord Ronaldshay was Governor of Bengal for five years after 1916 and has written several books on the East, especially his *Lands of the Thunderbolt*—with which many of our readers are doubtless familiar. I have been much impressed by the work under review. It gives us a rapid but philosophical account of the greatest of our possessions. Of course a work of 314 pages on the subject can be nothing more than pemmican; but pemmican may be good or bad. Lord Ronaldshay's book is not only historical and descriptive, but also analyses many of the principal political problems of the day, and even renders a short but excellent account of the malaria problem in India, with some words on hookworm disease—all quite sound. His three concluding chapters deal with pessimism in India and its causes, and will be found to be of great interest to men of science.

I quite agree that one of the most characteristic points about the Indian in general is his gravity, if not melancholy. When I was in India (Madras) the people always struck me as being physically feeble and mentally more or less depressed. The physique improves in the northern territories, but the indefinable lack of buoyancy remains. One seldom sees open-hearted or boisterous laughter in Indians. When they do laugh they are generally considering some sardonic jest, which is not always of a pleasant nature—the laughter is chiefly *risus canis*. This character prevails throughout the people, among the rich, the poor, and the very poor. Yet the similarity of the Indian type to the Aryan races of Europe is very manifest, and one sees Indians who, but for their clothing, the colour of their skin, and their gravity, might even be Englishmen, Germans, or Frenchmen—which is seldom the case in the inhabitants of North Africa for example. For the true philosopher, that is the scientific philosopher, there is no country which gives more food for thought than India; but Lord Ronaldshay's book makes no mention of that wonderful little work which I am never tired of commending, *The Revolutions of Civilisation*, by Sir Flinders Petrie, in which he advances the theory that the intellectual capacity of nations fluctuates in cycles or tides of about fifteen hundred years each, and that successive cycles follow each other fairly regularly. In every cycle there is a period of ascension, then a zenith, then a period of declension, and lastly a nadir; and he has formulated the hypothesis that the first of these periods generally follows conquest by and admixture with another race—that is, follows a *racial* zygosis. The reason why modern Europeans differ so much from Indians appears to be due to the fact that we are at present probably in the period of zenith while they are in the period of nadir. It is apparent from the literature and the ruins of India that at one time the Indians possessed far greater intellectual and probably physical vigour than they now show. One does not find in them to-day the same qualities of mental and physical activity which one sees in Europe; there is always the same depression, the same desire for rest, some indefinable back-hearkening of spirit which is seldom with us to-day, except perhaps among the more used-up intellectual elements of our universities. I have known many Indian gentlemen whose capacity for languages, for politics, history, sometimes

even for science, is remarkable, but in whom one always notes that judgment and opinion are rather derived than original. They scarcely seem to think for themselves; they always lean on authority. And, without forcing conclusions upon his readers, the author of this book well elucidates this phase in the Indian character. His analysis of Karma among the Hindus is very suggestive. I suppose that not one scientific man in a thousand is ever likely even to approach the acceptance of the Hindu mythologies and their basic modes of thought. To us, for instance, the whole reincarnation hypothesis appears to be of a nature which I will not be so rude as to name; yet more than two hundred millions of people seem to believe in it—more or less. The belief is not always logical; they are not always so anxious to try another stage of existence as might be supposed. In spite of their depression, some of them still take a little joy in life; and physical fear is not an unknown psychological element among them. The whole complex of the Hindu faiths seems to me to be rather a veneer of customary education than an independent growth of personal thought and conviction. Not that the European faiths are very much more, but they are not so adulterated with secondary and often grotesque dogmas. Similar dogmas govern Hindu art—as they did the parallel art of ancient Egypt. In the presence of both, we who have seen the Parthenon experience the discomfort of criticism where we should admire and of laughter where we should worship. All this belongs, we feel, to some divinity long dead, some supremacy long superseded. The face of Europe is set forward, the face of India, backward. Europe seems to possess to-day something like the mentality it possessed in the time of Julius Cæsar: India the mentality of Europe, or at least of Italy, seven hundred years later. There is nothing original in the Indian, and even his noisy politics of to-day are derived. He possesses many virtues, such as long-suffering, gentleness, courtesy; but he does not often possess the supreme virtue of intellectual vigour. One sees the seeds of similar decay in Europe, it is true; but not to the same extent as in India. Possibly in a few centuries we shall become as Italy became after Marcus Aurelius, and India will spring again to the forefront of the nations.

Of course these are only speculations—and possibly somewhat wild ones; but behind them there is a great and constant fact, the rise and fall of nations; and this is a natural phenomenon which requires a natural explanation. Nearly all the great world-work of to-day—science, art, statesmanship—is being done by a few nations which probably number less than a tithe of the world's population and which also possess the greatest military aptitude. Apparently they only produce the germ-cells of progress. It looks at least as if nations, like individuals, have an optimum age of vigour. But such notions are only in the course of formation; and we have not yet sounded the ultimate depths of biology, which may some day reveal possibilities beyond our present ken.

REVIEWS

MATHEMATICS

Cours de Mathématiques générales à l'usage des étudiants en sciences naturelles. Par G. VERRIEST. PREMIÈRE PARTIE: CALCUL DIFFERENTIAL, GÉOMÉTRIE ANALYTIQUE À DEUX DIMENSIONS. [Pp. 337.] (Paris: Gauthier-Villars et Cie, 1923. Price 38 frs.)

THIS type of book is common enough in England but is rarer on the Continent. It aims at familiarising natural science students with the notions of the calculus and at giving them the manipulative skill in differentiating which they will need in their physics and chemistry. It therefore makes no pretence of a rigorous treatment, and propositions are made to appear plausible by means of a geometrical representation. It is clearly written and should be of use to the people for whom it is intended. The English teacher may find some of the physical applications to be novel and of interest; for example, Maclaurin's series (with remainder) is illustrated by a lengthy note on the aberration of spherical mirrors. The author is perhaps too fond of differentials as distinct from derivatives.

F. P. W.

Practical Mathematical Analysis. By HORST VON SANDEN. Translated by H. LEVY. [Pp. xv + 195.] (London: Methuen & Co., 1923. Price 10*s*. 6*d*. net.)

INTEREST in methods of calculation seems to be reviving; we may offer as evidence the recent publication of Whittaker's *Calculus of Observations* and of Runge-König's *Numerisches Rechnen*. Prof. Levy has seized the opportunity to translate a much more elementary book on the subject, Sanden's *Praktische Analysis* (1914). The preface contains an amusing account of the way in which a pure mathematician will carry out a calculation "on odd scraps of paper, tossed about here and there," and certainly such an unmethodical person could not do better than read Sanden's tips on compact arrangement and kindred topics. The book provides a useful summary of methods of approximation, slide-rules, Horner's method, interpolation, Simpson's rule, and so on, with instructions how to tabulate the work in the most economical way. The bibliography at the end is a useful feature.

F. P. W.

Traité élémentaire des nombres de Bernoulli. Par NIELS NIELSON. [Pp. x + 398.] (Paris: Gauthier-Villars et Cie, 1923. Price 50 frs.)

THE numbers of Bernoulli first made their appearance in the *Ars conjectandi* of James Bernoulli, who introduced them in an elementary way and calculated the first five. Later Euler rediscovered them as the essential part of the coefficients which arise in the expression of cot x, tan x, and cosec x as power series in x. But this discovery, according to M. Nielsen, "a été fatale pour le développement de la théorie, parce que sa grande autorité

a poussé les géomètres à appliquer presque exclusivement dans les recherches sur ces nombres rationnels des méthodes transcendantes." M. Nielson has therefore set himself to develop the matter from an elementary point of view, basing the work on what he calls symmetric polynomials, *i.e.* polynomials of degree n which satisfy the functional equation $f(-x-1) = (-)^n f(x)$. He has produced an enormous work, full of recurrence formulæ, complete and incomplete, and curious congruence results, which is extremely interesting to dip into. Here are some of the proceeds of such a dip. Adams, the astronomer, calculated the first 62 of the Bernoulli numbers, but he has been outclassed by M. Serebrennikoff of Petrograd, who is reputed to have got as far as the ninetieth. Von Staudt did not occupy himself solely with projective geometry; he discovered that if B_n be the nth Bernoulli number, then $(-)^n B^n$ is equal to an integer, positive or negative, together with the sum of the reciprocals of the prime numbers p such that $p-1$ divides $2n-1$. Then we have 29 pages on the expression of sums of powers of the first n integers in terms of Bernoulli numbers, with generalisations of the well-known formula $(\Sigma n)^2 = \Sigma n^3$, and the less well-known $2(\Sigma n^2)^2 = \Sigma n^7 + \Sigma n^5$, due to Jacobi; we have properties of the coefficients in the expression of $x(x+1)(x+2) \ldots (x+p-1)$ as a polynomial in x, and the proposition that if $2n+1$ be a prime, then it is a factor of the numerator of

$$1 + \frac{1}{2^2} + \frac{1}{3^2} + \ldots + \frac{1}{n^2}.$$

But we cannot imagine anybody reading steadily through the book, and we do not think that it will rank in importance with the author's German works on the Bessel and Gamma functions.

F. P. W.

Analytical Conics. By D. M. Y. Sommerville, D.Sc. [Pp. vii + 310.] (London: G. Bell & Sons, 1924. Price 15*s*. net.)

Perhaps Salmon is out of date and difficult for students, but we are always a little surprised that anyone should take the trouble to write another book on analytical conics. However, Prof. Sommerville has done it very well indeed and we have nothing but praise for the result. There are several points of originality which deserve notice. In the first place, the conic is defined by the extension of the familiar rectangle property of the circle—the author having regretfully given up the idea of using the sections of the cone on the ground that this would be too great a shock to the convention that demands a separation between plane and solid geometry—and the focus-directrix property is introduced afterwards. Treatment of the parabola is postponed to that of the ellipse and the hyperbola. We are grateful to Prof. W. P. Milne for insisting on the inclusion of chapter xix on "Systems of Points on a Conic." It is interesting stuff and forms a useful introduction to the next chapter, on invariants, which, we are glad to see, is much more thorough than is usual in an elementary textbook. But the treatment of homogeneous co-ordinates is all wrong.

There are numerous examples, well selected and arranged.

F. P. W.

ASTRONOMY

The Vault of Heaven. By Sir Richard Gregory. Second edition, rewritten. [Pp. vii + 202, with 61 illustrations.] (London: Methuen & Co., 1923. Price 6*s*. net.)

This little volume is the second edition of a book first published in 1893, but it has been largely rewritten, and extensive additions have been made. It sets forth in simple and picturesque language those results of astronomical

achievement which require no mathematical or highly technical training to understand them. A special feature lies in the illustrations, most of which are reproductions of celestial photographs taken with first-class instruments.

The science of astronomy must always make a special appeal to the minds of men. Sir Richard Gregory's book is intended for the general reader, and will be greatly appreciated by those who wish to spend some peaceful hours in contemplating the treasures revealed by the astronomer's telescope. The author's well-known power of popular exposition, together with a very judicious selection of illustrations, has produced a well-balanced and readable little volume, which cannot fail to be appreciated by anyone who is not completely devoid of an æsthetic or spiritual sense.

W. M. H. G.

Time Measurement. By L. Bolton, M.A. [Pp. viii + 166, with 8 plates.] (London: G. Bell & Sons. Price 6*s*. net.)

THIS little volume is intended to present in short compass the rudiments of time measurement. It assumes no previous knowledge of the subject, and those who wish to acquire an elementary knowledge of the principles involved in measuring time and of the construction of time-pieces, will find it a good investment. It is naturally not intended to compete with more compendious (and more expensive) treatises, and can scarcely be recommended to anyone who wishes to indulge in a serious study of the subject, but should be appreciated by those whose ambitions are less far-reaching.

The first chapter is of an introductory nature, the next two present some elementary astronomical considerations relative to the subject, and the fourth chapter deals with ancient time-pieces such as the water-clock and also contains some descriptions of sun-dials. Chapters v to ix are devoted to weight- and spring-driven clocks. The treatment of weight-driven clocks is mainly historical, the author being of the opinion that it is wise to use the earliest such clocks of which we have accurate knowledge for the purpose "of illustrating, not of course the refinements, but the broad outlines of modern construction, with the advantage of investing somewhat dry mechanical details with an antiquarian interest."

Chapter x deals with watches and chronometers, and chapter xi with electrically controlled clocks. This chapter contains a brief but excellent description of the pendulum control devised by Cunyngham and Hope Jones. Chapter xii deals principally with the determination of time at the Royal Observatory, Greenwich, and the last chapter is concerned with the subject of the calendar.

Practically no mention is made of precision clocks, and in the opinion of the reviewer a couple of pages devoted to such would have improved the book. At first sight it seems curious that a book which contains a description of a chronometer escapement should not also contain some short account of, say, a Riefler clock escapement; but the author (*vide* page 136, footnote) considers that the details would be beyond the scope of the book.

W. M. H. G.

Réception des Signaux Horaires. [Pp. ix + 226.] (Published by Gauthier-Villars for the Bureau des Longitudes. (Price 27 frs.)

THE object of this volume is to set forth the details of the principal scientific services to which wireless telegraphy has been adapted. The use of wireless telegraphy for the purpose of transmitting time-signals is now familiar to all, and it is only natural to find that two chapters of the present compilation are devoted to various details of these signals, their transmission, re-

ception, and registration. Two subsequent chapters deal with the application of wireless telegraphy to meteorological and seismological purposes, and a preliminary chapter sets forth the technique of wireless reception in general. The five appendices contain a mass of valuable information.

The various scientific wireless services are of such importance that it is very gratifying to record the appearance of a volume which supplies practically any information on the subject that can be desired. For instance, lists of transmitting stations are provided, as are also complete explanations of the various international codes, and various technical instrumental details are expounded carefully and fully. The book should certainly be in the possession of all practical astronomers, meteorologists, and geophysicists. It is to be hoped that it will be found in all libraries of any scientific pretensions, and that it will be read by all who take an interest in scientific progress.

W. M. H. G.

PHYSICS

Relativity: A Systematic Treatment of Einstein's Theory. By J. Rice, M.A., Senior Lecturer in Physics at the University of Liverpool. [Pp. xv + 397.] (London: Longmans, Green & Co., 1923. Price 18*s.* net.)

As explained in the Preface, this book is primarily written for the science undergraduate anxious to learn "what changes this new idea—of Relativity—is producing in the principles and content of physical science." Some chapters, however, and the whole Part III are added for the sake of post-graduates and for a yet wider circle of readers.

The best and most originally written part of the book is the Introduction (pp. 1–30), which gives a very clear and instructive description, in general terms, of the origin and the fundamental concepts and postulates of the theory of relativity, including general relativity, and touching slightly even upon gravitation and the essence of its law as conceived by Einstein.

The mathematical development of the ideas, so ably sketched in this general outline, is carried out in three Parts, viz. Restricted Relativity, General Relativity, and World Geometry. The first of these, to which very wisely as many as seven chapters are devoted (pp. 31–163), gives an excellent exposition of the subject, which of late began to be cut down rather mercilessly by prominent authors anxious to arrive soon at the more sensational, generalised theory. Notwithstanding the many attractivities of the latter, it will be remembered that the restricted relativity has actually more points of contact with the domain of physics proper. The author, therefore, is to be warmly congratulated upon this part of his plan, laying, as it does, a sound foundation for the realistic freshman's further studies. Part II (p. 167–300) will, in spite of a certain clumsiness of mathematical technicalities (especially in chapter ix, on "Geodesics"), be found a good introduction into General Relativity, including the gravitational field-equations, in Einstein's pre-cosmologic form, and the generally covariant electromagnetic equations. This Part gives also to the reader a sufficient instruction in handling general vectors and tensors of any rank. To "Action" a whole chapter is devoted. Yet there is this welcome feature, that, unlike the leading writers, the author is free from the oversanguine expectations of Hilbert as to the heuristic efficiency of this principle. Nor does he follow the authoritative chorus in an undue hypostasy of the four-dimensional line-element. In general the book under review commends itself greatly by a note of sobriety, which seems particularly desirable for the class of readers Mr. Rice has in view.

The last Part, on World Geometry (pp. 303–89), gives a sufficiently complete account of the relativistic developments since 1916, comprising

Einstein's and de Sitter's cosmological speculations, and Weyl's, Eddington's, and Einstein's so-called unification theories, that is, attempts at spelling Gravitation and Electro-magnetism in one breath. Especially, Einstein's recent generalisation, the affine field-theory, providing for such a union but leaving the problem of matter (electrons, protons) unsolved, is treated in an Addendum. These subjects are preceded by an introductory information (chapter xiv) on Riemannian analysis of polydimensional manifolds. This chapter is somewhat marred by two erroneous statements, one concerning such manifolds as that of colours (p. 312), and another (p. 323) likely to induce the reader to believe that every n-fold can be embedded into a homaloidal or flat $(n + 1)$-fold. But these are only minor points, in no way detrimental to the whole work. In general the book is remarkably free from special errors and misprints.

Apart from the usual indices, the Table of Contents gives an unusual and very helpful detailed summary of the matter treated in each chapter.

In conclusion, this well-printed and carefully edited book can be warmly recommended to every student of the subject.

L. SILBERSTEIN.

CHEMISTRY

Chemical Encyclopædia, a Digest of Chemistry and Chemical Industry. By C. T. KINGZETT, F.I.C. Third Edition. [Pp. x + 606.] (London: Baillière, Tindall & Cox, 1924. Price 30*s*. net.)

THE aim of the author has been to prepare "an epitomised digest of chemistry and its industrial applications, in a form which should be useful not only as a work of reference for professional chemists . . . but also as an educational treatise serviceable to all classes of the community." The chief difficulty in compiling such a work is, naturally, the selection of the items to be dealt with, and it may be questioned whether the space devoted to various matters is in all cases commensurate with their importance. The information is at times a little vague, if not actually misleading: co-ordination is defined simply as "a chelate theory of the chemical structure of molecules, or, in other words, the arrangement of atoms in chemical entities," which is hardly helpful to "medical practitioners, pharmacists, merchants, brokers, barristers," etc., especially as no explanation is given of the word "chelate"; phthalic anhydride is stated to be obtained from "naphthalene vapour in association with air, using sulphuric acid in presence of a mercury salt as a catalyst" (there is some confusion here between the old Badische process and the modern catalytic oxidation method); anthraquinone is stated (p. 32) to be used as a dyestuff; the structure of "Rivanol" (p. 178) should be ethoxy-diamino-acridine hydrochloride; metol (p. 359) is not a cresol derivative; the method of preparation of benzidine (p. 57) is incomplete, as the product would be merely hydrazobenzene; it will be news to most chemists that toluene can be prepared by the action of caustic potash upon benzyl alcohol (p. 558), it would have been more useful to have referred to its occurrence in Borneo petroleum. The book would be improved by careful revision, but that it meets a definite demand is indicated by the fact that a third edition has been called for.

F. A. M.

A Dictionary of Applied Chemistry. Volume V: Oxygen—Rye. By SIR EDWARD THORPE, C.B., LL.D., F.R.S. [Pp. viii + 722, illustrated.] (London: Longmans, Green & Co., 1924. Price 60*s*. net.)

ONE by one the familiar blue bindings of the last edition of the *Dictionary* are being ousted from their places in our bookshelves and relegated to the basement or lumber-room, their places being taken by the red-covered

volumes of the new edition, the fifth volume of which has now appeared, so that the editor's long task is steadily drawing to a close, as it is announced that the last two volumes, VI and VII, are in the press.

One item of news will be of general interest, that the final volume is to contain a complete index to the whole work; the present reviewer has long pointed out the desirability and necessity of such an index, as its absence has always been felt to be the weak point in the *Dictionary*, and the editor is to be congratulated on having decided to undertake the task.

The present volume contains many new or revised articles of importance, such as the article on "Photosynthesis," by Profs. Baly and Heilbron, Prof. Wynne's excellent and useful summary of the chemistry of quinoline and related products, and other contributions too numerous to mention individually. The work fully maintains the standard set in the previous volumes and is in every way a worthy and valuable addition to chemical literature.

F. A. M.

The Vegetable Proteins. By T. B. Osborne, PhD., Sc.D. [Pp. xiii + 154, with diagrams.] (Monographs on Bio-chemistry, Longmans, Green & Co., London, 1923. Price 9*s*. net.)

We welcome the appearance of a second edition of Prof. Osborne's monograph. Such a concise and masterly survey of this important branch of biological chemistry must never be allowed to fall out of date. The decade following the publication of the first edition has seen careful progress in this intricate subject; due in the main to the labours and genius of the author himself.

A very thorough revision, necessitating the suppression of some of the original chapters and the inclusion of three or four new ones, has left the book the last word on its subject. We congratulate the author on the inclusion of a most interesting chapter from the pen of L. J. Henderson on the Relation of Proteins to Acids and Bases. The theoretical discussion elaborated deserves the attention alike of the analyst and of the physiologist. An excellent review of the methods of hydrolysing proteins and their values in elucidating the nature and measuring the amounts of the amino-acid constituents of proteins forms the subject-matter of another new chapter; and the importance of such accurate knowledge to the physiologist is brought out in the new section dealing with the nutritive value of the vegetable proteins. The recent work of Chibnall and Schryver and of the author on the proteins of the protoplasm of the green plant cell is dealt with in detail.

A much enlarged and comprehensive bibliography enhances greatly the value of this interesting and now quite up-to-date monograph.

P. E.

Fats: Natural and Synthetic. By W. W. Myddleton, D.Sc., and T. Hedley Barry. [Pp. xii + 182, with diagrams and illustrations.] (London: E. Benn, 1924. Price 25*s*. net.)

The intention of the authors has been to produce a volume which will be interesting and instructive to students desiring to specialise in the chemistry of the oils and fats. It is to be feared, however, that they have fallen between two stools as, on the one hand, the work is rather too split up and lacking in continuity for easy reading, whilst on the other hand it is hardly possible to produce a complete reference book and encyclopædia of the subject within the compass of 175 pages of fairly large type; and it may be doubted whether many students in these hard times will be willing or able to pay 25*s*. for a work of this type.

The book deals first with the natural fats and waxes, then nearly 100

pages are devoted to the study of hydrogenated fats, their production, purification, estimation, and uses, and a final chapter deals with the possibility of the complete synthesis of fats. The volume is clearly printed, and reflects credit on the authors; but the publisher, if one may make the suggestion, by printing on smaller and thinner pages and by publishing at a lower price, would have added attractions to the book. F. A. M.

The Chemical Basis of Growth and Senescence. By T. BRAILSFORD ROBERTSON, Ph.D., D.Sc. Monographs on Experimental Biology. [Pp. viii + 389, with many figures.] (London: J. B. Lippincott & Co., 1923. Price 12*s*. 6*d*. net.)

THE series of monographs to which this book is the latest addition forms the most comprehensive library which we have of a field which American biologists have made particularly their own. Whilst the author of the book under review is no longer in the United States, his argument owes much to their laboratories, and his monograph, therefore, fittingly preserves the continuity of the series.

Attention is first directed to the time relations of growth in the higher animals. These are found to be amenable to expression by a succession of three more or less well-defined "growth cycles," which each imitate the curve for the velocity of an autocatalytic monomolecular reaction. The process of protoplasmic synthesis being transparently a highly complex system of changes, it becomes necessary to assume that this complexity of events is governed by a slow reaction which, as it were, sets the pace of the whole. Prof. Robertson makes the assumption that this "master reaction" is that which controls the velocity of nuclear synthesis. So far the story is straightforward. What is to follow is a little bewildering. As nuclear synthesis proceeds autocatalyst will accumulate within the nucleus up to the moment of division, when, with the disruption of the membrane, some will be freed into the surroundings. The distribution between nucleus and pericellular fluid being a characteristic of the cell, it will follow that the growth rate of subsequent nuclei will be modified by the pre-existent charge of autocatalyst in the environment at the time of the previous division. This is emphasised by the consideration of the master reaction as reversible, for it will follow that, as autocatalyst accumulates in the medium, the velocity of nuclear synthesis will be accelerated or retarded according to the amount retained within the new nuclei at the moment of membrane formation.

The reader must pursue the further argument himself as it ramifies through the phenomena of senescence, differentiation, hyperdifferentiation and cancer, evolution and such specific growth agencies as the vitamins.

The thesis is a dauntless, if naïve, contribution of an imaginative mind to statistical biology. It must be recorded, however, that some of the more fundamental data to the argument have failed to find confirmation in other laboratories.

A great mass of literature, as a bibliography of 500 papers testifies, has been drawn upon, and the author merits our gratitude alike for this labour and for the inspiration of a real attempt at elucidation. Biologists will find much to criticise; they will find as much to interest and stimulate.

R. K. C.

AGRICULTURE

Grassland Farming, Pastures and Leys. By W. J. MALDEN. [Pp. xxiv + 314, with 113 figures.] (London: Ernest Benn, 1924. Price 30*s*. net.)

THE literature dealing with the treatment of grassland is so voluminous and scattered that Mr. Malden has rendered a great service to agriculture by gathering into one volume the more important points that arise in practice.

During the past few decades a steady decline has occurred in arable farming, and some millions of acres have reverted to grass, usually of very indifferent quality. In view of this continued decline and the existing need to wrest the maximum production from the soil, the importance of improving the quality and yield of the grassland is becoming steadily greater. *Grassland Farming* is intended to give practical advice to the practical man, and is written specifically for farmers. Written as the outcome of many years' personal experience, the book fulfils its purpose excellently, providing guidance for the inexperienced and suggestions which may lead even the experienced farmer to improve on his methods to his own financial benefit. Except in a few instances the theories underlying the treatment of grassland are but lightly touched upon, and it is to be regretted that no references to the work of other writers are given for the further aid of students.

The various aspects of the management of grassland are fully dealt with, and frequent emphasis is laid on points that make for success or failure. The author advocates a wider view than that of treating grass with basic slag only, rightly contending that this is insufficient in many cases, and that a judicious application of other fertilisers, including nitrogen on occasion, is desirable. The species present in old pasture are largely determined by the physical and manurial conditions of the soil, and much improvement of herbage can be brought about by treatment calculated to better these conditions. Various methods of draining and the best ways of dealing with such local troubles as anthills, molehills, pans, rushes, and tussocks are also described. Special attention is given to the plants used in the formation of temporary pastures and to the conditions under which each is most suitable, together with the various seeds mixture most adapted for sowing down different types of land. Drilling instead of broadcasting is advocated, as the benefit derived therefrom usually much more than balances the slight extra cost. The varieties of plants found in permanent pastures are discussed, especially in relation to their feeding value, and the possibility of improvement by careful management is duly pointed out. The chapters on haymaking, ensilage, and harvesting seeds of pasture plants sum up much information on practical points, while under the heading of Grazing notice is taken not only of poisonous and objectionable plants occurring thereon, but also of the various diseases that may be contracted by animals fed on infected or unclean land. In connection with the manuring and liming of permanent pastures a series of botanical analyses are given of plots on similar soil treated for two consecutive years with various combinations of manures, thus showing how rapidly and drastically the quantity and quality of the herbage on any particular soil may be modified by suitable treatment. This affords a useful demonstration of the value to the farmer of testing various fertilisers on a small scale before deciding on the general scheme of manuring to adopt on his land.

The book concludes with an appendix of analyses, descriptions, and illustrations drawn chiefly from various commercial sources, and an index to the numerous subdivisions into which the chapters are divided renders the work of reference easy.

W. E. BRENCHLEY.

The Production of Field Crops. By T. B. HUTCHESON and T. K. WOLFE, [Pp. xv + 499, with 145 figures.] (London: McGraw-Hill Publishing Co., 1924. Price 17*s.* 6*d.*)

THIS is a very comprehensive work which has been designed as an introductory course in field crops, the outline being based largely upon the recommendations of the American Society of Agronomy. The authors have very wisely recognised that successful crop culture is dependent upon

a variety of factors, and in consequence the book is properly balanced by reason of the first portion being devoted to the more important underlying fundamentals. This is particularly welcome in a work of this kind, for in recent years there has been a tendency to overlook important matters like the preparation of a good seed-bed, methods of economic tillage, and the selection of good strains, while magnifying the importance of artificial fertilisers. Fortunately these are being increasingly appreciated, and much valuable work has been done on problems arising out of soil management. The authors have drawn heavily upon recent research findings, which adds to the reliability of the book, as well as representing the latest opinions.

In the treatment of the individual crops the authors have endeavoured to cover the more important points in the growth and culture of species which are of agricultural importance. It is not intended to be exhaustive, however, while no attempt has been made to give any crop undue prominence by reason of national importance. In most cases the crops are dealt with in a clear and concise manner, and, as a textbook designed primarily for North American students, it is admirably suited to this purpose. At the same time the world distribution of crops has by no means been neglected, yet the usefulness of the work for this country would have been considerably increased had a few additions been made to the text concerning British varieties of the various crops dealt with. Some of our more important root crops, for example, receive scanty attention, while some of the recommended seeding rates per acre differ from those which obtain in this country. The cereal crops and some of the more widely grown North American forage crops are very satisfactorily treated, and taken throughout it is a work which can be thoroughly recommended. The text is liberally illustrated, and the style and quality of the book maintain the high standard of the McGraw-Hill Agricultural Series.

H. G. R.

ZOOLOGY

Invertebrate Zoology. By H. J. Van Cleave. [Pp. xvi + 259, with frontispiece and 126 figures.] (London: McGraw-Hill Publishing Co., 1924. Price 15*s.* net.)

This book, as the author explains in the preface, is intended as a textbook and reference work for students of invertebrate zoology who have already completed an introductory course in biology. The author expresses the view that such books should not give detailed descriptions of types of the several groups, but should concern themselves with the setting forth of general principles, and with presenting the characteristics of each group as a whole. The book embraces the entire animal kingdom with the exception of the phylum Chordata, but from the small number of pages it will be seen that such a vast field can only be covered by treating each group very superficially; the account of each phylum consists of little more than a catalogue of the principal characteristics of its several subdivisions, preceded by a few pages of more general discussion. Reference is made to the various types of marine pelagic larvæ that are of such importance in understanding the interrelationship of the phyla, but no account is given of their detailed development and organisation, and it is doubtful whether a student would be able to appreciate their significance with such scanty information. The book may find favour among students taking a pass degree in Zoology, but is hardly likely to prove of much service to those reading for an honours degree in that subject in the universities of this country. The book is well got up and the illustrations excellently reproduced.

J. H. W.

The Marine Plankton, with special reference to Investigations made at Port Erin, Isle of Man, during 1907-1914: A Handbook for Students and Amateur Workers. By JAMES JOHNSTONE, D.Sc., ANDREW SCOTT, A.L.S., and HERBERT C. CHADWICK, A.L.S. (Department of Oceanography, University of Liverpool). With an Introduction by Sir WILLIAM A. HERDMAN, F.R.S., Emeritus Professor of Natural History and sometime First Professor of Oceanography in the University of Liverpool. [Pp. xvi + 194.] (Liverpool: at the University Press; London: Hodder & Stoughton. Price 12*s.* 6*d.* net.)

THIS book aims at helping those, both student and amateur, who wish to study the floating life of the sea. It is based on one prolonged piece of research extending over several years, undertaken by Sir William Herdman, who writes the introduction, assisted by the three authors for the purpose of studying the plankton of the bay of Port Erin, Isle of Man, both quantitatively and with a view to ascertaining the value of the existing methods of collecting and estimating the various catches.

With this combination we expect to have a good book, and are not disappointed. A good idea is given of the quality and quantity of the planktonic life in this one particular area, and this is the basis of the whole work.

After a short introduction on the plankton in general a number of clear and well-drawn named figures are given, some of which are very beautiful. Among them we seem to recognise the individual work of each of the three writers. These represent most of the commonest plankton organisms, and a second chapter explains their occurrence and seasonal distribution. To this part are attached tables and graphs showing the frequency of the species for each month during the several years. Afterwards comes a chapter on organic production in the sea, and finally one on the methods for obtaining the specimens.

Owing to lack of discussion and the very small space given to some of the introductory remarks certain statements want qualification, and are slightly misleading. For instance, we are told on page 6 that "several groups of Coelenterates are planktonic throughout their entire life-history; these are . . . the Medusæ." Surely, even if we omit the Hydromedusæ (these being called Medusoid gonophores) we may regard Aurelia as a true medusa! Again, we find it stated many times that the Peridinians feed like typical green plants, and in no case is there any qualification showing that some are holozoic and others probably saprophytic. Indeed, as only the larger Peridinium species are taken into account in the tables it is highly probable that only a few of these contain chlorophyll.

The book will be useful in many ways, serving as an introduction to plankton work, and giving a good idea of the aims of such research; but it covers only a small part of what a student has to face in the serious study of plankton, and one cannot imagine anyone being content with identifying his catches from these figures and not going on further to make out the descriptions of each species from systematic books. We should describe the book as a stimulus to the study of plankton rather than a work of reference, and regarded in this light it is instructive, extremely interesting, and a valuable addition to any marine library. MARIE V. LEBOUR.

An Introduction to Oceanography, with Special Reference to Geography and Geophysics. By JAMES JOHNSTONE, D.Sc. [Pp. xii + 351, with 69 figures.] (Liverpool: at the University Press; London: Hodder & Stoughton. Price 15*s.* net.)

IT is only in recent years that oceanography has taken the place which it deserves as a branch, and a very important branch, of geography in its

widest sense, and a book on such a subject is specially welcomed when it comes from an author who has himself worked at the subject from all standpoints. Dr. Johnstone approaches it mainly from the physical side, but this does not prevent him from showing that the other points of view are equally important, and that mathematics, chemistry, physics, geology, and biology all take a part in the science of oceanography.

It is probably because the biologist has hitherto been to the front in the study of the ocean (for, at any rate, the beginning of the study of the sea had to do to a large extent with the life in it) that the author offers us the geologist's and the geophysicist's theories, together with the chemist's, rather than the biologist's. There is a tremendous fascination in the whole subject, some of it extremely complex and difficult to understand, but here put before the reader with great lucidity. Theories of the origin of the oceans, their permanence, the permanence of continents, together with all physical characters of the sea—these make up the bulk of the work. Also there is much to say on ocean depths and their measurement, the composition of the sea bottom, the oceanic margins, the chemical composition of sea water and its physical characters, whilst a generous portion of the work is given up to a discussion on the origin of the tides, currents, and the oceanic circulation.

The author, although offering two theories of the origin of the earth, apparently accepts neither completely, for neither accounts perfectly satisfactorily for all the oceanographical phenomena. His discussion on the origin of the oceanic water is intensely interesting, leading up as it does to the origin of the salts contained therein, which problem largely depends on the earth's age, and the planetesimal theory of the origin of the earth seems to explain best the origin of the ocean, the salts according to this view being from the first present in the earth's nucleus.

The practical part of the subject is not neglected, and we find all the most recent information as to the methods adopted for investigating the ocean depths, estimation of salinity and hydrogen-ion concentration, temperature and density, whilst the measurements of the tides and ocean currents are given a large amount of attention. Finally, in his summing up the author leaves us with hints on the lines for future research which will surely appeal to all who are interested in any way in the science of the sea. The book should have a large circle of readers and is a valuable work of reference for all lovers of true geography.

Marie V. Lebour.

Medical and Veterinary Entomology. By W. B. Hermes. Second edition. [Pp. xiv + 462.] (New York and London: The Macmillan Co., 1923. Price 28*s*. net.)

Medical entomology may be defined as the science which deals with the study of insects, arachnids, and associated organisms as causative agents or vectors of disease, and with methods for controlling them. An extensive and widely-scattered literature has developed around this branch of entomology within recent years, and a new edition of Prof. Hermes' textbook, which includes the progress made in the subject since the first edition appeared, will be greatly appreciated.

Students of parasitology, medical men, particularly medical officers of health, and veterinary officers, both at home and abroad, who must of necessity keep themselves in touch with advances made in medical entomology, will find this work a valuable and readable textbook. It is a systematic and liberal survey of the subject, and the author writes with authority, having had a wide personal experience both as a teacher and investigator in the laboratory and as an experimenter, observer, and adviser in the field.

While the book is in the main based on experience in America, the work of authorities in other countries has been freely drawn upon, where new observations have been recorded. Several chapters have been revised and brought up to date and a brief historical account of the subject has been added.

There are twenty chapters, of which chapters i–v deal with a general historical review of medical entomology, the anatomy and classification of insects, and the importance of insects as parasites and disease carriers.

The remaining chapters deal with particular groups of insects and the diseases associated with them. The section on mosquitoes occupies chapters ix–xi. There is some inconsistency in the use of the scientific names of one or two species, which may be a little confusing to the reader who is not familiar with the difficulties of nomenclature. For instance, for the yellow-fever mosquito, *Ædes argenteus* Poiret is given on p. 4, whereas on p. 152 *Ædes ægypti* = *Stegomyia calopus* is used. Similarly on p. 2 *Culex fatigans* Wied (probably = *quinquefasciatus* Say) is given, whereas on p. 155 *Culex* (*fatigans*) *quinquefasciatus* Say and *Culex* (*fatigans* Wied) *quinquefasciatus* Say are given, and again on p. 157 *Culex quinquefasciatus* (= *C. fatigans*).

The house-fly, the diseases associated with it, and methods of control, is fully dealt with in chapters xiii–xiv, observations based on the experience of the author with the army in the field during the war period being included.

There is a typographical error on p. 223, line 14 being misplaced to line 17.

Chapter xv treats of blood-sucking flies, tsetse-flies, stable-flies, etc.

The chapter on lice (vii) has been extended to embrace the more recent work on the relation of these insects to typhus and trench fever, and practical methods for combating them. Other chapters deal with fleas (xvii), ticks and mites (xviii-xix), cockroaches (vi), cone noses and bed-bugs (viii), buffalo gnats and horseflies (xii), venomous insects and arachnids (xx), Myiasis (xvi).

A bibliography is not given, but a number of important special papers are referred to in footnotes. The author points out in the preface that information as to bibliography may be readily obtained from the bibliographical works of other writers. It would be an advantage if a short bibliography, including a few of the more important papers, were given at the end of each section. There is an index to subjects and to authors. The work is well illustrated with 229 figs., mostly half-tone blocks, the subject-matter is clearly set out, and the price at 28*s*. is reasonable.

James Davidson.

The Physical Basis of Life. By E. B. Wilson. [Pp. 51, with 6 plates and 20 illustrations.] (New York: Yale University Press; London: Humphrey Milford, 1923. Price 7*s*. net.)

This beautiful, and therefore expensive, little volume presents the first William Thompson Sedgwick Memorial Lecture delivered by Prof. Wilson in Massachusetts at the end of 1922. It is significant of the breadth of vision of him whom the lecture commemorates that those who sought to do him honour turned not to a lecturer notable in the field in which Sedgwick himself had been long active—public health and hygiene—but rather sought to emphasise the fundamental biological attitude from which he approached practical problems by extending their invitation to an academic cytologist.

The subject of the lecture may be described as the anatomy of the cell in the sense that the physicist might be tempted to-day to speak of the anatomy of the crystal. Prof. Wilson is happy to express the view that chemists, physicists, and cytologists are united in the opinion that protoplasm is to be considered, not as a chemical entity, but as a morphological

concept. Life is not so much a question of molecular structure as of dynamic equilibrium in a polyphasic system. Just as with the atomic theory modern work has but given reality to a hypothetical conception, so, in genetics and cytology, the conception of the chromosomes in the minds of Sutton, Roux, and De Vries has become a real, if devastating, nuclear microcosm. We are led beyond the limits of the microscope even till we are within reach of the dimensions of the colloidal world. What then? The author will have no burkeing of the question, If life is "organisation" wherein resides this orderliness? We do not know. The mechanist, the seeker, speaks in the words "Some day we may be able to show how, in accordance with recognised principles of physics, a complex of specific, autocatalytic, colloidal particles . . . can engineer the construction of the vertebrate organism." The great riddle will remain to-morrow, but the reader of to-day will find much to inspire him in the imaginative survey and lively speculation which this lecture offers.

R. K. C.

Heredity and Eugenics. By R. RUGGLES GATES, Ph.D., F.L.S. [Pp. xiii + 288, with 35 illustrations.] (London: Constable & Co., 1923. Price 21*s.* net.)

PROF. GATES insists that a knowledge of inheritance must form the basis of any enlightened attempt to influence the future development of the human race. As the number of characters in man known to follow a Mendelian type of inheritance is surprisingly large, he gives a general account of Mendelism; this is followed by a chapter on the inheritance of physical characters in man, which to a great extent treats of dwarfs, albinos, various digital malformations, and other abnormalities, reminding us of Sir Archdall Reid's remark, that no one has yet seen a useful or a beautiful mutation. With regard to skin-colour we are told that the evidence in favour of a strictly Mendelian inheritance in white-black crosses is by no means conclusive; this would seem to apply also to the inheritance of mental and moral characters, for it is a matter of theoretical interest rather than of practical importance whether, by imagining the existence of a large number of factors, the inheritance of these characters can be said to follow Mendelian principles. Davenport's attempt to analyse temperaments and their inheritance does not convince Prof. Gates, who states "the desirability of creating 'factors' for calmness and cheerfulness appears very doubtful." A chapter on the limits of heredity is concerned mainly with the evidence from "identical" twins, showing that the resemblances between them may extend to minute structural details, and also to tastes, dispositions, and talents.

With regard to Eugenics, Prof. Gates considers that the problem of formulating laws to foster an increase of the more efficient and desirable members of society would seem almost to surpass the wit of man, but he thinks that the prevention of reproduction on the part of undesirables, such as the feeble-minded, is an obvious negative measure that can be carried into effect. It almost seems that for the present we shall have to rely on education and environment for racial improvement.

C. T. R.

A Bibliography of Eugenics. By SAMUEL J. HOLMES, Professor of Zoology in the University of California. [Pp. iv + 514.] (Berkeley, California: University of California Press, 1924.)

THE scope of this bibliography is ambitious; it contains references to something approaching 10,000 books or contributions to periodicals. These are classed according to the subject-matter into about forty sections, and within

each section are arranged under the author's name in alphabetical order. Thus there are sections entitled Heredity and Evolution, The Problem of Degeneracy, Alcohol in Relation to Heredity, The Birth Rate, The Sex Ratio, etc., etc. As is almost inevitable in so wide a field of inquiry, there are a number of omissions, while references to many rather unscientific treatises and articles have been included; but the book is to be welcomed as something of real assistance to the student of Eugenics who has sufficient judgment to pick the good from the bad. E. S. P.

MEDICINE

The Insulin Treatment of Diabetes Mellitus. By P. J. CAMMIDGE, M.D., D.P.H. [Pp. viii + 172, with charts.] (Edinburgh: E. and S. Livingstone, 1924. Price 6s. net. Post free 6s. 5*d*.)

THIS volume is presumably to be regarded as a supplement to the recent publication of Dr. Cammidge and Mr. Howard entitled *New Views on Diabetes Mellitus*. The latter provided a comprehensive survey of the condition of glycosuria, and advanced an ingenious hypothesis to correlate the many factors known to be involved. The advance which has attended the discovery and protracted use of insulin received no great attention—indeed the book was probably in press before any but preliminary notice of the experimental use of the pancreatic preparation was to hand. Dr. Cammidge therefore now records his own extensive experience with the clinical effects of insulin and claims that these tend to confirm the hypothesis previously published.

The subject is approached through an historical survey of the relation of the pancreas to diabetes and the early work of the Toronto school with insulin. A résumé of the author's theory follows, and the discussion is directed particularly towards an analysis of the symptom complex which we call diabetes in so far as to marshal the indications and contra-indications for insulin treatment. The author's method of administration and control of the treatment is detailed and illustrated by a few exhaustive case records.

All familiar with the works of Dr. Cammidge will need no reminder that he is wedded to the chemical aspect of metabolism, and therein has chosen a sound if exacting partner. Admiration will not be withheld from the exhaustive labour attending the routine analyses of urine, blood, and fæces which govern the control of a case in his hands, though the programme as outlined will appal the majority of his readers.

The exacting demands which Dr. Cammidge makes of empirical analytical data in building up his views are certain to meet with criticism. In particular it seems that the "difference value" of the blood sugar, which forms a recurrent argument, is a chemical quantity of dubious origin and doubtful significance, whilst the tacit assumption that the determination of the sugar of the blood when performed on 0·2 c.c. has an accuracy of 1–2 per cent. cannot be upheld. Confidence is not increased by such careless slips as the statement on page 77 that the calcium content of the blood is 5·3 to 6·8 per cent., and on page 125 the record of a blood sample which contained 12 per cent. of phenols! In the field of insulin literature—so badly overgrown—this book merits the attention of the clinician and the criticism of the pathologist. R. K. C.

ENGINEERING

Design of Concrete Structures. By L. C. URQUHART, C.E., and C. E. O'ROURKE, C.E. [Pp. ix + 452, with 163 figures.] (London: McGraw-Hill Publishing Company. Price 20*s*. net.)

As stated in the preface, this book only aims at such treatment as would be given in an elementary course at an engineering college. The general

theory and some simple designs are well treated, but the portions dealing with more advanced detail have been somewhat severely curtailed.

The preparation of concretes of varying strengths by using different proportions is very fully dealt with in the first chapter.

The treatment of Shear and Diagonal Tensile Stresses is rather hard reading, which, however, is usual in textbooks. The section dealing with the design of T beams is excellent and very practical. A number of very useful curves for Bending and Direct Stress designs are incorporated.

The book owes much to the matter taken from *Reinforced Concrete Construction*, by G. A. Hool.

On the whole the book should be very useful to those for whom it is prepared.

H. T. D.

Stresses in Framed Structures. By G. A. Hool and W. S. Kinne. [Pp. xiv + 620.] (London: McGraw-Hill Publishing Co. Price 25*s.* net.)

This volume is one of a series of six, and is itself a collection of articles by a number of engineers, each of whom is an expert in the subject of his special section. The six books are each independent, so that the one under consideration is complete in itself. The authors suggest that the book may be of service as a textbook in engineering schools and colleges; but, if so, it would be more especially for the most advanced classes in the Theory of Structures. The book is a very exhaustive treatise on the subject of the title, and the treatment is very far from being diffuse. It is, in fact, a condensed technical work containing a great fund of information, tables, and formulæ for practical design. Many completely worked examples are interspersed with the text and difficult points cleared up by their aid. Both graphical and analytical methods are freely dealt with, and an appendix contains tables of results for a very great number of practical cases, both ordinary and statically indeterminate. The book can be warmly recommended to the advanced student and to the designer.

H. T. D.

Théorie Générale sur les Courants Alternatifs. Par M. E. Piernet. Premier fascicule. [Pp. x + 100.] (Paris: Gauthier-Villars et Cie, 1924. Price 12 frs.)

A clearly written treatise on alternating current theory from elementary principles up to rotating elliptical fields.

The simple single phase sine wave voltage is first considered, followed by current and power, and the effect of induction and capacity. This is followed by the general consideration of polyphase circuits and the vector and graphical methods of treatment.

Now follows a consideration of non-sinusoidal wave form and a very complete treatment of the Fourier method of analysis. At this point practical methods of wave form determination are described, amongst them being the oscillograph, ondograph, and the harmonic resonance method.

The last chapters are taken up with the application of complex quantities to alternating current problems and the mathematical treatment of rotating fields. These chapters are particularly well written.

A valuable feature is the completeness of the argument throughout the work—all steps, no matter how obvious they may appear, being shown in full. Forming as it does a sound textbook on alternating current theory and introducing to the reader the electrical vocabulary of the French, it should prove of great value as a textbook in French for engineering students.

A. N. Jackson.

Principles and Practice of Telephony. Vol. I.—Principles and Apparatus. By JAY G. MITCHELL, Am.I.E.E. [Pp. v + 245, with 98 illustrations.] (London: MoGraw-Hill Publishing Company, 1924. Price 12s. 6d.)

THE first of five volumes covering the principles and practice of telephony, and intended for the use of non-technical readers.

The general principles are covered more or less completely within limits imposed by the class of reader for whom the work is intended.

Various types of transmitters, receivers, ringers, relays, etc., are described in some detail, the more important practical points of design being indicated.

The chapters on "Magnetic Induction" and "Alternating Currents" endeavour to describe the phenomena in a non-technical manner, which is not altogether convincing. All intricate explanation is avoided by the use of analogies which in some cases are quite unsuitable.

Other statements are rather misleading, and in some cases definitely incorrect, among which may be quoted:

Page 19: "When additional ampère turns . . . produced no increase in flux the iron is working at the point of magnetic saturation."

Page 113: "Whenever an electric current is sent through a circuit all of its energy that is not expended in overcoming resistance or in doing work . . . is dissipated in the form of heat."

Although written for non-technical readers, such looseness of expression can hardly be justified.

A. N. JACKSON.

The Control of the Speed and Power Factor of Induction Motors. By MILES WALKER, M.A., D.Sc. [Pp. 151, with 125 diagrams.] (London: Ernest Benn, 1924. Price 18s. 6d. net.)

RECENT years have seen great developments in the performance and application of induction motors. The present work summarises these modifications in a convenient form for practical use.

As introduction the author describes the inversion method of vector diagram solutions, and then applies it to the construction of the circle diagram. In subsequent chapters modifications to the simple diagram are described, which enable it to be applied to more complex forms of motor.

The remainder of the volume is devoted to the methods of control of speed and power factor at present available. Each is considered under many headings, among them being: rotor resistance, cascade running, pole changing, multiple windings, Leblanc exciter, Hunt, Creedy, and double rotor motors for speed control: auxiliary rotor windings, alternating voltage supply to slip-rings, synchronous and frequency converters, etc., for power factor variation. The characteristics and application of the various methods are enumerated, and in most cases illustrations of typical installations are given together with an ample description.

Winding diagrams are provided where desirable, and performance curves readily enable comparisons to be made.

The final chapter is devoted to a more detailed treatment of the phase advancer (Leblanc Exciter) with application of the circle diagram corrected for iron saturation. The Kapp vibrator is also treated.

It is to be regretted that relative costs are not quoted, as, although the performance in some cases is very good, it may be counteracted by intricate design or the necessity for much auxiliary apparatus with consequent heavy first cost.

Considering the wide field covered the treatment is very thorough, and the references to original papers and sources of information enhance the value of the work.

Such a summary must be of great service to an engineer faced with the installation of variable speed plant or the improvement of power factor, while the detailed description of the inversion method for graphical solution and the circle diagram are of general application.

A. N. JACKSON.

The Manufacture of Electric Steel. By FRANK T. SISCO. [Pp. x + 304, with 44 illustrations.] (London: McGraw-Hill Publishing Co., 1924. Price 15s. net.)

WITHIN the last ten years the development of electro-thermal methods of steel manufacture has been rapid and extensive. In this country the large demand for alloy steel, the need for utilising the accumulation of heavy steel turnings, and the shortage of high-grade raw materials during the war period presented exceptional opportunities for the electric furnace, but at the present time the economic conditions and the cost of power are not favourable to a wide use of this furnace. In the United States the conditions are not quite the same and, although in recent years there has been a big reduction in the amount of steel made in the electric furnace, the total production now is between 150,000–200,000 tons per year.

The first book to give a complete account of the electro-metallurgy of steel was that written by Mr. C. C. Gow in 1921 on the modern English processes. In the work now under review, Mr. F. T. Sisco describes very carefully and thoroughly the application of electricity to the melting and treatment of steel in America. Naturally, in view of the importance of the basic process, most of the volume is devoted to this, one chapter only being allotted to acid-melting practice. The chemical reactions are considered, and the actual making of a heat of steel described. Following an introductory chapter on the past, present, and future of "electric steel," the early chapters deal with the materials used and the electric arc furnace. Later chapters consider in quite a complete manner the cold scrap process. A chapter is given to the hot metal process which includes refining steel. The chapter entitled "Electric steel pouring practice," on casting temperature, rate of teeming, mould design, etc., is on a subject which is of importance in steel-making generally.

The object of the author has been to describe fully the process of making steel in the electric furnace; to consider power supply, electrodes and linings, to describe generally various kinds of furnaces, but to give little attention to electrical details.

The electric furnace provides the steel-maker with an appliance which has quite special deoxidising features—the atmosphere is strictly reducing, while very fluid deoxidising and desulphurising slags can be produced—which means that particularly high-grade metal is possible. Mr. Sisco is enthusiastic on this point, and predicts an extensive use for such steel in the future.

The book is clearly printed and well illustrated, and can be recommended to those engaged on making steel in the electric furnace, to the young metallurgist, and to those interested in the subject.

E. COURTMAN.

The Design of Diagrams for Engineering Formulas and the Theory of Nomography. By LAURENCE I. HEWES, B.Sc., Ph.D., and HERBERT L. SEWARD, Ph.B., M.E. [Pp. xi + 108, with 83 illustrations.] (London: McGraw-Hill Publishing Company, 1923. Price 25s. net.)

THIS book is a short and admirable treatise on the general principles of "Nomography," and their practical application to engineering formulæ. It is based throughout on the methods of analysis adopted by Prof. M. d'Ocagne, the originator of this valuable system of applied mathematics.

Colonel R. K. Hezlet, in 1913, published the first English exposition of the subject, written in conformity with the original treatment of d'Ocagne; but to the average reader, with scant knowledge of analytical geometry and the properties of determinants, it was not very comprehensible, and in consequence appealed only to a limited circle.

In the volume under review the same ground has been covered, but in a more comprehensive manner, and this extended treatment, together with the numerous worked-out cases of an advanced character, should be of great assistance to those students of the subject who have hitherto found a difficulty in following the general methods of analysis and chart construction.

In the preface the authors state that "the aim of the present writing has been therefore not merely to give elementary methods of drawing simple diagrams, but also to develop the grasp of the reader, so that he will be able to analyse the more complex formulas of engineering practice."

The latter part of this statement is fully borne out by the authors' consistent adherence to the fundamentals. General functional notation is the medium of expression throughout. Although on a first reading the reader acquainted only with elementary algebra may have some difficulty in grasping the significance of the unfamiliar symbolism, further perusal and study of the very clear explanatory text will soon remedy this defect, and he will then find himself in a position to deal effectively with the graphical presentation of the more complicated types of engineering formulæ.

In this country, as a rule, the "theory of determinants" is not made a part of the ordinary college course of the engineer, and, as already indicated, the use of the determinant as an analytical tool proves a stumbling-block to the majority of readers.

To the reader with a fair knowledge of elementary algebra the difficulty here will be found more apparent than real. Only a few elementary and simple properties of the third order of determinants are required in nomographic design, and this necessary information the authors have given concisely in an appendix, which can be read and quickly mastered before the reader commences the study of the alignment diagrams in chapter iii. The determinant method of analysis is used from the beginning of this chapter to the end of the book.

The book is divided into six chapters. In chapter i the function scale, scale factors, and various graphical methods of deriving function scales from already existing ones are discussed and illustrated. This preliminary matter is followed by a discussion of the simple form of nomogram derived from an equation of two variables.

Chapter ii is devoted to the "simple or elementary diagram," graphed from an equation of three variables. This is the familiar type consisting of a family of curves of one of the variables plotted in the co-ordinate field. The principle and application of "anamorphosis," whereby such a family of curves is converted into a system of straight lines, is next discussed, and various cases, including the logarithmic chart, are worked out in detail. The special case of the hexagonal diagram is also considered. The functional notation employed in this section should afford the reader, who is not versed in this, a good opportunity of studying and becoming familiar with its use, before he takes up the more difficult matter dealt with in subsequent chapters.

In chapter iii the alignment or collinear "type of nomogram," derived from an equation of three variables, is discussed at considerable length. The methods of transformation of the given formula into the "first" and then to the final "reduced" determinant form, and the formation from the latter of the "parametric" defining equations of the three corresponding plane curves, are clearly explained. Their applications are then very fully illustrated in detail, in the design of eighteen nomograms of well-known engineering formulæ.

Chapter iv deals with the case of the alignment type of nomogram for an equation of more than three variables. A general discussion of "binary function scales" and "curve-nets" is given, and is followed by detailed consideration of the collinear diagram with two parallel scales and one curve-net for an equation of four variables, then the collinear diagram with three curve-nets for an equation of six variables.

Four nomograms of this type are worked out in detail.

In chapter v "diagrams of alignment with two or more indices" are discussed. The "index" referred to by the authors is the operative line which has to be drawn across the scale axes or supports of the nomogram in order to obtain a solution of the given equation. In many instances this equation can be split up into two or more parts, each part containing two of the variables and connected with the succeeding part by an auxiliary variable.

When this division is possible, two or more alignment nomograms of three variables can then be constructed and linked together. The scale lines or supports of the auxiliary variables form the connecting links and are called "hinge or pivot scale supports." This is the type of nomogram discussed, together with a variant for a four-variable equation in which the pivot axis is dispensed with and the two index lines are drawn either parallel or perpendicular to one another.

Six nomograms of complex engineering formulæ are worked out in detail.

In chapter vi the authors have introduced what they state is "a new class of diagram based on the fundamental principles already developed." This is called an "alignment diagram with adjustment," since the index line has to be pivoted about a known point in one curve-net until it determines the value of the unknown variable simultaneously in two other curve-nets.

By this general alternative method the number of types of equations to which the alignment principle is immediately applicable is enlarged, and various equations, which cannot be readily identified with the type equations discussed in the preceding chapters, can be effectively dealt with. It is shown, in fact, that the type equations previously discussed are simply special cases of the general type dealt with in this chapter. Seven charts illustrating the application of this general principle are worked out in detail.

In addition to the appendix on "determinants of the third order," there is another on "the projective transformation," which is implicitly involved in various algebraical processes necessary for the construction of the alignment type of nomogram.

A list of fifty-three type equations discussed throughout the text is given at the beginning of the book, and also a list of the examples solved.

At the end of each chapter a number of problems are given for solution by the reader.

Most of the charts, which are excellently drawn and figured, fill the page, which measures 12 inches × 9 inches. The scales are well chosen and open, and the results can be read to a degree of accuracy quite sufficient for practical purposes.

The type-setting is excellent, and the text is practically free from typographical slips—a matter of great importance in a book of a mathematical nature.

Altogether the book reflects credit on both authors and publishers. It is an important addition to the literature of this subject in English, and may be recommended with confidence to those engineers who wish to obtain a comprehensive grasp of the fundamental principles of this important branch of applied mathematics and put their knowledge to practical use in their professional work.

MISCELLANEOUS

(1) **Psycho-Analysts Analysed.** By P. McBride, M.D., F.R.C.P.E., F.R.S.E. [Pp. ix + 142.] (London: William Heinemann, 1924. Price 3*s.* 6*d.* net.)

(2) **A Critical Examination of Psycho-Analysis.** By A. Wohlgemuth, D.Sc. [Pp. 250.] (London: George Allen & Unwin; New York: The Macmillan Company. Price 10*s.* 6*d.* net.)

(3) **Modern Theories of the Unconscious.** By W. L. Northridge, M.A., Ph.D. With an Introduction by Prof. J. Laird, M.A. [Pp. xv + 193.] (London: Kegan Paul, Trench, Trübner & Co., 1924. Price 8*s.* 6*d.* net.)

So many books are now being written on psycho-analysis that we suffer from an *embarras de richesses* and hesitate to say anything at all on the subject, especially as it is a medical one which is rather outside the scope of SCIENCE PROGRESS. But these three works afford a good opportunity for the study of scientific reasoning at least, and the two first ones pretty well dispose of a much-advertised bogey. The books of Drs. P. MacBride and A. Wohlgemuth appeared simultaneously and are in general agreement as to the invalidity of what is called psycho-analysis; and we think that the authors have substantiated their case. At the present day there are many spurious medical movements which, like weeds, flourish for a day or two and then die down. We should say, judging from numerous excerpts in these books and from some of his own writings which we have read, that the dream-theory of Dr. Freud is almost entirely baseless. Probably every dream that has ever been dreamed could be interpreted in many different ways. For instance, Dr. Wohlgemuth gives an analysis of the famous dream of Pharaoh regarding the fat kine and the lean kine, and shows that Joseph's interpretation was only one of many possible interpretations. The same can evidently be said of all the interpretations given by all the psycho-analysts of all the dreams dealt with by them. In fact the interpretation evidently brings in an outside element, namely, the mind of the interpreter; and on reading the numerous examples given by the psycho-analysts we cannot help thinking that it is the psycho-analyst himself who is describing his own Psyche. We do not wonder that Sir Clifford Allbutt exclaimed regarding these efforts: "Is it unfair to say that the interpretations given by psycho-analysts to the dreams of our modern Jacobs and Daniels are incredible nonsense?" Then, again, the psycho-analysts bring in an entity which they call the subconscious mind, without proving that there is any such thing. Undoubtedly our memory holds facts which we forget for years until something recalls them out of the depth of the great mass of knowledge which is held within every person's intellect—such as, for instance, the remembrance of faces of persons whom we have not seen for a long time. But this is not the same thing as the alleged "subconscious mind" of the analysts. We think it likely that the subject has now had its day and will shortly disappear from scientific ken. There may be one or two small points of value contained in this "new philosophy," but they seem to be mixed up with too much spurious matter to be of any real use.

The third book, however, is apparently in favour, not only of psycho-analysis, but of many other speculations, such as telepathy, ghosts, and similar entertainments of a few peculiar minds, and can hardly be dealt with as a serious contribution to science.

Comparing such books with some recent excursions of exceptional medical men, one really begins to wonder whether a little senility is not now beginning to appear in the public intellect, as it did in Italy after the third century. Science has flourished exceedingly for five centuries; but we know what

happened to the science of the ancients after it also had flourished for a period not much longer than that which has elapsed since Copernicus. Are we certain that all of those who dabble in scientific hypotheses are really intellectually capable of climbing the arduous scientific Parnassus?

R. R.

An Elementary Treatise on Frequency Curves and their Application in the Analysis of Death Curves and Life Tables. By ARNE FISHER. With an Introduction by RAYMOND PEARL. [Pp. xv + 240, with 11 figures and an Index.] (New York: The Macmillan Co., 1924.)

ALTHOUGH the large-type heading of this book is *Frequency Curves*, Mr. Fisher's main purpose is to present a new method of construction of mortality tables which he believes will be of great value in supplementing the present actuarial methods. This proposed method depends, however, on the fitting of Charlier's Frequency Curves, and the first half of the book is therefore devoted to this subject. It would be out of place to discuss here the relative merits of these and other types of frequency curves, but there is no doubt that the English statistician or actuary who has only vague ideas of the Scandinavian methods associated with the names of Gram, Thiele and Charlier will find Mr. Fisher's first chapter well worth reading. The several methods of procedure for fitting both the "A" and "B" type curves, whether by finding the constants in terms of the moments of the observations, or by least squares, or by a logarithmic transformation, are all illustrated with numerical examples. One gathers, however, that it is only after considerable experience that the statistician can decide which of these half-dozen methods to apply, and in this way the theory appears to compare unfavourably with that of the Pearson system of curves where definite discriminating criteria exist.

The second half of the book is entitled "The Human Death Curve." Here is outlined the method of forming mortality tables from a knowledge of deaths alone, *i.e.* without information of the numbers exposed to risk at each age. The method depends on the hypothesis that it is possible to divide the causes of death into seven or eight typical groups, and that within each group the percentage frequency distribution of deaths according to age, in a stationary population, can be represented by a Charlier frequency curve whose parameters can be chosen *a priori*. We have, for example, deaths typical of (1) Extreme Old Age, or again (vii^a) Early Life Occupational Hazard, etc., the whole of the 189 causes of death of the International Classification being divided in this way among the groups.

The method is a novel and suggestive one, but the sole justification depends on the validity of this hypothesis. In the case of the material from the State of Massachusetts where the numbers exposed to risk were known, a table has been calculated by Mr. Fisher in the ordinary way, and the final rates of mortality for different ages obtained by new and old methods show satisfactory agreement. On the other hand, for the Michigan data, the diagrams given to illustrate "Goodness of Fit" between actual and theoretical proportional death-rates are not convincing. For all the seven groups represented there is a systematic difference between observation and theory, suggesting that the particular system of Charlier curves used by Mr. Fisher does not adequately represent the American data. And one cannot help wondering whether an actuary is really content "for practical purposes" with fits that often give theoretical frequencies differing 10 per cent. and sometimes 20 per cent. from the observed values in a group of 1,000 or 2,000 deaths?

But if the author appears a little over-sanguine about the adequacy of his methods, they are certainly worth studying.

E. S. P.

Cambridge Readings in the Literature of Science. Arranged by WILLIAM CECIL DAMPIER WHETHAM, M.A., F.R.S., and his daughter, MARGARET DAMPIER WHETHAM. [Pp. x + 275.] (Cambridge: at the University Press. Price 7*s*. 6*d*.)

THIS entertaining little book comes very opportunely. The tide of literature in scientific history flows well and strong, and marks a happy movement in the right direction. Attention has been drawn increasingly during the past few years to the cultural aspects of scientific education. There is something more in science than the cash value of its utilitarian inventions and its capacity to produce "death" and other rays. The progress of scientific thought through the ages is interlocked with the progress of civilisation generally; each has reacted on the other, so that it is right and inevitable that the liberal-minded student of humanity must count within his ken the influence upon humanity of the broad movements in scientific history. It is but a short year since London University, recognising this fact, set up its new Board of Studies in the History and Method of Science under the chairmanship of Prof. Whitehead and the secretaryship of Dr. Charles Singer; and now, carrying this to its logical conclusion, a new M.Sc. degree has been instituted in the History and Method of Science which undoubtedly opens up a new era in the conception of university teaching in scientific thought. It is hoped that other universities will quickly respond to the lead given by London. Certainly the fine scheme of lectures and lecturers arranged is a happy augury of the attitude of our foremost teachers of science towards the scheme as a whole.

In their way, such works as that which is here under notice all serve to play their part in this important project. Hence our view as to the timely appearance of these *Cambridge Readings in the Literature of Science*. The authors have planned their work well. In a review of the past, two essentials to success are, as a matter of practical experience, the rigid adherence to a systematic plan, and the avoiding of doing too much. The authors have succeeded well in both respects. They have developed the story of three broad movements in the history of science—the evolution of modern conceptions of cosmogony, the growth of atomic theories, and the development of conceptions of biological evolution. Here are three important stories, and our authors have rightly allowed the philosophers of the past to speak for themselves. The arrangement of matter and the preliminary notes on each writer are excellent, as are also the illustrations and index. A book to be thoroughly recommended.

I. B. H.

Visual Education: A Comparative Study of Motion Pictures and other Methods of Instruction. Edited by F. N. FREEMAN. [Pp. viii + 391.] Chicago: University of Chicago Press. Price $3.50.)

THIS interesting work gives a full account of a comprehensive series of experiments made in various schools of the United States on the use of motion pictures in education. Chosen subjects were taught in two or three ways: by demonstration, or ordinary oral teaching, by the film with sub-titles, and sometimes also by fixed lantern-slides with or without oral teaching. The children were in each case subjected afterwards to a test, and the results tabulated. A certain degree of artificiality may have arisen from the necessity to use existing films and base the lessons on them. Teachers, however, who are interested in the question will find the report well worth perusal. The limitations of the use of the film in education are just those which general considerations would lead us to expect.

W. E. B.

Economic Geography. By R. H. Whitbeck and V. G. Finch. [Pp. x + 558.] (London: McGraw-Hill Publishing Co., 1924. Price 17*s.* 6*d.* net.)

In common with most American geographical publications, the first impression made by this book is the excellence of its maps, diagrams, and other illustrations, which are both profuse and pertinent. One criticism, however, may be made. Many of the distribution maps have no date, nor do they say on how many years' average they are based.

The preface states that the book is intended for "students of college age," and aims at showing "human adjustment to geographic environment." At the same time, the "utilitarian" aspect has not been neglected.

More than half the book is taken up with the section on the United States and Canada, and these pages are distinctly good, with the emphasis in the right place and a number of exceedingly well-made points, *e.g.*: "The cotton-manufacturing industry of the United States has outgrown its domestic market," p. 103. Another praiseworthy feature of this section is the attention paid not only to exports, but to imports also, *e.g.*: of raw silk, of woollen and even cotton goods, etc. The chapter on Fuel and Power is particularly interesting, with its eloquent cross-sections across the Appalachian and the East Pennsylvania coal-fields, its diagram of the pipe-line system for distributing petroleum and its reasons why the United States are *not* likely to become large exporters of coal in the near future. The chapter on Inland Transportation makes some very pertinent remarks on the "glamour" surrounding inland waterways.

It follows, from the amount of space devoted to the United States, that the treatment of the rest of the world is slight.

As far as South America is concerned, the summaries are adequate, with emphasis on essential points, and are assisted by maps illustrating, *e.g.*, mineral products of the west coast, the irrigated lands of Peru, etc.

The chapters on Europe, though fuller, suffer somewhat from over-compression, but the introductory chapter, though primarily for the American public, may be helpful even to Europeans, since it deals with facts which, because so obvious, are often overlooked. It is disappointing, however, to find a map purporting to show the principal racial stocks of Europe actually showing merely linguistic divisions.

The sections on Asia, Africa, and Australia are slight, and nowhere approach the excellence of the United States chapters.

The statistics are mostly well up to date, in some cases being as recent as 1923–4.

M. R. Shackleton.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Practical Calculus for Home Study. By Claude Irwin Palmer, Associate Professor of Mathematics, Armour Institute of Technology. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1924. (Pp. xx + 443.) Price 15*s*. net.

The Mathematical Groundwork of Economics. An Introductory Treatise by A. L. Bowlby, Sc.D., F.B.A., Professor of Statistics in the University of London. Oxford: at the Clarendon Press, 1924. (Pp. viii + 98.) Price 7*s*. net.

On the Direct Numerical Calculation of Elliptic Functions and Integrals. By Louis V. King, M.A., D.Sc., F.R.S., Macdonald Professor of Physics, McGill University, Montreal. Cambridge: at the University Press, 1924. (Pp. viii + 42.) Price 3*s*. 6*d*. net.

Linear Integral Equations. By William Vernon Lovitt, Ph.D., Professor of Mathematics, Colorado College. New York and London: McGraw-Hill Book Company, 1924. (Pp. xiii + 253.) Price 15*s*. net.

Astronomie Générale. Par Luc Picart, Directeur de l'Observatoire de Bordeaux. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel, 1924. (Pp. vi + 188.) Price 6 frs. net.

La Théorie de la Relativité. Par M. von Laue. Traduction faite d'après la quatrième édition allemande revue et augmentée par l'auteur. Par Gustave Létang. Tome I, Le Principe de Relativité de la Transformation de Lorentz. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins. (Pp. xvi + 331.) Price 40 frs.

The Theory of Relativity. Studies and Contributions. By Archibald Henderson, Allan Wilson Hobbs, and John Wayne Lasley, Jr., of the Department of Mathematics in the University of North Carolina. Chapel Hill, N.C., U.S.A.: The University of North Carolina Press; London: Oxford University Press, 1924. (Pp. xiii + 99.) Price 11*s*. 6*d*. net.

Einstein's Theory of Relativity. By Max Born, Professor of Theoretical Physics in the University of Göttingen. Translated by Henry L. Brose, M.A., Christ Church, Oxford. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xi + 293, with 135 diagrams and a portrait.) Price 12*s*. net.

The National Physical Laboratory. Collected Researches. Vol. XVII, 1922. London: His Majesty's Stationery Office, 1922. (Pp. v + 353.) Price 17*s*. 6*d*. net.

The New Theories of Matter and the Atom. By Alfred Berthoud, Professor of Physical Chemistry at the University of Neuchâtel. Translated from the French by Eden and Cedar Paul. London: George Allen & Unwin, 40 Museum Street, W.C.1; New York: The Macmillan Company. (Pp. 258.) Price 10*s*. 6*d*. net.

The Structure of Matter. By J. A. Cranston, D.Sc., A.I.E., Lecturer in Physical Chemistry, Royal Technical College, Glasgow. London: Blackie & Son, 50 Old Bailey, 1924. (Pp. xvi + 196.) Price 12*s*. 6*d*. net.

The Evolution of Mathematical Physics. Being the Rouse Ball Lecture for 1924. By Horace Lamb, Sc.D., F.R.S., Honorary Fellow of Trinity College, Cambridge. Cambridge: at the University Press, 1924. (Pp. 48.) 2*s*. net.

Matter and Change. An Introduction to Physical and Chemical Science. By William Cecil Dampier Whetham, M.A., F.R.S. Cambridge: at the University Press, 1924. (Pp. viii + 280, with 103 figures and 3 plates.) Price 7*s*. 6*d*. net.

The Earth: Its Origin, History, and Physical Constitution. By Harold Jeffreys, M.A., D.Sc., Fellow and Lecturer of St. John's College, Cambridge. Cambridge: at the University Press, 1924. (Pp. xi + 278.) Price 16s. net.

Atoms and Rays. An Introduction to Modern Views on Atomic Structure of Radiation. By Sir Oliver Lodge, F.R.S. London: Ernest Benn, 8 Bouverie Street, E.C.4, 1924. (Pp. x + 208.) Price 21*s*. net.

The Kinetic Theory of Gases. By Eugene Bloch, Professor at the Lycée St. Louis. Translated by P. A. Smith, B.A. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xiv + 178.) Price 7*s*. net.

The Specific Heats of Gases. By J. R. Partington, M.B.E., D.Sc., and W. G. Shilling, M.C., M.Sc. London: Ernest Benn, 8 Bouverie Street, E.C.4, 1924. (Pp. 252, with 46 figures.) Price 30*s*. net.

A Course of Instruction in Instrumental Methods of Chemical Analysis. By William N. Lacey, Associate Professor of Chemical Engineering, California Institute of Technology. New York: The Macmillan Company, 1924. (Pp. vii + 95.) Price 7*s*. net.

Les Combustibules Liquides, et le problème du carburant national. Par M. Aubert, avec une préface de Paul Sabatier. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1924. (Pp. xv + 328.) Price 20 frs.

A School Chemistry. By O. J. Flecker, D.Sc., Teacher of Chemistry at Dean Close School. Oxford: at the Clarendon Press, 1924. (Pp. viii + 238.) Price 3*s*. 6*d*. net.

Textbook of Cellulose Chemistry. For Students in Technical Schools and Universities as well as for Cellulose Experts. By Emil Heuser. Translated from the second German edition by Clarence J. West and Gustavus J. Esselem, Jr. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1924. (Pp. xv + 212.) Price 12*s*. 6*d*. net.

Chemical Synthesis. Studies in the Investigation of Natural Organic Products. By Harry Hepworth, D.Sc., F.R.C., a Member of the Research Staff of Nobel Industries. London: Blackie & Son, 50 Old Bailey, 1924. (Pp. xx + 243.) Price 20s. net.

Complex Salts. By William Thomas, M.A., M.Sc., Ph.D., A.I.C., Lecturer in Chemistry, the University of Aberdeen. London: Blackie & Son, 50 Old Bailey, 1924. (Pp. xi + 122.) Price 10*s*. net.

Applied Chemistry. By Ira D. Farard, Ph.D., Professor of Chemistry in New Jersey College for Women, State University of New Jersey. New York: The Macmillan Co., 1924. (Pp. vii + 496.) Price 15*s*. net.

Introduction to General Chemistry. By William Foster, Professor of Chemistry in Princeton University. Princeton: at the University Press, 1924. (Pp. vii + 643, with 29 illustrations.) Price 17*s*. 6*d*. net.

Examples in Chemistry. By William W. Myddleton, D.Sc., Lecturer in Chemistry, Birkbeck College, University of London. London: Methuen & Co., 36 Essex Street, W.C. (Pp. viii + 134.) Price 3*s.* net.

The Carbohydrates and the Glucosides. By E. Frankland Armstrong, D.Sc., Ph.D., F.R.S., F.I.C., Fellow of the City and Guilds of London Institute. Fourth Edition. London: Longmans, Green & Co., 39 Paternoster Row, 1924. (Pp. xi + 293.) Price 16*s.* net.

A First Chemistry for Schools. By W. H. Hewitt, B.A., B.Sc., A.R.C.S., and S. T. Dark, B.Sc., Science Masters at Strand School. London: Methuen & Co., 36 Essex Street, W.C. (Pp. viii + 316, with 87 diagrams.) Price 5*s.* net.

The Plant Alkaloids. By Thomas Anderson Henry, D.Sc., Director, Wellcome Chemical Research Laboratories. Second Edition. London: J. & A. Churchill, 7 Great Marlborough Street, 1924. (Pp. viii + 456, with 8 plates.) Price 28*s.* net.

A Textbook of Geology. For use in Universities, Colleges, Schools of Science, etc., and for the General Reader. Part I, Physical Geology, by Louis V. Pirsson. Part II, Historical Geology, by Charles Schuchert. Second Revised Edition. New York: John Wiley & Son; London: Chapman & Hall, 1924. (Pp. viii + 724, with 237 figures.) Price 22*s.* 6*d.* net.

Permeability. By Walter Stiles, Sc.D., Professor of Botany in University College, Cambridge. New Phytologist Reprint, No. 13. London: Wheldon & Wesley, Arthur Street, New Oxford Street, W.C.2, 1924. (Pp. 296.) Price 12*s.* 6*d.* net.

Soil Management. By Firman E. Bear, Professor of Soils, the Ohio State University. New York: John Wiley & Sons; London: Chapman & Hall, 1924. (Pp. vi + 268, with 33 figures.) Price 10*s.* net.

Outlines of Fungi and Plant Disease. For Students and Practitioners of Agriculture and Horticulture. By F. T. Bennett, B.Sc., Assistant Lecturer in Agricultural Botany, Department of Agriculture, the University, Leeds. London: Macmillan & Co., St. Martin's Street, 1924. (Pp. xi + 254, with 24 plates.) Price 7*s.* 6*d.* net.

The Biology of Flowering Plants. By MacGregor Skene, D.Sc., Lecturer on Plant Physiology in the University of Aberdeen. London: Sidgwick & Jackson, 1924. (Pp. xi + 523, with 8 plates and 68 text figures.) Price 16*s.* net.

The Evolution and Distribution of Fishes. By John Muirhead Macfarlane, D.Sc., LL.D., Emeritus Professor of Botany. New York: The Macmillan Company, 1923. (Pp. vi + 564.) Price 25*s.* net.

The Evolution of Man. Essays by G. Elliot Smith, M.A., M.D., Litt.D., D.Sc., F.R.C.P., F.R.S., Oxford University Press, 1924. (Pp. viii + 157, with 19 figures.) Price 8*s.* 6*d.* net.

Big Game and Pigmies. Experiences of a Naturalist in Central African Forests in Quest of the Okapi. By Cuthbert Christy, M.B., C.M. With an Introductory Chapter by Sir Harry H. Johnston, G.C.M.G., K.C.B., D.Sc. London: Macmillan & Co., St. Martin's Street, 1924. (Pp. xxxi + 325, with 120 illustrations and 1 map.) Price 21*s.* net.

Fishes: the Source of Petroleum. By John Muirhead Macfarlane, D.Sc., LL.D., Emeritus Professor of Botany, University Carnegie Foundationer, late Director of the Botanic Garden, University of Pennsylvania. New York: The Macmillan Company, 1923. (Pp. 451.) Price 25*s.* net.

Evolution at the Crossways. By H. Reinheimer. London: The C. W. Daniel Company, Graham House, Tudor Street, E.C.4. (Pp. 191.) Price 6*s*. net.

Growth. By G. R. de Beer, B.A., B.Sc., F.L.S., Fellow of Merton College, Demonstrator in Zoology and Comparative Anatomy in the University of Oxford. London: Edward Arnold & Co., 1924. (Pp. viii + 120, with 7 plates.) Price 7*s*. 6*d*. net.

Biology and Human Welfare. By James Edward Peabody, A.M., and Arthur Ellsworth Hunt, Ph.B. New York: The Macmillan Company, 1924. (Pp. xii + 585.) Price 8*s*. net.

Primeval Man in Central Europe. By Professor Dr. P. Goessler, Director of the State Museum of Antiquities in Stuttgart. Stuttgart: Franckische Verlagshandlung. (Pp. 87, with 40 plates and explanatory text.)

The Home of an Eastern Clan. A Study of the Palaungs of the Shan States. By Mrs. Leslie Milne, F.R.A.I., M.R.A.S. Oxford: at the Clarendon Press, 1924. (Pp. viii + 428, with 20 plates.) Price 16*s*. net.

Social Organisation of the Manchus. A Study of the Manchu Clan Organisation. By S. M. Shirokogoroff, Anthropologist of the Museum of Anthropology and Ethnography of the Russian Academy of Sciences at Petrograd. Shanghai: Royal Asiatic Society (North China Branch), 1924. (Pp. vi + 194.) Price 4 dollars.

Ancient Hunters and their Modern Representatives. By W. J. Sollas, Sc.D., M.A., F.R.S. London: Macmillan & Co., St. Martin's Street, 1924. (Pp. xxxvi + 697, with 368 figures.) Price 25*s*. net.

The Leucocyte in Health and Disease. Being an Inquiry into Certain Phases of Leucocytic Activity. By C. J. Bond, C.M.G., F.R.C.S. London: H. K. Lewis & Co., 1924. (Pp. viii + 84, with 48 figures and 24 plates.) Price 12*s*. 6*d*. net.

The Pigmentary Effector System. A Review of the Physiology of Colour Response. By Lancelot T. Hogben, M.A., D.Sc., F.R.S., Lecturer in Experimental Physiology, The University, Edinburgh. Edinburgh: Oliver & Boyd, Tweeddale Court; and at 23 Paternoster Row, London, 1924. (Pp. xi + 152.) Price 10*s*. 6*d*. net.

The Mongol in our Midst. A Study of Man and his Three Faces. By F. G. Crookshank, M.D. London: Kegan Paul, Trench, Trübner & Co.; New York: E. P. Dutton & Co., 1924. (Pp. 128, with 28 plates.)

Technique of Tissue Culture *in vitro*. By T. S. P. Strangeways, Lecturer in Special Pathology in the University of Cambridge. Cambridge: H. Heffer & Sons, Ltd., 1924. (Pp. xii + 80.) Price 7*s*. 6*d*. net.

Tissue Culture in Relation to Growth and Differentiation. By T. S. P. Strangeways, Lecturer in Special Pathology in the University of Cambridge. Cambridge: H. Heffer & Sons, 1924. (Pp. x + 50.) Price 5*s*.

Heredity and Eugenics. By R. Ruggles Gates, Ph.D., F.L.S. London: Constable & Co., 1923. (Pp. xiii + 288, with 35 illustrations.) Price 21*s*. net.

Automatic Telephone System. By William Aitken, M.I.E.E., A.Am.I.E.E. Volume III, Large Multi-Office Automatic Systems; Semi-Automatic Working; Miscellaneous Systems; Lay-out and Wiring; Power Plant; Traffic. London: Ernest Benn, 8 Bouverie Street, E.C.4, 1924. (Pp. xiv + 339, with 140 figures.) Price 55*s*. net.

Ore Dressing Principles and Practice. By Theodore Simmons. London: McGraw-Hill Publishing Co., 8 Bouverie Street, 1924. (Pp. xvii + 292.) Price 17*s*. 6*d*. net.

Theoretical Metallurgy. By Reginald Scott Dean, B.S., Chief of Metallurgical Department, Hawthorne Works, Western Electrical Company. New York: John Wiley & Sons; London: Chapman & Hall, 1924. (Pp. vii + 246.) Price 15*s.* net.

An Introduction to the Practice of Civil Engineering. By E. E. Mann, M.Sc., Assoc.M.Inst.C.E., Senior Lecturer in Engineering at the University College, Southampton. London: Macmillan & Co., St. Martin's Street, 1924. (Pp. xi + 296.) Price 7*s.* 6*d.* net.

A Handbook of Telephone Circuit Diagrams with Explanations. By John M. Heath. London: McGraw-Hill Publishing Co., 8 Bouverie Street, E.C.4, 1924. (Pp. x + 279.) Price 12*s.* 6*d.* net.

Wireless Possibilities. By A. M. Low. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1924. (Pp. 77, with 4 diagrams.) Price 2*s.* 6*d.* net.

Mines et Torpilles. Par H. Stroh. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel, 1924. (Pp. iv + 183.) Price 6 frs.

Elements of Electrical Design. By Alfred Still. London: McGraw-Hill Publishing Co., 8 Bouverie Street, E.C.4, 1924. (Pp. xxi + 535.) Price 25*s.* net.

The Planning, Erection, and Operation of Modern Open Hearth Steel Works. By Hubert Hermanns. London: Ernest Benn, 8 Bouverie Street, E.C.4. (Pp. viii + 307, with 273 figures.) Price 43*s.* net.

Alternateurs et Moteurs Synchrones. Par Edouard Roth. Tome I. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel, 1924. (Pp. iv + 206.) Price 6 frs.

Arc Welding Handbook. By C. J. Holslag. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1924. (Pp. xi + 250.) Price 10*s.* net.

An Introduction to the Strowger System of Automatic Telephony. By H. H. Harrison, M.I.E.E., M.I.R.S.E. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1924. (Pp. vii + 146, with 161 diagrams.) Price 7*s.* 6*d.* net.

A Long Life's Work. An Autobiography. By Sir Archibald Geikie, O.M., K.C.B., D.C.L., Sc.D., LL.D., Ph.D. London: Macmillan & Co., St. Martin's Street, 1924. (Pp. xii + 426.) Price 18*s.* net.

Crime and Insanity. By W. C. Sullivan, M.D., Medical Superintendent, Broadmoor Criminal Lunatic Asylum. London: Edward Arnold & Co., 1924. (Pp. vii + 259.) Price 12*s.* 6*d.* net.

Spectroscopy. By E. C. Baly, C.B.E., M.Sc., F.R.S., Grant Professor of Inorganic Chemistry in the University of Liverpool, Fellow of University College, London. In two volumes. Vol. I. Third Edition. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1924. (Pp. xi + 298, with 138 figures.) Price 14*s.* net.

Book of Knowledge Acquired Concerning the Cultivation of Gold. The Arabic Text edited with a Translation and Introduction by E. J. Holmyard, M.A., Head of the Science Department, Clifton College. Paris: Paul Geuthner, 13 Rue Jacob, 1923. (Pp. iv + 62.) Price 30 frs.

Introduction to Modern Philosophy. By C. E. M. Joad. Oxford University Press, 1924. (Pp. 112.) Price 2*s.* 6*d.* net.

A History of Magic and Experimental Science during the First Thirteen Centuries of our Era. By Lynn Thorndike, Ph.D., Professor of History in Western Reserve University. London: Macmillan & Co., St. Martin's Street, W.C.2, 1923. (Pp. Vol. I, xl + 835, Vol. II, vi + 1036.) Price 42*s.* net.

The Nature of Intelligence. By L. L. Thurstone, M.E., Ph.D., Professor of Psychology in the Carnegie Institute of Technology. London: Kegan Paul, Trench, Trübner & Co.; New York: Harcourt, Brace & Co., 1924. (Pp. xvi + 167, with 10 illustrations.) Price 10*s.* 6*d.* net.

Kant's Treatment of Causality. By A. C. Ewing, D.Phil. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C., 1924. (Pp. vii + 243.) Price 10*s.* 6*d.* net.

The Mechanism of the Cochlea. A Restatement of the Resonance Theory of Hearing. By George Wilkinson, M.B., F.R.C.S., and Albert A. Gray, M.C., F.R.S.E. London: Macmillan & Co., St. Martin's Street, 1924. (Pp. xx + 253.) Price 12*s.* 6*d.* net.

La Méthode Générale les Sciences Pures et Appliqués. Par André Lamouche. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins. (Pp. xii + 298.) Price 30 frs.

With My Wife Across Africa by Canoe and Caravan. By Colonel J. C. B. Statham, C.M.G., C.B.E., Fellow of the Royal Geographical Society, Fellow of the Royal Anthropological Institute, Fellow of the Zoological Society. London: Simpkin, Marshall, Hamilton, Kent & Co., 4 Stationers' Hall, E.C.4. (Pp. 324, with 55 illustrations and 3 maps.) Price 12*s.* 6*d.* net.

The Greatest Story in the World. The Further Story of the Old World up to the Discovery of the New. By Horace G. Hutchinson. London: John Murray, Albemarle Street, W. (Pp. xiv + 238, with 14 illustrations.) Price 3*s.* 6*d.* net.

A Short Course on Physical Anthropology. By M. R. Drennan, M.A., M.B., F.R.C.S.C., Professor of Anatomy in the University of Cape Town. Cape Town: The South African Electric Printing Co. (Pp. 43.) Price 5*s.* net.

Masters of Science and Invention. By Floyd L. Darrow. London: Chapman & Hall, 11 Henrietta Street, Covent Garden, W.C.2. (Pp. v + 350, illustrations.) Price 10*s.* 6*d.* net.

Les Rois Thaumaturges Etudes sur le caractère surnaturel attribué à la puissance royale particulièrement en France et en Angleterre. Strassbourg: Librairie Istra, 15 Rue des Juifs. London: Oxford University Press, 1924. (Pp. 541.) Price 12*s.* 6*d.* net.

Assurances sur la Vie. Calcul des Primes. Par Henri Galbrun. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins. (Pp. 310.) Price 35 frs.

This volume forms one of a series edited by Professor Emile Borel, dealing with the application of the Theory of Probability to the Economic and Biological Sciences. The standpoint from which the chapters on mortality tables, endowments, annuities, assurances, etc., are developed by M. Galbrun is therefore the theory of probabilities. There is a chapter dealing with interpolation and summation, while the final chapter discusses the close analogy from the actuarial point of view of the law of errors and the chances of death.

Printed in Great Britain by
Hazell, Watson & Viney, Ld., London and Aylesbury.

SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

PURE MATHEMATICS. By F. Purver White, M.A., St. John's College, Cambridge.

An International Mathematical Congress was held in Toronto from August 11 to 16, 1924, under the auspices of the University of Toronto and the Royal Canadian Institute. It was conducted in conformity with the regulations of the International Research Council, which meant, in short, that Germans were not invited. In spite of the distance, some 130 visitors from Europe attended the meeting, but naturally enough they were outnumbered by residents in the American continent, there being some 200 from the United States and 70 from Canada itself. The hospitality of our hosts was most generous, and Prof. J. C. Fields, the Chairman of the Congress, who was largely responsible for its organisation, is to be congratulated on its success. Special weight was laid on the Applied side, and the Congress was thus able to take advantage of the presence in Toronto for the British Association meeting of distinguished men whose interest was in the applications of mathematics to practical problems of science and engineering.

To one whose first experience it was of a learned international gathering, the proceedings were most interesting. To see Prof. Koenigs in the magnificent costume of the Paris *Académie des Sciences* was ample compensation for having to hear him read out an interminable list of delegates from all the unimportant societies in the world, and there was always the pleasing feeling of not knowing what language the next speaker would use. And it was thrilling, though perhaps disappointing, to see in the flesh men with whose names and writings one had long been familiar. But at times one felt isolated and lost in the crowd. At the first session of the Geometry Section Prof. Severi of Rome was unanimously voted into the chair; he thereupon delivered a short harangue in Italian, in the course of which he referred to the contribution of the Anglo-Saxon race to geometry and mentioned in glowing terms the work of Cayley, Sylvester, and Salmon. The only Englishman present endeavoured to raise a cheer, but the

rest of the audience were either not following or thought the occasion too solemn, and the attempt was a dismal failure.

It may perhaps be profitable to give some account of the papers in Pure Mathematics which were down for communication to the Congress, as indicating some of the lines in which mathematical research is active at the present time.

Algebra.—L. E. Dickson gave a lecture entitled, "Outline of the Theory to date of the Arithmetics of Algebras," a theory which has been put upon a satisfactory basis by the work of the lecturer, and which is to be found in his recent book, *Algebras and their Arithmetics.* The chief novelty is a new definition of the integral elements of any associative algebra over the field of rational numbers, it being proved that there always exists a set of integral elements with this definition; whereas under the earlier definitions of Hurwitz and Du Pasquier most algebras fail to possess integral elements. L. E. Dickson also read a paper in which the theory is extended to algebras over any algebraic field and, in particular, quaternions over any quadratic field are examined. Du Pasquier also gave an historical account of the development of the idea of the integers of an algebra.

Determinants.—P. A. MacMahon gave an account of his interesting and curious discoveries about determinants and permanents; one awaits with impatience the appearance of his memoir in the *Cambridge Philosophical Transactions.*

Groups.—The only paper on this subject was by G. A. Miller, who considered the subgroups of the holomorph of an abelian group which have the property that two cycles which are conjugate under the subgroup are necessarily commutative.

Fermat's Last Theorem.—If the equation $x^p + y^p + z^p = 0$ is satisfied by integers x, y, z, p being an odd prime which does not divide xyz, then it has been proved that

$$m^{p-1} \equiv 1 \ (mod\ p^2),$$

for $m = 2, 3, 5, 11, 17$, and if $p \pm 1$ $(mod\ 3)$ also for $m = 7, 13$. H. S. Vandiver, by simpler methods, shows that the relation is true for $m = 7, 13$ with p any oddprime, and he extends the criteria still further.

Differential Equations.—F. H. Murray examines the asymptotic distribution of the characteristic numbers (*Eigenwerte*) of a self adjoint linear partial differential equation of the second order in two variables corresponding to the boundary condition $\frac{du}{du} - \sigma u = 0$. A Green's function is obtained and investigated by means of the theory of integral equations, the method being similar to that used by Weyl for the case of the boundary condition $u = 0$.

Invariants.—Polynomials in the coefficients of a form $f(x_1, x_2 \ldots x_n)$ which have the invariantive property with respect to all linear homogeneous transformations on $x_1, x_2 \ldots x_n$ with integral coefficients taken modulo p, where p is a prime, are called ***formal invariants modulo p*** of f if the coefficients of f are independent variables, but are called ***modular invariants*** if the coefficients of f are integers taken modulo p. The theory of the latter is much the simpler and has been extensively developed by Dickson and others. In particular the finiteness of the totality of concomitants of this type has been proved for any form or system of forms. The concept of formal modular invariants was introduced by Hurwitz in 1903, but comparatively little progress has been made in the general theory and, as far as I know, the theorem of finiteness has not been proved in the general case. O. E. Glenn, however, communicated a paper containing a proof for the binary quantic. The method is one which the author used to obtain a new proof of Gordan's theorem (*Trans. Amer. Math. Soc.*, **20**, 1919), and consists of an extension of the theory of associated forms to concomitants which are functions of the coefficients of the transformation as well as of the coefficients and variables of the form. Another paper on formal modular invariants of forms in several variables was read by W. L. G. Williams.

Functions.—M. Plancherel communicated some theorems on conditions under which series of orthogonal functions converge almost everywhere. Thus, if $\sum_1^\infty \cdot \frac{a^2_n}{2n+1} (log\ n)^{1+\delta}$ converges for $\delta > 0$, then the series of Legendre polynomials $\sum_0^\infty a_n P_n(x)$ converges almost everywhere in the interval $(-1, 1)$. The method is that which has been used by A. Kolmogoroff and G. Seliverstoff for Fourier series.

J. H. Shohat had down for communication a paper on the asymptotic properties of a class of Tchebycheff polynomials at a point where the characteristic function vanishes.

W. B. Ford read a paper on the asymptotic behaviour of the continuation of a power series outside the circle of convergence.

J. I. Hutchinson developed new methods for numerical investigations in connection with the Riemann Zeta function; he has calculated 14 roots in addition to those obtained by Gram.

Calculus of Variations.—A. Razmadzé drew attention to the possibility of problems in the calculus of variations in which there is no continuous curve making the integral $\int_a^b f(x, y, y')\,dx$ a minimum, but there are discontinuous curves with points of discontinuity of the first kind which give a true

minimum to the integral. He developed the theory for extremals with one discontinuity.

G. A. Bliss showed how the transformations used by Clebsch in obtaining a necessary condition for the minimising curve in Lagrange's problem can be derived quite simply by applications of Hilbert's invariant integral and of a formula due to Weierstrass.

L. Tonelli gave an account of the method, expounded in his book *Fondamenti di Calcolo delle Variazioni*, by means of which he seeks to render the calculus independent of theorems of existence of ordinary and partial differential equations. He was led to adopt the point of view of the functional calculus, that is to consider the integrals as functions of lines or surfaces, in the manner of Volterra ; this meant that the theory could not be built up upon the concept of continuity, for which he substitutes the idea of *semicontinuity*, to the examination of which the larger part of his first volume is devoted.

Geometry of the Triangle.—J. H. Weaver gave an account of a system of triangles having a fixed circumcircle and a fixed nine-point circle, and showed that the sides touch a conic having the circumcircle and orthocentre as foci, while the in- and ex-centres lie on a quartic curve which inverts into itself with respect to the nine-point centre.

Polyhedra.—W. D'A. Thompson showed how by means of the plane repeating patterns of the regular polygons the whole series of the five regular (Platonic) and the thirteen semiregular (Archimedean) solids may be built up.

A. H. Wheeler exhibited new " blanks " for the regular solids and for the four-star polyhedra of Poinsot.

Algebraic and projective Geometry.—A. B. Coble investigated the effect of Cremona transformations upon a rational plane sextic curve and the related ten-nodal quartic surface, the symmetroid.

F. Morley developed the process given by Sylvester and explained in Salmon's *Higher Algebra* for determining whether three curves whose equations are given have a common point.

N. B. Maclean gave an account of certain surfaces related covariantly to a given ruled surface and obtained by means of the osculating planes to the two branches of the flecnode curve and of the complex curve.

Differential Geometry.—E. Cartan gave an account of the relation of group theory to recent work on the differential geometry of n dimensions.

J. L. Synge defined $n - 1$ mutually perpendicular normals and $n - 1$ corresponding curvatures for a curve in Riemann space of n dimensions and obtained a set of n Frenet-Serret formulæ.

G. Fubini explained the methods which he has used in his researches on projective differential geometry. Gauss's definition of applicability is extended to congruences and other systems of lines for any group of transformations and in particular for the group of collineations. Some of the theorems found are similar to those of metric geometry, but others are quite new. Many difficult problems arise which still await solution.

ASTRONOMY. By W. M. H. Greaves, M.A., Royal Observatory, Greenwich.

Gravitational Displacement of Light.—An account of the results obtained by the expedition sent from the Dominion Astrophysical Observatory at Victoria to observe the total solar eclipse of Sept. 21, 1922, appears in the Observatory's *publications*, vol. ii, no. 15. The instrument used was a camera of 6 inches aperture and 11 feet focal length. The eclipse was observed at Wallal on the north-west coast of Australia, and the night comparison plates were obtained at Tahiti through the co-operation of the Lick party. Two eclipse plates and three night comparison plates of the same field were obtained. One of the comparison plates was taken film down through the glass, and was used as an intermediary for comparing each of the eclipse plates with each of the other two night plates. Three solutions were made. In the first of these all the stars on the plates were taken into account, in the second solution three stars were omitted, and in the third solution one of these three stars was retained. The values of the Einstein displacement at the sun's limb given by the three solutions are $1''\cdot75$, $1''\cdot42$ and $2''\cdot16$ respectively, the predicted value being $1''\cdot75$. The observations are thus in harmony with the predicted amount, but they are not accurate enough for the purpose of determining the form of the law connecting displacement with distance.

The Form of the Luminosity Function.—F. H. Seares (*Astrophysical Journal*, vol. lix, no. 5, pp. 310–38, June 1924) gives a determination of the Luminosity Function which expresses the relative number of stars of different absolute magnitudes. If we could determine the absolute magnitudes of all stars within a certain region of space we could obtain the Luminosity Function directly, but in order to know a star's absolute magnitude we must determine its distance, and, owing to the fact that parallax observers select their material for high apparent brightness, and large proper motion, the intrinsically faint stars tend to be lost. Consequently, in order to determine the luminosity function, it is necessary to resort to some

statistical process, which automatically eliminates errors arising from selection of material.

The observational data used by Seares is drawn from Luyten's list of stars of large proper motion, and from the Mount Wilson catalogue of 1,646 spectroscopic parallaxes. From these sources a list of 325 stars, nearly all within 50 parsecs of the sun, was prepared. For each star the absolute magnitude $M = m + 5 + 5 \log \pi$, and a quantity H defined as being equal to $m + 5 \log \mu$ was written down (m denotes apparent magnitude, π parallax in seconds of arc, and μ proper motion in seconds of arc per annum). The material was then divided into groups according to values of H and for each group the mean value $\bar{M} - H$ of the quantity $M - H$ was formed. The values of $\bar{M} - H$ so obtained were plotted against the values of H and the resulting graph was found to consist of two straight lines intersecting at $\bar{M} - H = -1 \cdot 1$, $H = 7 \cdot 5$. So that for groups of stars for which $H < 7 \cdot 5$ $\bar{M} - H$ is connected to H by a linear relation, and for groups for which $H > 7 \cdot 5$ the relation is again linear, but is not the same as before. From these relations formulæ can be deduced, giving the geometrical mean parallax of a group of stars of apparent magnitude m and proper motion μ. The formulæ obtained by Seares are ($\bar{\pi}$ denoting geometrical mean parallax):

$$\log \bar{\pi} - \log \mu = -0 \cdot 72 - 0 \cdot 066\, H \; (H < 7 \cdot 5).$$
$$\log \bar{\pi} - \log \mu = -1 \cdot 17 - 0 \cdot 006\, H \; (H > 8 \cdot 0).$$

It will be noticed that for stars for which $H > 8 \cdot 0$ (the *intrinsically* fainter stars), the parallax is correlated mainly with the proper motion, and is practically independent of the apparent magnitude.

From these formulæ Seares deduces the Luminosity Function by the well-known method of Kapteyn (a brief account of which appears in English in *Eddington's Stellar Movements*, pp. 215–218). The value used for the probable deviation of $\log \pi$ from its mean value was $0 \cdot 159$.

A previous determination of the Luminosity Function had been made by Kapteyn and van Rhijn, who obtained a Gaussian frequency curve with a maximum at about $M = 7 \cdot 7$. Their determination was based on an empirical relation, which is practically the same as the first of the two relations given above. Seares's work has now shown that, for the intrinsically fainter stars, this relation must be replaced by another (the second relation quoted above), and so we would expect the luminosity curve of Kapteyn and van Rhijn to be in error for the intrinsically fainter stars. Actually Seares's luminosity curve follows that of Kapteyn and van Rhijn up to about $M = 6$; it then diverges, and the divergence becomes serious

beyond $M = 9$, Seares's curve rising steadily up to $M = 14$. For higher values of M data are not available, and it is impossible to tell whether the curve ultimately descends or not.

Seares's result, as far as it goes, indicates that the number of stars of a given luminosity becomes more and more as we proceed to the intrinsically fainter stars, the number of stars of low luminosity being much more than has hitherto been supposed. Since luminosity is known to be closely related to mass, it suggests that the most frequent mass of the stellar order of size is less than that of any known luminous star.

For the neighbourhood of the sun, Kapteyn and van Rhijn found a total density of 0·0451 stars per cubic parsec. With a Luminosity Function such as that now obtained, total density has no meaning, and we can only determine the number of stars per unit volume brighter than a given limit of absolute magnitude. Seares gives a table showing the results for various limiting values of M. He finds that the total number of stars per cubic parsec brighter than $M = 14{\cdot}86$ is 0·098, and is thus about twice the total for all magnitudes found by Kapteyn and van Rhijn.

The Maxima of Absorption Lines in Stellar Spectra.—A second paper on this subject by R. H. Fowler and E. A. Milne appears in *Monthly Notices, R.A.S.*, vol. lxxxiv, no. 7, pp. 499–515, May 1924. In the previous paper formulæ had been developed connecting the temperature at which a given absorption line in the spectrum should be of greatest intensity with the partial electron pressure in the region of origin, and with certain atomic constants relating to the elements concerned. The necessary constants have now been obtained by A. Fowler for the cases of trebly ionised silicon, and ionised carbon, which are important in the spectra of the hotter stars, and these results are applied by Milne and Fowler to the calculation of the temperatures and pressures in the reversing layers of these stars.

Denoting the partial electron pressure in the reversing layer by P_e, and the temperature there at which a given line attains maximum intensity by T_{max}, it was assumed in the previous paper, that $P_e = 1{\cdot}31 \times 10^{-4}$ atmospheres, this being the value which gave $T_{max} = 10{,}000°$ for the Balmer lines, which are observed to have their maximum in stars of type A0. Using this value of P_e, the temperature in the reversing layers of stars of type B2 comes out to be 16,000° as calculated from the lines of unionised helium and 16,560° as calculated from the lines of ionised carbon. For stars of type B0 to O9, the lines of trebly ionised silicon give a temperature of 26,600°, and the

lines of ionised helium give a temperature of 35,200° for stars of type O5. It should be remembered that the temperatures refer to those regions of the reversing layer in which the lines in question have their origin. The "effective" temperatures which refer to a deeper level should be higher.

The effective temperatures of B0 stars have been determined by spectrophotometric methods, and the values that have been thus obtained lie in the neighbourhood of 15,000°. Fowler and Milne's value for the reversing layer is over 10,000° higher. Now the reversing layer temperatures, as deduced by the above method, depend on the assumed value of P_e, and if P_e were smaller the deduced temperatures would be smaller. Fowler and Milne investigate the possible modifications in their assumed value of P_e, which was derived from assigning a temperature of 10,000° to the A0 stars. They find that P_e should increase with increasing values of surface gravity, and with increasing excitation potential, and should decrease with increasing abundance of the element relative to free electrons, and with increasing atomic absorption coefficient. The discussion gives no basis for suspecting serious errors in the temperatures found, and indeed suggests that some of them should be increased.

The dependence of P_e on the value of surface gravity g is interesting. Fowler and Milne find that P_e should increase with g and accordingly in dwarf stars (for which g is higher) a given line should attain its maximum intensity at a higher temperature than is the case in giants. This would mean that for a given spectral type giants should have lower surface temperatures than dwarfs, and this is what is actually observed.

Fowler and Milne also investigate the values of P_e for principal lines which have their origin at a much higher level than subordinate lines (on which the above values are based), and for which accordingly the values of P_e are much smaller. They also investigate the corrections which must be applied to their previous formulæ when two ionisation potentials are close together.

The outstanding result is the high values of the reversing layer temperatures obtained by Fowler and Milne as compared with the effective temperatures obtained by spectrophotometric observations. Fowler and Milne recall a suggestion of Plaskett's to the effect that for the early type stars the departure from black body distribution is so great that the effective temperature has no relation to the true temperature. They point out that, if this is so, the objections to the high temperatures obtained by them disappear.

The Origin of Stars.—In *Harvard College Observatory Circular,*

no. 257, H. Shapley criticises Jeans's theory of the origin of stars. According to this theory stars are normally created out of spiral nebulæ, and first make their appearance as condensations in the arms of one of these nebulæ. Jeans accepts van Maanen's measures of the apparent angular rotation of the spirals, and is thus led to assume that the bright spirals are less than 10,000 parsecs distant.

Van Maanen has suggested 2,000 parsecs as the distance of Messier 33. Adopting this figure, Shapley finds that it would lead to the conclusion that the absolute magnitudes of the condensations in the spiral are fainter than + 6, indicating a luminosity similar to that of dwarf stars of spectral type K. But, according to the generally accepted ideas of stellar evolution, stars should be born in the giant stage, and their absolute magnitudes at birth should be in the neighbourhood of −2. Shapley's result is, therefore, adverse to Jeans's theory. Similar results are obtained for other bright nebulæ.

Shapley also calls attention to a study of a group of nebulous objects situated near R.A. 12^h, 27^m Dec. + 14°. The integrated photographic magnitudes of twenty objects in this group have been estimated and their mean photographic magnitude is 11·2, the total range in magnitude for the twenty objects being $2{\cdot}7^m$. The average brightness is thus only a magnitude fainter than that of the nebulæ whose internal motions have been measured, and, assuming that they are comparable with the latter, Shapley deduces that any stars of absolute magnitude −2 associated with these nebulæ should be of apparent magnitude 11·5. A study of the stars in this region, however, shows that there is no unusual excess of stars of this magnitude. According to Jeans's theory such an excess would have been expected.

From these and other considerations, Shapley concludes that " if we accept the evidence from the measures of internal motions and radial velocities, with regard to average distances, the direct data of photometry and stellar distribution lend little support to the hypothesis that spiral nebulæ are generating normal low density stars."

The Distance of the Lesser Magellanic Cloud.—A revised estimate of the distance of this cloud is given by H. Shapley in *Harvard Observatory Circular*, no. 255. The method used is the well-known one of comparing the apparent magnitudes of Cepheid variables in the cloud with their absolute magnitudes as deduced from the Luminosity-Period relations. Previous investigations by this method, which were based on Miss Leavitt's preliminary magnitudes, gave a distance of about 10,000 parsecs. Revised magnitudes are now available, and Shapley obtains a distance of 31,000 parsecs with an estimated

probable error of 15 per cent. In obtaining this result, the adopted mean absolute magnitudes of typical Cepheids has not been changed from the values used in previous investigations.

Using this value of the distance, Shapley finds that the average transverse linear diameter of the small cloud is 2,000 parsecs. He also finds (using Wilson's value of + 170 $^{km}/_{sec}$, for the velocity in the line of light) that the cloud was in or near the Milky Way $1{\cdot}9 \times 10^8$ years ago. He also finds that the brightest stars in the clouds exceed absolute photographic magnitude $-5{\cdot}5$. For these stars Harvard spectrum plates show a predominance of late types, and the visual absolute magnitudes must occasionally exceed $-7{\cdot}0$. They are accordingly the brightest stars yet recorded. Shapley estimates that the linear diameters of the brightest variables in the cloud are of the order of 10^8 kilometres.

METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

Prof. S. Chapman discusses the lunar atmospheric tide at Mauritius and Tiflis (Trans-Caucasia) in the *Quarterly Journal of the Royal Meteorological Society* for April 1924. This paper is one of a series dealing with the atmospheric tide at Greenwich, Batavia, Hong-Kong, and Aberdeen. He finds the amplitude of the semi-diurnal component of the tide to be 51 microbars at Mauritius and 27 microbars at Tiflis. In the case of Mauritius the maximum pressure precedes the moon by 17 minutes but at Tiflis it is the other way about, for there is a lag of 52 minutes. In addition to these and other interesting facts about the atmospheric tide at these two places the paper contains valuable advice about the best general methods of working up barometric records in researches of this kind. The same journal for July 1924 contains a second paper by the same writer on the semi-diurnal oscillation of the atmosphere. Beginning with a summary of the earlier researches of well-known mathematical physicists, in particular those of Kelvin, Margules, and Lamb, Prof. Chapman describes some of his own researches into the oscillations that are probably produced by world-wide variations of temperature and by the tidal action of the moon. These researches are of a mathematical kind, and the paucity of observations of pressure and temperature in the free air unfortunately prevents any verification of the deductions made. Prof. Chapman does not consider that there is reliable evidence of a variation of the semi-diurnal oscillation of the atmosphere with height in the free atmosphere, such as Hann deduced from the observations of mountain stations, for he considers that the existence of any such varia-

tion could be proved only by analysis of observations made in the free air.

The daily oscillations of the atmosphere at certain Scottish stations are dealt with in another interesting paper, by Dr. A. Crichton Mitchell, entitled, "On the Diurnal Variation of Atmospheric Pressure at Eskdalemuir and Castle O'er, Dumfries-shire," which appears in *The Quarterly Journal of the Royal Meteorological Society* for April 1924. The writer compares the diurnal inequalities of pressure obtained at these two stations, which are slightly less than 10 kilometres apart, at altitudes of 242 and 180 metres respectively, and have not widely dissimilar exposures, both being in the valley of the Esk. The periods analysed are 1911–20 for Eskdalemuir and 1902–08 for Castle O'er, the discussion of the Castle O'er figures having been made by Dr. C. Chree some years ago (*Q.J. Met. Soc.*, **37**, 1911). The present comparison yields some considerable divergencies, the figures for Eskdalemuir being in nearly all respects similar to those obtained by averaging the harmonic elements found by Angot in 1887 for stations between 50° and 60° N. Dr. Mitchell attributes the divergencies to faulty compensation of the Richard barograph used at Castle O'er, and considers that the Castle O'er figures are not representative of that area. Dr. Chree points out, however (*Q.J. Met. Soc.*, July 1924), that the discrepancies are not of the simple kind that might have been expected if incomplete compensation for temperature were their main cause, and gives some figures showing that widely different harmonic elements may occur in different years at one station. The complexity of this subject is made very evident in the course of these discussions, which indicate that a great deal of further research might usefully be made.

Dr. Anders Ångström's work on radiation is well known. In a report to the International Committee for Solar Research on "Actinometric Investigations of Solar and Atmospheric Radiation," a summary of which he has forwarded to the Royal Meteorological Society (*Q.J. Met. Soc.*, April 1924), some statistics of fundamental importance in the physics of meteorology are given. The figures given are based on over a year's observations made with his pyranometer[1] at Stockholm. An outstanding result is the important contribution to the total incoming radiation in high latitudes made by diffused radiation from the sky, as is shown in the table on the following page.

Angström devotes particular attention to the relation between the total incoming radiation on a particular day and the sunshine, the sunshine being expressed as a percentage

[1] Described in *Monthly Weather Review*, Washington, 47, 1919, p. 795.

RADIATION INCOME AT STOCKHOLM, JULY 1922—JUNE 1923

(Gram-calories per square centimetre)

Month and year.	Total income for the day. Mean.	Highest for a day.	Lowest for a day.	Aggregate recorded radiation from sun and sky.	Sun radiation alone, computed from time of sunshine.	Sky radiation alone.	Duration of sunshine, per cent. of possible, 1922–3.	Average per cent. duration of sunshine, 1908–20.
1922								
July	362	607	45	11,221	8,270	2,951	47	48
August	287	—	—	8,890	5,630	3,260	40	42
September	201	405 *	48 *	6,035	3,310	2,725	38	42
October	114	216	31	3,537	1,630	1,907	38	27
November	47	130	9	1,400	324	1,076	20	14
December	21	76	3	662	17	645	26	7
1923								
January	27	76	2	851	180	671	15	15
February	79	220	22	2,203	576	1,627	19	26
March	143	335	23	4,422	2,560	1,862	33	34
April	308	582	41	9,231	5,980	3,251	47	44
May	323	616	86	10,015	6,690	3,325	39	55
June	327	653	—	9,810	6,420	3,390	35	51

* These refer to 1923.

of the maximum possible. From the figures for Stockholm he arrives at the following empirical formula :

$$Q_s = Q_o\,(0{\cdot}25 + 0{\cdot}75S).$$

where Q_s = total radiation income during the day.
Q_o = total for a perfectly clear day.
and S = sunshine (ratio of actual to max. possible).

This formula makes it possible to determine the total incoming radiation at places which are not equipped with a pyranometer provided there is a record of the sunshine. Almost exactly the same constants were found for the records obtained by Kimball at Washington.

In the figures for individual days obtained at Stockholm the influence of convection was clearly shown by the presence of a minimum between noon and 4 p.m. on days of abundant sunshine, due doubtless to the formation of cumulus clouds, and of a minimum between the same hours on cloudy days due to the tendency for convection to break up uniform cloud-sheets. The nocturnal radiation sent out by a horizontal totally absorbing surface towards the sky was also investigated and found to depend chiefly on the temperature of the surface itself and on the absolute humidity of the air near the surface ; the latter is to some extent a measure of the moisture content of the atmosphere, while the former is with the arrangement usually adopted simply the temperature of the surrounding air.

We may write $R = F(p, t)$
where R = nocturnal radiation
F = a function of the absolute humidity (p) and of the temperature (t)

The radiation of the atmosphere (A) is, of course, given by the difference between the radiation which should be sent out according to the Stefan-Boltzmann formula and the radiation R observed

so that $A = \sigma T^4 - F(p, t)$,
where T is the absolute temperature.

Ångström has attempted to determine the function $F(p, t)$ by numerous observations of R made in Algeria, California, and in northern Sweden; also in balloon ascents at Lindenberg, near Berlin, and by observations of absorption by the lower layers of the air together with measurements of the radiation received from above and below at various points in the atmosphere.

He finds that:

(1) For a group of long waves, constituting about 25 per cent. of the radiation emitted by a perfectly "black" surface, the atmosphere, even when moist, is perfectly transparent. The energy of this group for temperatures between + 30° C. and − 30°C. is roughly proportional to the fourth power of the temperature.

(2) There is another group of waves constituting about 50 per cent. of the radiation emitted by a perfectly "black" surface which is totally absorbed by quite a thin layer of the atmosphere (30 metres is very nearly sufficient when the water vapour present has a pressure of the order of 10 mm. of mercury); the absorption is probably due to water-vapour.

(3) A third group, constituting about 25 per cent. of the "black body" radiation, is absorbed to a very variable degree, chiefly by the water vapour in the atmosphere. In the case of (3) and (2) just as for (1) the energy is roughly proportional to the fourth power of the absolute temperature between + 30° and − 30° C.

(4) The atmosphere at levels between 4,400 and 5,200 metres radiates about 50 per cent. of the radiation of a black surface at the same temperature, as would be expected from (2).

(5) From (2) and the distribution of water vapour we may surmise that at higher levels, up to 10,000 or 15,000 metres, the atmosphere also radiates 50 per cent. of the radiation of a black surface at the same temperature.

Ocean Currents and Wind.—In the same journal (*Q.J. Met. Soc.*, April 1924) C. S. Durst makes an attempt at verifying Ekman's theory of ocean currents [1] by means of information

[1] Ekman, V. W., "On the Influence of the Earth's Rotation on Ocean Currents," *Ark. Matem.*, Stockholm, 2, 1905, No. 11.

about the currents in the open ocean obtained by observing the differences between ships' true positions at noon and the positions obtained by "dead reckoning," due allowance being made for leeway, etc., these differences enabling the mean current for the twenty-four hours between successive noon observations to be calculated; the winds corresponding with these currents were obtained from the four-hourly estimates made as part of the ships' routine.

The observations for various tracts of ocean were analysed separately; an immediate result was to show that, in the northern hemisphere the current tends to set to the right of an observer standing with his back to the wind, as would be expected. For the southern hemisphere the rule was found to be reversed.

Comparing the figures for current and wind velocity, grouped according to latitude, Durst finds a tendency for proportionality; certainly the stronger winds produced the stronger currents. Calculations of the co-efficient of turbulence[1] outside latitudes 20° N. and S. showed that this coefficient varied to a moderate extent only. This being so, it was found possible to combine the means for different latitudes outside those limits, and, after multiplying the mean current velocities by $\sqrt{\sin \phi}$, figures were arrived at which showed approximate proportionality between the current and the wind. From this, using Jeffreys' formula, it follows that the coefficient of turbulence varies as the square of the wind velocity.

Separate treatment of the currents and winds in the tropics showed that the currents are out of proportion to the Trade winds. This, it is concluded, is either due to a definite tilting of the ocean's surface or to the surface current extending down to regions of small turbulence.

The 27-Day Period in Terrestrial Magnetic Disturbance.—A paper by the Rev. A. L. Cortie with this title appears in *Proc. Royal Soc.*, Series A, vol. 106, No. A 735. The paper is an account of some observations which suggest strongly that the 27-day period in terrestrial magnetic disturbances is connected with a restricted area of solar disturbance and not with a periodic disturbance of the whole solar surface.

The observations were made during a long period with comparatively calm conditions on the sun, from May 1921 to July 1923. During this time the few sunspots that occurred were almost confined to one-half of the sun's disc, and were arranged around two well-marked centres. There were two very long sequences of terrestrial magnetic disturbance at approximately 27-day intervals; from the exact length of

[1] This constant was defined by H. Jeffreys when corroborating Ekman's theory (Jeffreys, H., "On Turbulence in the Ocean," *Phil. Mag.*, 39, 1920, p. 578).

these intervals the solar latitudes of the assumed sources of disturbance on the sun were derived, while the solar longitudes were obtained by noting the longitudes of the sun's central meridian on the days of magnetic disturbance. The two positions on the sun obtained in this way from the terrestrial magnetic phenomena, one in the southern and the other in the northern hemisphere of the sun, were found to fit the two regions of observed solar disturbance. The most marked magnetic disturbances, however, did not correspond with the periods of greatest sunspot and floccular activity. The magnetic disturbances persisted after the sunspots had disappeared, when only flocculi and faculæ remained to show the activity of the region.

Prediction of Oceanic Swell in Morocco.—Montagne in *Annales Hydrographiques*, 3me série, No. 709, 1922 (pp. 157–83), describes the work of the " Service de Prédiction de la Houle " at Rabat, a service which was organised in July 1921. It is not proposed to describe in detail the aims and work of this institution. Much of the pioneer work was carried out by Gain (*Ann. du Serv. Hydr.*, 1918, pp. 66–8), who examined synoptic weather-charts for Western Europe for the period 1915–18 and observed the state of the sea at Casablanca during the same period. He arrived at a number of empirical rules connecting the occurrence of swell at Casablanca with the general meteorological situation. These rules may be summarised by saying that swell reaches Morocco when there is a deep depression suitably situated out on the Atlantic provided certain other conditions are also fulfilled. For example, the depression must not move too rapidly towards the East, nor must it follow a track to the north of Iceland, nor south-eastwards between Ireland and Norway; the interposition of an anticyclone between the depression and Morocco also prevents the arrival of swell. Montagne, in the present paper, extends the rules of Gain; he used observations of swell from a larger area, including the Azores and Portugal, and studied the differences as regards liability to swell from various directions shown by particular stretches of the Moroccan coast.

It is impossible in a short space to deal with all the points touched on by Montagne. One notes that swell from W. and N.W. is the most dangerous and generally reaches all parts of the Moroccan coast almost simultaneously. The time which elapses between the appearance of a swell-producing depression and the arrival of the swell at Morocco is generally between two and four days. In the later part of the paper Montagne discusses a method of prediction which is based upon the observation that the " period " of swell generally increases in length previous to an increase in its intensity. The " period," it may be noted, varied from 10 to 18 seconds, or, rarely, a little

longer, the longest swell originating generally at the greatest distance. The conclusion is reached that, whereas observations of swell by ships out on the Atlantic would be useful for forecasting purposes, exactly what phenomena should be observed is not yet certain.

Wind Wave and Swell on the North Atlantic Ocean.—A summary of a paper by Dr. Vaughan Cornish bearing this title, delivered to Section E of the British Association at Toronto on August 8, 1924, appears in *Nature* (September 13, 1924, p. 394). Cornish made his observations on a voyage from Southampton to Trinidad and back. Waves were nearly always observed to travel with a speed less than that of the wind, but in those cases where there was a swell running in the same direction as the waves the difference was very slight. There was abundant evidence that a crossing swell hindered the development of waves and reduced their velocity, especially when the swell was meeting the waves. The direction of breakers was found to be intermediate between those of swell and wave (the breaker being formed when they override), so that the practice of estimating the direction of the wind by observing the direction of the "curl" on the water gives erroneous results whenever there is a cross-swell; the general run of the waves, however, gives a trustworthy result.

Thermal Stratification in the Atmosphere.—A long paper on this subject by W. Peppler appears in *Beiträge zur Physik der freien Atmosphäre*, Band xi, Heft 3, 1924, pp. 79–95. This paper is largely statistical, being an analysis of nearly seven thousand sets of meteorological observations obtained during captive balloon ascents at Lindenberg (Berlin) from 1910 to 1916. The number of observations is over a thousand for all levels below 4,000 metres. Of the various statistical tables the second is perhaps the most interesting, as it shows the frequency with which inversions of the normal vertical decrease of temperature, and isothermal layers, occur between various limits of height up to 5,300 metres. The principal maximum is in the layer 122 to 310 metres above the ground, but there are pronounced secondary maxima at 1,310–1,500, 2,310 to 2,500, 3,910–4,100 and 4,310–4,500 metres. Most inversions, except those near the ground, were found to be smaller than 3° C., while those exceeding or equal to 6° C. were very rare and were practically confined to the autumn and winter. As regards thickness, layers of "inversion" and of constant temperature were found to have an average thickness varying between 193 metres and 225 metres for heights less than 1,900 metres. Thicknesses of more than 1,000 metres sometimes occurred, but were very rare; they were, with only one exception, confined to the autumn and winter. When inversions of at least

5° C. were considered, 63 out of 75 were found to be of the type which appears above stratus and strato-cumulus clouds, and is associated in most cases with extremely low relative humidity. Peppler discusses the causes of inversions. Apart from those near the ground, which are caused by loss of heat from the surface by simple radiation, the most fruitful cause appears to be the dynamical warming of air which sinks towards the ground, especially in the neighbourhood of anticyclones. Such descending movements rarely approach nearer than within 1,500 metres of the ground, and are undoubtedly responsible for the important maximum in the frequency at 1,310–1,500 metres, which has already been mentioned. A multiple stratification is not uncommon, and is doubtless caused by irregular interpenetration of cold and warm air currents.

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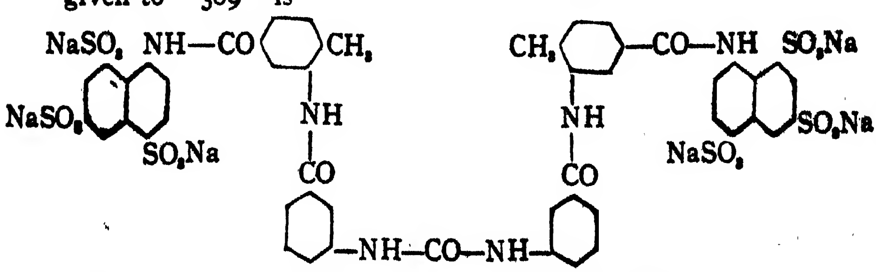
BIOCHEMISTRY. By R. KEITH CANNAN, M.Sc., University College, London.

THE scientific occasion of the summer has been, undoubtedly, the meeting of the British Association at Toronto during August. To biological and medical science the event was of particular significance. On the one hand, the opportunity was presented of felicitating the Nobel Laureates, Prof. MacLeod and Dr. Banting, in the town and university where the dramatic work on insulin, which earned for them the applause of the world, was accomplished. On the other hand, it was the privilege of experimental medicine to provide this year, in the person of Sir David Bruce, the President of the Association.

It is not our intention to review again the progress in the understanding of the many problems raised by the experimental

use of insulin. The work was surveyed a year ago, and, it must be confessed, the further progress is likely to be more imperative of attention a year hence than it is to-day. A subject of real interest is, however, suggested by the Presidential Address of Dr. H. H. Dale to the section of Physiology. Dr. Dale reviewed the position and prospects in Chemotherapy (*Chemistry and Industry*, 1924, **43**, 931), pointing out that the most promisihg results in this field were in those diseases caused by parasitic trypanosomes—diseases of which our knowledge is in large measure due to Sir David Bruce.

Chemotherapy.—Chemotherapy may be regarded as the outgrowth of the study of the natural antibodies. When it came to be realised that to certain forms of infection the body was unable to reply by the production of efficient antibodies the thought arose in the mind of Ehrlich to supplement the deficiencies of nature by the products of the synthetic chemist; to produce in the laboratory substances specifically toxic to the parasite but innocuous to the tissues of the host. The audacity of the project is sufficiently revealed by the confession of our almost complete ignorance of wherein lies the specific chemical distinction between the protoplasm of the parasite and of its host. Attracted first by the specific staining properties of many dyes, Ehrlich found a benzidine dye, Trypan Red, which gave promise as a therapeutic agent in attacking the trypanosome. A related toluidine dye named Trypan Blue was later shown to be still more discriminating. More recent work has added new and more valuable drugs at the same time as it has rendered the nature of their action the more perplexing. In 1920 Handel and Joetten published observations on the therapeutic use of "Bayer 205"—a substance whose identity, if not its general nature, was concealed under patent rights by the famous German chemical firm. Quite recently, however, Fourneau and his associates (*Ann. Inst. Pasteur*, 1924, **38**, 81) have synthesised a compound, which has been labelled "309," and which appears to be either identical with or to be a substitute of equal value as the product of Bayer. The structure given to "309" is



Fourneau's "309" (possibly identical with "Bayer 205").

The ratio between the organotoxic and the parasitoxic dose is reported to be, in the case of infected mice, more favourable than 100. Cases are recorded of cures of sleeping sickness in human beings by its aid. A further startling development has been that due to Levaditi and Nicolau (*Ann. Inst. Pasteur*, 1924, **38**, 179, and earlier numbers) that certain bismuth preparations and even bismuth suboxide and finely divided metallic bismuth yield, on incubation with cell-free liver extracts, substances with specific toxicity towards spirochaetes *in vivo*. The successful treatment of some human syphilitic is reported (Fournier, *Ann. Inst. Pasteur*, 1924, **38**, 40). Levaditi and Nicolau consider that the bismuth compound unites with a constituent of the tissue extract which they call "bismogène" to produce the active agent "bismoxyl." Certain other observations of recent date are significant in this connection. "Bayer 205," on standing in serum for an hour, becomes so combined with the proteins as to be retained on ultrafiltration and precipitated on coagulation of the proteins whilst it will, as will also Trypan Blue, prevent the normal precipitation of the serum proteins by such agents as mercury salts or tannin (Dale, *loc. cit.*). Again Bayer 205 (Steppuhn and Brychonenko, *Biochem. Z.* 1923, **140**, 1) and Trypan Blue as well as certain other structurally unrelated dyes (writer's unpublished observations) will inhibit the clotting of blood in sub-toxic doses both *in vivo* and *in vitro*.

These facts but emphasise a special function of the tissues of the host in the activity of the drug—a function more implicit in the following observations. Neither the substances discussed nor the familiar arsenical preparations (salvarsan, atoxyl, tryparsimide) have a visible toxic action upon trypanosomes or spirochaetes *in vitro*, though cultures so treated fail to infect animals into which they are subsequently injected. Again, the curative value of a particular preparation in the case of the same infection varies notably with the species of the host.

Many years ago Ehrlich showed that the reduction of atoxyl yielded a substance—an arsenoxide—extremely toxic to trypanosomes. Similarly the partial oxidation of salvarsan gives a highly toxic arsenoxide. It is reasonable to infer that the efficiency of these drugs lies in the respective reduction or oxidation by the body at a rate favourable to the elimination of the parasite without prejudice to the tissues of the host. Voegtlin (*Pub. Health Rep. U.S. Pub. Health Serv.*, 1923, **38**, 1882) gives this view more substance by the observation that derivatives of arsenious oxide combine readily with substances containing the sulphydryl group, whereby their toxicity is much reduced. It is a natural suggestion that such a reaction

with possible respiratory sulphydryl compounds, such as glutathione, may explain the reaction of these drugs by the consequent disturbance of the oxidation-reduction equilibria of the protoplasm. A similar affinity remains to be demonstrated in the case of the therapeutic dyes. It certainly appears to the writer significant that the only obvious property which these various agents have in common is a certain ease of reversible oxidation and reduction. If they should be amenable to our technique for the investigation of oxidation-reduction potentials data might accrue to establish the view of Voegtlin in more concrete and more embracing terms.

The Chemistry of Immune Specificity.—A survey of the problems of chemotherapy lead inevitably back to a demand for some knowledge of the chemical nature of the reactions specific to particular tissues and notably to the chemistry of the problems of specificity encountered in Immunology. It is particularly happy, therefore, that we should be able to record an important series of papers (Avery and Heidelberger, *J. Exper. Med.*, 1923, **38**, 73, and 1924, **40**, 301), wherein the "soluble specific substance of Dochez and Avery (*J. Exper. Med.*, 1917, **26**, 477) has been submitted to a very full examination. This substance has been isolated from the filtrates of cultures of pneumococcus, and yields precipitates with the immune sera of the homologous type of pneumococcus in dilutions up to 1 in 3,000,000. The serological specificity of the various types of pneumococcus is probably dependent entirely on this substance, so that it is highly satisfactory that gross chemical differences are to be found between the soluble specific substance isolated from cultures of Type II and that from cultures of Type III. Both consist, in their present state of purification, of about 0·2 per cent. of nitrogen, 75 per cent. of a polysaccharide, and 25 per cent. of unidentified structure. However, the Type II substance is dextro-rotatory, the Type III substance lævo-rotatory; the former, if the salt of an acid is the salt of a weak soluble acid, the latter is the salt of a very strong, sparingly soluble acid. The carbohydrate moiety of the substance of Type II is built up of glucose units whereas that of Type III consists of glucuronic acid or an allied sugar acid. Assuming the two substances to be chemical individuals the authors represent their specific characters by the formulæ.

Type II	Type III
$R_1\left\langle\begin{matrix}CH_2OH\\COOH\end{matrix}\right.$	$R_2\left\langle\begin{matrix}COOH\\COOH\end{matrix}\right.$

Biochemists will appreciate the attractive possibilities when it is recorded that the specific soluble substances are stable to

heat, to acids, and to the action of enzymes. The criticism that the material isolated is merely inert material containing the specific substances appears to have been well met.

The same authors (*J. Exper. Med.*, 1923, **38**, 81) have shown that pneumococcus contains a second specific agent, separable and chemically distinct from that discussed above and having different immunological properties. This substance is a protein and is both serologically and antigenically less specific than is the bacterial cell. Its nature is at present under investigation. Whereas the polysaccharide substance is specific to the type of pneumococcus, the protein substance appears to be specific only to the species. Finally, it should be emphasised that although the soluble specific substance is specifically reactive with antibody produced in response to immunisation with the intact organism, yet the substance, when dissociated from the other constituents of the cell, is but slightly, if at all, capable of producing antibody formation.

The Isolation of the Vitamins.—Although the earlier work on vitamins raised fears that they were likely to prove as elusive as the specific agents of the bacterial cell, a very few years of active work has brought us almost in sight of the isolation and chemical definition of several of them. The growth-promoting and anti-rachitic factors are found most richly in cod-liver oil, which has consequently been the source from which their isolation has been attempted. The first step was made when Steenbock and Boutwell (*J. Biol. Chem.* 1920, **42**, 121) and Drummond and Coward (*Lancet*, 1921, **2**, 698) found that after saponification of the oil the whole of the potency remained concentrated in the unsaponifiable matter. A concentration of the vitamin one hundredfold was thus effected in one step. Removal of cholesterol from this material reduces the bulk by one-half but leaves the activity unimpaired. Drummond's (*Chemistry and Industry*, 1924, **43**, 928) latest report was delivered at the British Association and described how fractionation of the oil with superheated steam followed by a distillation in a high vacuum yielded four fractions one only of which was active. This fraction, which boiled at 350° at 2 mm. pressure, consisted essentially of an unsaturated alcohol with a molecular weight of 350 or thereabout. Whether the vitamin is to be identified with this alcohol or with some lesser constituent of the fraction remains to be determined. It is interesting that the activity survives such drastic treatment as the acetylation, benzoylation, and hydrogenation of the alcohol, but is susceptible to oxidation.

Vitamin B has been always a more popular member to seek since the early work of Funk. The latest and most notable recruit to the problem is Levene (*J. Biol. Chem.*, 1924, **61**, 429),

who, with van der Hoeven, surveys in a preliminary paper methods of concentrating yeast extracts as the first step in the isolation of the active factor. Even so the solutions obtained are of astonishing potency comparable to the activity of the crystalline preparation reported by Seidell. The latter worker has obtained a crystalline picrate which analyses as $C_6H_{13}O_2N_3OH . C_6H_2(NO_2)_3$ and whose prophylactic dose for young pigeons is about 2 mgm. every other day. Levene believes that when vitamin B does really submit to purification its potency will be found to be much greater than this.

Eddy, Kerr, and Williams (*Proc. Soc. Exp. Biol. and Med.*, 1924, **21**, 307) have prepared in small quantity a crystalline substance which they regard as the substance called "bios" by Wildiers. The proof of the identity waits upon the preparation of the substance in larger quantity.

The search for the antiscorbutic factor or vitamin C continues to be prosecuted diligently by Zilva, who has obtained very active concentrates of decitrated lemon-juice (*Biochem. J.*, 1923, **17**, 410 and earlier papers), which, after the removal of invert sugar by fermentation, contain chiefly nitrogenous substances (*Biochem. J.*, 1924, **18**, 182, 186), although the total nitrogen present is only 0·004–0·014 per cent.

PHYSICAL CHEMISTRY. By W. E. Garner, M.Sc., University College, London.

Active Molecules.—The conception of active molecules put forward by Arrhenius to account for the speed of chemical reactions has been very fruitful in initiating experimental work and theoretical speculation. The precise nature of the activity in thermal reactions is, however, still uncertain, it being doubtful whether chemical reactivity be due to molecules possessing high internal or high kinetic energies. In photochemical processes, it seems clear that reaction proceeds between molecules which, in absorbing radiation, undergo some intermolecular change, but the radiation theory advocated by Lewis and Perrin, according to which thermal reactions are activated by radiant energy, is not supported by recent work. Even the existence of the "true" monomolecular reaction, which was the foundation-stone of the radiation theory, is in doubt, the decomposition of phosphine having been shown by Hinshelwood (*J. Chem. Soc.*, 1924, **125**, 393) to be a surface reaction at least up to 1,044° Abs.

Chemical activity is increased, not only by heat but also by the action of light, slow-moving electrons, α-particles, β-particles, γ-rays, positive rays, etc., the different agents not always bringing about the same change in a given chemical system. The same active molecular states may, however, be

produced by different agents as shown by experiments on the production of molecular resonance and ionisation, by means of radiant energy and by electron collisions.

Several investigators have suggested that the same active states are produced in corresponding thermal and photochemical reactions, the molecular activation proceeding by collision in the one and by absorption of radiation in the other. For such reactions, the values of A, in the Arrhenius equation, $d \log k/dT = A/RT^2$, should be the same as those deduced from the absorption spectra of the compounds undergoing the chemical change. Thus, Rideal and Norrish (*J. Chem. Soc.*, 1923, **123**, 3216), from measurements of the temperature coefficients of chemical reactions undergone by sulphur, deduce that the values of A are simple multiples of 12,500 calories. It is suggested that the activation consists in setting free one or more chemical bonds in sulphur, either by collision or by the absorption of radiation, which processes require some multiple of the above amount of energy. On this view, activation consists in the rupture of chemical linkings, either by means of light or collision. It is doubtful, however, if the disruption of the chemical bond precedes chemical action in all cases, especially in organic compounds where the formation of intermediate compounds and their subsequent decomposition very probably occur by the gradual transference of valencies from one atom to another.

Light is thrown on the nature of the molecular states by spectroscopic measurements, although no distinction can yet be made as to those active in chemical reaction. V. Henri and H. de Laslo (*Proc. Roy. Soc.*, 1924, A, **105**, 662) working on the ultraviolet absorption band-spectra of a number of organic substances, have shown that these fall into three classes: (*a*) groups of band series with fine structure, (*b*) groups of band series with no fine structure, 5–10 Å wide, and (*c*) continuous absorptions over regions 100–300 Å wide. In the first class, it is suggested that (1) the electronic movements, (2) the vibratory energy of the component atoms, and (3) the rotational energy of the molecule as a whole are all quantised, thus giving a large number of molecular states. In the second class, only (1) and (2) are quantised and in (*c*) only (1). If a number of these molecular states are chemically active, then the critical increment for thermal reactions is very probably an average value for the difference between the energy contents of a number of molecular states, and the normal state.

Henri has found evidence of abrupt molecular change in the structure of molecules, the significance of which is not yet clear. The absorption spectrum of a molecule evolves in type from (*a*) through (*b*) to (*c*) with increase in the frequency

of the absorbed light, and he suggests there is a connection between the transition and intramolecular changes in structure. For naphthalene, absorption of type (*a*) was observed in the region 3,200–2,820 Å and one of type (*b*) in the region 2,820–2,500 Å. It is concluded that the sharp change in the spectrum is associated with an abrupt increase in the internal energy of the molecule of more than 101,000 cal./mol.

Evidence of the occurrence of active states is afforded by a study of the light emitted during physical and chemical processes. For example, radiation is emitted in the combustion of carbon monoxide and hydrogen, in the condensation of mercury vapour, and during the activation of molecules by slow electrons. From the wave-length of the light emitted deductions with respect to the critical increment of the active molecules may be made. Thus, H. A. Taylor and W. C. McLewis (*J.A.C.S.*, 1924, **46**, 1606), from a study of the absorption and fluorescent spectra of anthracene, have deduced that the fluorescence in the anthracene-dianthracene system is due to the return of the activated molecule to the normal state. The energy change is 61,300 cal/mol of fluorescing substance. But the majority of chemical changes take place without the emission of light, and the physical processes of crystallisation, evaporation, electrolysis, which involves the discharge of ions, etc., are similar in this respect. Also an electron may be absorbed from infinity by an atom with the occurrence of light emission.

The existence of active states which do not emit radiation under normal conditions has been demonstrated by Franck and Einsporn (*Z. Physik.*, 1920, **2**, 18), who determined critical potentials for mercury vapour (see also Takamine and Fukuda, *Nature*, 1924, **114**, 382). These states may be defined as those whose period of stability, τ, is considerably greater than the average interval between the collisions. Thus, they only return to the normal or other states on collision with other atoms, their energy being dissipated as kinetic energy. Such states must be produced during the absorption of radiation by a non-fluorescing or phosphorescing substance.

It is the non-radiative character of the active molecules in thermal processes which makes their examination so difficult. It is possible that radiation is emitted in all physical and chemical change when carried out at a sufficiently high dilution, but that in solution and in gases at high concentrations, the period of activity is large compared with the interval between the molecular collisions. This would indicate that the occurrence of radiation in certain gaseous and other reactions is an accident due to lack of stability of the active state.

An interesting example of the deactivation of a molecule

by collision is given by the experiments of Franck and Cario (*Z. Physik.*, 1922, **9**, 259); who showed that hydrogen molecules could be dissociated on collision with mercury atoms activated by exposure to light of wave-length 2,536Å ($2p_2$ state). Duffendack and Compton (*Physical Rev.*, 1924, **23**, 583) have confirmed this by experiments in a low voltage arc; and also claim that nitrogen is similarly dissociated by mercury atoms in the $2P$ state. Compton and Turner (*Phil. Mag.*, 1924, vi, 28, 360) conclude that the activated mercury atoms on collision with a hydrogen molecule gives rise to a hydrogen atom and mercury hydride which then dissociates into the atoms. The formation of transitory chemical compounds of this character in positive ray tubes has been shown by Sir J. J. Thomson (*Rays of Positive Electricity*, 1921). Recently, several investigators, from a study of the band spectra emitted from mined vapours and gases, have been led to assume the formation of similar compounds. O. Oldenburg (*Z. Physik*, 1924, **25**, 136) has observed fluorescence from a compound of nitrogen and iodine, and Barratt (*Proc. Roy. Soc.*, 1924, A, **105**, 221) and Newman (*Phil. Mag.*, 1924, **48**, 159) have found band spectra which they ascribe to the association of the atoms of sodium and potassium.

Phenomena in the Liquid State.—The specific heat temperature curves of benzene, ethyl benzene, and carbon tetrachloride have been investigated by J. W. Williams and F. Daniells (*J.A.C.S.*, 1924, **46**, 903, 1569) and slight irregularities have been observed (maxima, etc.), which indicate molecular changes in the liquids with rise in temperature. These irregularities are not so marked when water is present, the water catalysing the change from one molecular species to another. These phenomena recall the work of Baker on dried liquids (cf. SCIENCE PROGRESS, 1923, **67**, 357). Smits (*J. Chem. Soc.*, 1924, **125**, 1068) claims to have shown, by means of fractional distillation, that a fixed equilibrium is set up by the drying process. The existence of crystal nuclei in such liquids appears to have been established by Eastman (*J.A.C.S.*, 1924, **46**, 917). He has shown that X-ray diffraction patterns may be obtained from liquid benzene, lines being obtained corresponding with those given by the crystals.

Dehydration of Salt Hydrates.—Faraday noted that perfect crystals of certain hydrates, e.g. sodium carbonate, did not effloresce, but that the dehydration took place rapidly if they were scratched or broken. Similar phenomena have since been found to occur in many heterogeneous processes. The dissociation of a solid into a gas and another solid may take place in one of two ways: (1) the original substance and its solid product may commingle so as to give a solid solution, *e.g.*, $6Fe_2O_3 \longrightarrow 4Fe_3O_4 + O_2$, where a continuous series of solid

solutions is formed, and (2) the two solids form two distinct phases, *e.g.*, $CaCO_3 \rightarrow CO_2 + CaO$. According to the phase rule, the first type does not give a dissociation pressure independent of the proportions of the two solids, whereas the latter, at constant temperature, gives a constant equilibrium pressure as long as the two phases are present. Langmuir (*J.A.C.S.*, 1916, **38**, 2263) has given a kinetic explanation of the two types of dissociation, and shows that, for equilibrium to be obtained in the second of these, condensation and evaporation must take place at the interface between the two solid phases. Thus CO_2 does not evaporate from $CaCO_3$ unless this is in contact with CaO, nor does CaO absorb CO_2 appreciably unless it is attached to a neighbouring molecule of $CaCO_3$. Only under these conditions is the calculated ratio-rate of evaporation/rate of condensation—a constant at a given temperature. It was predicted that the rates of evaporation will be very slow in the initial stages of the dissociation of the pure substance, for here the area of interface will be very small. The rate of evaporation should be at a maximum when the two solid phases are practically in equal proportions, and again small as the composition approaches that of the second phase. Analogous behaviour should occur during rehydration. Richards had previously observed the existence of sudden changes in the rates of dehydration of salt hydrates (*Z. Anal. Chem.*, 1898, **37**, 583) and Davis and Eyre have observed similar behaviour on the rehydration of fibres (*Proc. Roy. Soc.*, 1923, **104A**, 512). More recently, Crowther and Coutts (*Proc. Roy. Soc.*, 1924, **106A**, 215), using the Odén-Keen automatic recording balance, have obtained accurate values for the rates of dehydration of $CaSO_4 5H_2O \rightarrow CaSO_4 3H_2O \rightarrow CaSO_4 H_2O$, and $BaCl_2 2H_2O \rightarrow BaCl_2 H_2O \rightarrow BaCl_2$. The velocity of dehydration is less rapid the more nearly the composition approaches that of the pure phase, and is a maximum at the mid-point in the dehydration. These phenomena have not, so far, been found on rehydration, nor with all salt hydrates, but this was conceivably due to the difficulty of approximating to the true equilibrium conditions. This work provides an interesting confirmation of the Langmuir theory, which has been widely used to explain contact catalysis (Taylor, cf. SCIENCE PROGRESS, 1923, **67**, 359).

GEOLOGY. By G. W. TYRRELL, F.G.S., A.R.C.Sc., Ph.D., University, Glasgow.

Isostasy; Diastrophism; the Wegener Theory.—An important publication by W. Bowie (*U.S. Coast and Geodetic Survey, Special Publ.*, No. 99, 1924, pp. 91) deals with isostatic investigations and data for gravity stations in the United States estab-

lished since 1915. The paper treats what may be called the higher applications of the theory of isostasy to the mechanics of the earth's crust. Amongst the numerous topics discussed the most important is that of the elevation and subsidence of the crust. The most significant conclusions are that, since the crust is now in almost perfect isostatic equilibrium, this condition must have obtained in the past, and therefore, during the processes of elevation and subsidence, the isostatic equilibrium is not greatly disturbed. Dr. Bowie further believes that the cause of at least the major surface changes has been a change in density of the crust directly beneath the affected area, thus reversing the orthodox notion of the order of cause and effect in this particular case.

The strandflat is the relatively level area which extends along the western coasts of Norway and fringes the islands at a height of somewhat less than 40 metres above sea-level. This feature in its relation to isostasy and recent earth movements has been treated by Dr. F. Nansen in an exhaustive work (*Vidensk.-selsk. Skr. Math.-Nat. Kl.*, 1921, No. 11, Kristiania, pp. 313). The greatest breadth of the strandflat, 40–50 kms., is in Helgeland. The width of the feature diminishes towards the south, and to a less degree towards the north.

According to Nansen the strandflat is the physiographic expression of the response of the earth's crust within the Scandinavian region to its loading and unloading by the Quaternary ice-sheet. He infers that the crust is very responsive to local disturbances of equilibrium, and movements do not require a regional cause. With unloading isostatic equilibrium is re-established to within a few metres with only a short lag in time. Furthermore, isostatic movements of the lithosphere are not limited to especially mobile regions nor to periods of special crustal mobility.

In his Presidential Address to the Geological Society of America D. White discusses gravity observations in relation to the local geology at stations east of the Rocky Mountains (*Bull. Geol. Soc. Amer.*, **35**, 1924, pp. 207–78). His chief contention is that the local geology should be studied before the positive or negative anomaly at any station is interpreted in relation to the doctrine of isostasy. He says: "Both the signs and amounts of the anomalies are, in large measure at least, controlled by the excess or deficient density of the underlying rocks in the upper crust, and, accordingly, are subject to compensations which, on the whole, will greatly reduce the apparent departure from isostatic balance. The inspection of the anomalies, with brief examination of the local geologic data, encourages the view that the rock materials of excessive or deficient weight causing the principal portions of the anomalies

lie relatively near the surface, and that allowance in the computation for such matter would largely liquidate the anomalies. It follows that the equilibrium of the crust below the gravity station is very much nearer complete than is indicated by the anomaly as uncorrected for local abnormal densities relatively close to the instrument."

In a paper on the geological implications of the doctrine of isostasy (*Bull. Nat. Research Council* (U.S.), **8**, pt. 4, No. 46, pp. 1–22) Prof. A. C. Lawson shows that if the principle of isostasy be operative for areas occupied by mountain-ranges, large graben, large deltas, and thick marine deposits, the isostatic adjustments of the shifts of load involved in each of these necessitate changes which would be expressed in stratigraphic, physiographic, and structural adjustments, and these constitute geological tests of the validity of the principle. Dealing with isostatic shifts of load, Lawson distinguishes two sets of conditions: (1) closed systems which, after local compensation, remain in balance with the rest of the earth; (2) quasi-closed systems in which local compensation is not competent to restore balance with the rest of the earth. There are probably two kinds of isostatic compensation: (1) local, by means of sub-crustal flow from the loaded to the unloaded region; (2) general, involving a deformation of the geoid.

A. Born's new treatise on "Isostasie und Schweremessungen, ihre Bedeutung für geologische Vorgänge" is reviewed at length by W. D. Lambert (*Amer. Journ. Sci.*, **8**, 1924, pp. 90–8), to which paper the reader is directed for a full summary.

Prof. J. Joly has supplemented his paper on the movements of the earth's surface crust by a second part which amplifies his ideas regarding the conditions of equilibrium which lead to transgressional seas, and also the distribution of temperature in the continental crust (*Phil. Mag.* (6) **46**, 1923). The closely knit argument scarcely allows summary. The isostatic equilibrium of the continents is disturbed by the advancing liquefaction of the postulated "basaltic ocean" beneath in such a way as to cause the land masses to sink unevenly relatively to the sea-level, and thus become partially flooded (see SCIENCE PROGRESS, Jan. 1924, p. 382). The thermal conditions arising out of radioactive heating impose a limit to the thickness of the continents, thereby also limiting their horizontal extent and the area and depth of the oceans.

Abendanon's theory of crustal deformation ("Considérations sur la dynamique de l'écorce terrestre," *Soc. Linn. de Lyon*, Sept. 25, 1922) is difficult to summarise in a paragraph. It involves the compression of weak radial segments of the earth's crust by heavier and more competent segments during radial shrinkage; the up-arching of the weak segment with

compression in the lower part, and tension in the upper, resulting in the formation of great fissures or graben parallel with the axes of folding. A long summary of the theory is given by L. J. Weeks (*Amer. Journ. Sci.*, **8**, 1924, pp. 85–90).

W. H. Bucher shows that the earth's "mobile belts," in which great movements have taken place since the later Palæozoic, are virtually continuous, and form a definite and characteristic pattern, namely, that of the closed belt or ring, and the cross (*Journ. Geol.*, **32**, 1924, pp. 265–91). This is based chiefly on Haug's palæogeographic reconstructions. The continuity of these belts points to the conclusions (1) that they are due to stress conditions affecting the geoid as a whole, and not to local stresses; and (2) that their pattern offers a clue to the mechanics of major diastrophism.

Experiments made with glass spheres to show the mode of failure under compressive or tensile stresses indicate that the pattern of the mobile belts of the earth's crusts is unlike that displayed by wrinkles formed on a thin shell adhering to a contracting core, or by fractures produced by compression on a thin shell adhering to a contracting core; but that, on the other hand, it shows a distinct likeness to the pattern of fractures produced by tension on a thin shell adhering to an expanding core.

Prof. Wilhelm Ramsay (Helsingfors), dealing with the probable solution of the climate problem in geology (*Geol. Mag.*, **61**, 1924, pp. 152–63), connects changes of climate in geological time with the general rhythm of the geological cycle. An increased severity of climate, leading to *miothermic* conditions, accompanies the *orocratic* relief which prevails during and after the great orogenic periods. Similarly ameliorations of climate, leading to *pliothermic* conditions, follow the erosion of continents and mountains to *pediocratic* relief (peneplanation). Glaciation follows as a result of maximum relief, but interglacial periods do not imply corresponding fluctuations in relief. They depend on crustal movements and eustatic variations of sea-level due to the growth of ice-sheets in a miothermic period.

Wegener's views regarding the bearing of his continental drift hypothesis on ancient glaciations are drastically dealt with by Prof. A. P. Coleman (*Amer. Journ. Sci.*, **7**, 1924, pp. 398–404). For the Pleistocene glaciation Wegener assumes a North Pole shifted 20° so as to be in Greenland, and centrally placed in his conglomerated northern continental area. He also assumes a further easterly polar movement, so that the western glaciation is assumed to be earlier than the eastern. Prof. Coleman has no difficulty in establishing the contemporaneity of glaciation over the whole world in the Pleistocene

and a universal lowering of temperature. The Permian refrigeration, too, probably affected the whole world, and hence had some more general cause over and above the grouping of the land masses.

Prof. W. H. Pickering discusses the Wegener hypothesis from the astronomical point of view (*Geol. Mag.*, **61**, 1924, pp. 31–4); and refers to his theory of 1907 that the fissuring of the crust was caused by the birth of the moon. The bed of the Pacific is regarded as the scar left by the catastrophe, whose point of origin was the northern point of New Zealand. Diametrically opposite, near Gibraltar, another great fragment must have left the earth's crust, probably initiating the system of fissures which later widened into the Atlantic Ocean. According to Prof. Pickering this took place in the Archæan, whereas Wegener places the initiation of rifting in the Mesozoic.

E. Jaworski has written a useful summary of the developments and criticisms of the Wegener theory of continental drift, with bibliography up to 1922; but he notices relatively little non-German literature (*Geol. Rundschau*, **13**, 1922, pp. 273–96).

Petrography of Igneous Rocks.—A very detailed statistical study of the quantitative mineralogical composition of the St. Austell granite has been made by Dr. W. A. Richardson (*Quart. Journ. Geol. Soc.*, **79**, pt. 4, 1923, pp. 546–76). Three distinct types have thus been found: biotite-muscovite-granite, lithionite-granite, and gilbertite-granite. The field relations and history of the intrusion phenomena have also been elucidated.

The application of statistical methods (like those of Richardson's) to mineralogical and petrological problems is fully discussed by Dr. P. Niggli ("Anwendungen der mathematischen Statistik auf Probleme der Mineralogie und Petrologie," *Neues Jahrb. f. Min. Beil.-Band.* **48**, 1923, pp. 167–222).

The igneous rocks of the Tortworth inlier have again been studied by Prof. S. H. Reynolds (*Quart. Journ. Geol. Soc.* **80**, 1924, pp. 106–12). The lower band, consisting of albitised olivine-enstatite-basalts, is probably intrusive; but the pyroxene-andesites (again with albitised felspars) of the upper band, which are associated with calcareous tuffs, are contemporaneous lavas, and carry numerous quartz and felspar xenocrysts.

In a paper on the petrographical provinces of Russia, F. Loewinsson-Lessing (*Bull. Soc. Geol. France*, (4), **23**, 1923, pp. 142–57) comes to the conclusion that chemical study of the rocks in general, and the distribution of the alkaline types in particular, do not support the idea of two universal classes of igneous rocks, Atlantic and Pacific, associated with different types of crustal movement. Further, the mean acidity (and

probably other parameters) of the igneous rocks of Russia is identical with that of igneous rocks in general as calculated by Clarke and Washington.

Dr. N. L. Bowen proposes an alternative explanation for the calcite-bearing igneous rocks of the Fen district, Telemark, Norway (*Amer. Journ. Sci.*, **8**, 1924, pp. 1–11), than that of magmatic origin proposed by Prof. W. C. Brögger (see SCIENCE PROGRESS, April 1922, p. 549). His view is that the carbonates are due to replacement, which has acted selectively on the original minerals. There are veining, interfingering, and speckling of the minerals by calcite in such a way as to indicate the work of circulating solutions. Further, recent experimental work by Smith and Adams on the system CaO-CO_2 shows that there are great difficulties in the way of accepting carbonate magmas in the accessible crust of the earth.

The construction of a tunnel over three miles long through Mount Royal, Montreal, one of the essexite plugs of the Monteregian Hills, has revealed seven different episodes of igneous intrusion (J. A. Bancroft and W. V. Howard, *Trans. Roy. Soc. Canada* (3), **17**, 1923, pp. 13–45). Essexite was the first to be intruded, and forms the major portion of the igneous mass. It was itself later intruded by several series of dykes and small plugs of tinguaite, camptonite, nepheline-syenite, etc. The paper deals with the petrology of the essexites only. While 95 per cent. of the essexite consists of the four minerals, augite, hornblende, plagioclase, and olivine, and definitely alkalic minerals form only a part of the remainder, yet chemical analysis shows that alkali molecules enter into the chemical constitution of the first three of the above-mentioned minerals. Consequently the diagnosis of the rock as essexite is maintained notwithstanding the apparent resemblance of some of the varieties to ordinary diorite and gabbro.

In a paper on the igneous geology of Central Colorado R. D. Crawford finds that the igneous and tectonic events have taken place in the following order: (1) intrusion of porphyries from great depth; (2) large-scale folding and faulting; (3) intrusion of quartz-diorite in small bodies; (4) batholithic invasion of quartz-monzonite; (5) minor faulting; (6) deposition of ores emanating from the quartz-monzonite magma; (7) a second intrusion of porphyries; (8) renewed faulting. The co-magmatic relations of the rocks indicate a petrographic province of wide extent.

A description, with some new analyses, of the nepheline-syenites of the Los Islands (Guinea), especially those that contain an abundance of rare earth minerals, such as eucolite, låvenite, astrophyllite, and hiortdalite, is given by Prof. A. Lacroix (*Comptes Rendus*, Paris, tom. 178, 1924, pp. 1109–14).

The Los suite is very similar in many respects to that of the Langesundsfjord region of Norway.

Prof. Lacroix has also described analcite-tephrite and analcite-basalt from Algeria (*Comptes Rendus*, Paris, tom. 178, 1924, pp. 529–35); and has added an excursus on the classification of analcite-bearing lavas. Semi-crystalline rocks in which the minerals appropriate to their chemical composition are not expressed in crystalline form are said by Lacroix to be *cryptomorphic*. Thus *rhyolitoids*, *dellenoids*, *dacitoids*, are rocks of rhyolitic, dellenitic, or dacitic composition, in which the free silica is not expressed in the form of quartz. In the same way the term *scanöite* (from a Sardinian locality) is used by Lacroix for a cryptomorphic form of analcite-basanite in which felspar is not expressed in crystalline form. Washington's term *ghisite* is similarly used for a cryptomorphic form of analcite-basalt.

Dr. H. S. Washington has studied the lavas of Barren Island and Narcondam in the Bay of Bengal (*Amer. Journ. Sci.*, **7**, 1924, pp. 441–56). Barren Island volcano has flows mainly of augite-andesite; Narcondam consists entirely of hornblende-dacite lavas. In petrological characters these lavas correspond with those which are found on the outer or convex sides of the Malay series of tectonic arcs or festoons.

Lacroix has described a series of plutonic rocks consisting of nepheline-syenite, alkali-syenites, quartz-syenite, granite, and eucrite, which have been obtained from glacial blocks in Kerguelen (*Comptes Rendus*, Paris, tom. 179, 1924, pp. 113–19). The mode of geological occurrence of these is unknown. The presence of spilites, however, amongst specimens obtained from the island, leads Lacroix to speculate on the probable occurrence of a volcanic series, much older than that of the main basaltic plateau. Analyses of the plutonic types are given, as well as of phonolitic and trachytic types associated with the basalts.

Two very large and detailed petrographic memoirs, one on the igneous rocks of the Nyassaland part of the East African rift valley region (E. Lehmann, "Das Vulkangebiet am Nordende des Nyassa als Magmatische Provinz," *Zeitschr. f. Vulkan, Erg.-Bd.* IV, 1924, pp. 209), the other on the igneous rocks of the Argentinian Andes (H. G. Backlund, "Der magmatische Anteil der Cordillera von Süd-Mendoza," *Medd. Åbo Akad. Geol.-Min. Inst.*, no. 3, 1923, pp. 298), have come to hand. They cannot be satisfactorily summarised within the limits of short paragraphs.

PLANT PHYSIOLOGY. By PROF. WALTER STILES, Sc.D., University College, Reading (Plant Physiology Committee).

Carbon Assimilation.—When this subject was last reviewed in these pages (SCIENCE PROGRESS, **18**, 27–34, 1923) reference

was made to the work of Lundegårdh, who found a different relation between the rate of photosynthesis and light intensity in sun leaves and in shade leaves, in air of normal carbon dioxide concentration. Some further information on this question is now forthcoming from observations made by N. Johansson on ferns (" Zur Kenntnis der Kohlensäureassimilation einige Farne," *Svensk Bot. Tidsk.*, **17**, 215–23, 1923). It was found that in the case of *Polypodium vulgare* the relation between photosynthetic velocity and light intensity is that typical of a shade leaf, whereas *Pteris aquilina* behaves like a typical sun plant. Unusual relations were observed in the case of *Dryopteris austriaca*. In this plant the relation between rate of photosynthesis and light intensity is that typical of a shade leaf for low and moderate intensities of illumination, but a maximum rate of assimilation occurs in a light intensity of 30 per cent. of full sunlight, the photosynthetic velocity declining with further increase of light intensity. *Dryopteris spinulosa* was found to behave similarly, the highest rate of photosynthesis occurring in this fern in 60 per cent. of full sunlight.

Observations on the effect of high temperatures on the rate of photosynthesis of a number of marine algæ have been made by R. Wurmser and R. Jacquot (" Sur la relation entre l'état physique du protoplasma et son fonctionnement," *Bull. Soc. Chim. Biol.* **5**, 305–15, 1923). The species used were *Ulva lactuca*, *Codium tomentosum*, *Rhodymenia palmata*, *Laminaria saccharina*, *L. digitata* and *Iredea edulis*. After subjection to temperatures between 36° C. and 45° C. for periods varying from 1 to 15 minutes, the algæ were returned to sea-water at normal temperature, 16° C., and the rate of photosynthesis determined by measuring the evolution of oxygen by Winkler's method, or by determining the change in carbon dioxide concentration of the sea-water. It was found that the rate of photosynthesis was always depressed as a result of this treatment, the depression being greater the higher the temperature and the longer the time of exposure to the high temperature. The suggestion was made by Wurmser and Jacquot that the effect of the high temperature is to modify the condition of the cell colloids in such a way that the mechanism in the cell bringing about the reduction of carbon dioxide is thrown out of action. It is a point worth noting that the temperature sufficient to bring about complete suppression of photosynthesis is lower than that required to kill the plant. That assimilation is more adversely affected than other plant processes by harmful agents appears to be a general phenomenon; at least O. Warburg (" Über die Geschwindigkeit der photochemischen Kohlensäurezersetzung in lebenden

Zellen," *Biochem. Zeitschr.*, **100**, 230–70, 1919; **108**, 188–217, 1920) found that in strong light photosynthetic velocity is reduced by hydrocyanic acid when present in as low a concentration as N/10,000, whereas respiration was similarly adversely affected only when the concentration of hydrocyanic acid was as high as N/100. A similar difference between the influence of glycerol in retarding photosynthesis and respiration has been observed by C. Fromageot (" L'assimilation chez les cellules vertes et la structure du protoplasma," *Comp. rend. acad. sci.*, **177**, 892–4, 1923; " Sur les relations entre l'état physico-chimique et le fonctionnement du protoplasma : photosynthèse et respiration," *Bull. Soc. Chim. Biol.*, **6**, 169–80, 1924).

An attempt has been made by S. Kostytschew (" Studien über Photosynthese. II. Wirkt Wundreis stimulierend auf die Kohlensäureassimilation am Lichte ? " *Ber. deut. bot. Ges.*, **39**, 328–33, 1921) to determine whether wounding has a stimulating effect on assimilation. To this end leaves of *Betula pubescens* and *Lamium album* were wounded by a series of strokes made with a fine-pointed glass needle, and the rate of photosynthesis of the injured leaves compared with that of uninjured controls. No evidence was obtained of any stimulating effect resulting from the wounding. No account, however, appears to have been taken of respiration and the effect of wounding on this process, and it would appear that the question remains in doubt.

In another contribution the same writer discusses photosynthesis in the Leguminosae (" Studien über Photosynthese. IV. Die CO_2-Assimilation der Leguminosen," *Ber. deut. bot. Ges.*, **40**, 112–20, 1922), and comes to the conclusion that plants of this order are capable of very considerable rates of photosynthesis. Thus, in an atmosphere containing at the commencement of the experiment 8·86 per cent. of carbon dioxide exposed apparently to sunlight in July and at a temperature of 19·3° C., *Trifolium repens* assimilated at the rate of about 63 milligrams per square decimetre per hour, whereas under similar conditions *Veronica chamaedrys* and *Leucanthemum inodorum* only assimilated at the rate of about 34 milligrams per square decimetre per hour. But this high rate of assimilation observed in the Leguminosæ appears to have no relation to the presence of root tubercles, for *Alnus glutinosa*, which also possesses root nodules, assimilates at the lower rate. Moreover, rates of assimilation as high as those recorded by Kostytschew for Leguminosæ had been previously observed by Willstätter and Stoll for *Cucurbita*, and an even higher rate had been observed by the same investigators in the case of the sunflower.

Kostytschew has also examined the photosynthetic activity

of plants during the summer night of subarctic regions (" Studien über Photosynthese. III. Findet eine Kohlensäureassimilation während der Sommernächte in der subarktischen Region statt ?" *Ber. deut. bot. Ges.*, **39**, 334–8, 1921), and records that photosynthesis can be detected in *Pinus Strobus* and *Abies sibirica*, but not in the Angiosperms examined. It is thought possible that in these latter closure of the stomata takes place so that absorption of carbon dioxide is prevented, whereas in the Coniferæ the stomata remain open.

Yet another question examined by Kostytschew is the value of the assimilatory coefficient (" Studien über Photosynthese. I. Das Verhalten CO_2/O_2 bei den Kohlensäureassimilation," *Ber. deut. bot. Ges.*, **39**, 319–28, 1921). He makes the somewhat startling assertion that over very short periods of illumination the quantity of oxygen is very much less than the amount of carbon dioxide absorbed, although over longer periods the ratio of these two quantities is approximately unity. The same result was obtained with both higher plants (*e.g.*, leaves of *Betula verrucosa* and of *Potentilla anserina*) and lower ones (a mixture of green algæ). A mixture of *Spirogyra communis* and *Zygnema stellatum* yielded an apparent assimilatory coefficient as low as 0·21 during a period of 5 minutes' illumination. The suggested explanation of this finding is that the chlorophyll and other constituents of the cells absorb carbon dioxide which does not result in an immediate evolution of oxygen.

According to Kostytschew (" Die Photosynthese der Insektivoren," *Ber. deut. bot. Ges.*, **41**, 275–80, 1923) the insectivorous plants *Drosera rotundifolia* and *Pinguicula vulgaris* are quite normal in their photosynthetic relations, and the same has been found to be the case with the semi-parasites of the Rhinanthaceæ (S. Kostytschew, E. Tswetkowa, and M. Tillmann, " Untersuchungen über die Ernährung der grünen Halbschmarotzen." *Beih. z. bot. Centralbl.*, Abt. 1, **40**, 351–73, 1924), thus confirming the earlier results of Ewart and Heinricher.

Not many contributions have been made recently to our knowledge of the products of photosynthesis, but two papers deserve notice. In one of these H. Melchior (" Über das Vorkommen von Inulin in den Blättern der Marcgraviaceen," *Ber. deut. bot. Ges.*, **42**, 198–204, 1924) records the presence of inulin in the leaves of two species of *Marcgravia*, and he regards this substance as a product of assimilation in these plants and also in chicory, in the leaves of which inulin was observed eleven or twelve years ago by Grafe and Voux.

The other contribution deals with the much-debated question of the first sugar to be produced in photosynthesis and has been made by T. Weevers (" The First Carbohydrates that Origi-

nate during the Assimilatory Process. A Physiological Study with Variegated Leaves," *Kon. Akad. Wetensch. Amsterdam, Proceedings* (English Version), **27**, 1–11, 1924). Determinations of the quantities of hexose and sucrose in the green and yellow parts of variegated leaves of a number of species were made, and in most cases hexose and sucrose were found to be present in the green parts, whereas sucrose was the only sugar detected in the yellow parts. In the only two cases in which hexose was found to be present in the yellow parts of the leaves the quantity of this class of sugar was much less than in the green parts of the same leaves. The restriction of hexoses to the actual assimilating regions of variegated leaves strongly suggests that hexoses precede sucrose in the building up of carbohydrates in the leaf. Weevers obtained supporting evidence for this view from experiments with leaves of *Pelargonium zonale*, which, when exposed to light after a period in darkness, were found by chemical analysis to form first hexose, then sucrose, and finally starch.

Two interesting investigations on the efficiency of the photosynthetic system have been made by O. Warburg and E. Negelein. As experimental plant they used the green alga *Chlorella*, which, being a unicellular plant, does not introduce complications incident to a cellular structure such as a foliage leaf. In the first of their researches ("Über den Energieumsatz bei der Kohlensäureassimilation," *Zeitsch. f. physik. Chem.*, **102**, 235–66, 1922) they allowed the light from a metal filament lamp to pass through screens of ferrous sulphate, copper sulphate, and aniline dye solutions, so that all rays except the yellow and yellow-red were removed. The issuing light, composed of rays with wave-lengths between 570 and 645 $\mu\mu$, illuminated a suspension of the *Chlorella* cells. From determinations of the total energy falling on the cells as measured by means of a bolometer, and from calculations of the rate of photosynthesis as given by the rate of gaseous exchange, the proportion of the incident energy actually utilised in photochemical work in the photosynthetic process was determined. Two intensities of incident light were employed, and it was found that the proportion of energy utilised is less, the higher the intensity of the incident light. Warburg and Negelein, therefore, in order to obtain a definite value for the ratio of incident to utilised energy, calculated by extrapolation the value of the ratio for the limiting case when the intensity of the incident light is zero. In this way they obtained a mean value of the ratio of 0·71. This is, of course, an exceptionally high value for the ratio of the energy utilised to the incident energy. Clearly, however, values found in this way by extrapolation from two values only are not likely to be very exact,

and, indeed, the unsatisfactoriness of treating results in this way was later realised by Warburg and Negelein.

In the second paper of these authors ("Uber den Einfluss der Wellenlänge auf den Energieumsatz bei der Kohlensäure-assimilation, *Zeitschr. f. physik. Chem.*, **106**, 191–218, 1923) an attempt was made to determine the influence of the wave-length of the light on its utilisation in photosynthesis. The method was essentially the same as that used in their earlier work, but a number of different screens were used to obtain light of definite wave-lengths, while prisms were used as well to isolate light of some wave-lengths. It was then found that the proportion of the incident energy utilised decreased with decreasing wave-length, the values actually found in light of low intensity being as follows: red (660 $\mu\mu$) 0·59; yellow (578 $\mu\mu$) 0·535; green (546 $\mu\mu$) 0·444; blue (436 $\mu\mu$) 0·338. The values are lower than that found previously for yellow light, but the divergence is due chiefly no doubt to the method of extrapolation used previously to which attention has already been called.

Finally, reference may be made to experiments by R. Harder on the assimilation of a blue-green alga *Phormidium foveolarum*, in light of different colours ("Über die Bedeutung von Lichtintensität und Wellenlänge für die Assimilation farbiger Algen," *Zeitschr. f. Bot.*, **15**, 305–55, 1923). This plant is one that exhibits the so-called chromatic adaptation; in red light it develops a green colour and in blue light a purple colour. Cultures of the alga were grown in both intense light and weak light of each colour and determinations of the rate of photosynthesis of material grown under these different conditions made in red and blue light of the same and different intensities. It was found that when the rate of photosynthesis was measured in red and blue light of the same intensity, material that had grown in red light assimilated more rapidly in red light, while that grown in blue light assimilated more rapidly in light of that colour. But also it was found that material grown in strong light, either red or blue, assimilated more rapidly in intense light of either colour than in weak light of either colour. The conclusion drawn by Harder from his experiments is, therefore, that material assimilates best in light of the same colour and the same intensity as that in which it had developed; that is, the plant is adapted not only to light of the colour of that in which it has grown, but also to light of the intensity of that in which it has been cultivated. His results thus bring into harmony the two formerly opposed theories of Engelmann, according to which plants assimilate best in that coloured light in which they have grown, and of Richter according to which plants are adapted to that intensity of light in which they have been growing.

EDUCATION. By A. E. Heath, M.A., University, Liverpool.

The question of academic freedom is of interest to us all, since the price of liberty is eternal vigilance; but to our colleagues on the other side of the Atlantic it is more than this—it is a matter of immediate practical concern. Principal Ernest Barker's choice of subject for his Presidential Address to Section L (Educational Science) of the British Association at Toronto was therefore a happy one. In his paper on "The Nature and Conditions of Academic Freedom" Principal Barker points out that if the cause of academic freedom was fought in the past on the ecclesiastical field, it is likely to be fought in the future on the fields of politics and economics. A professor of such subjects cannot stop short of running into the actualities of the present. "If he were required to do so," Dr. Barker continues, "he would be stopped from reaching what we may almost call the point of fertilisation, where his knowledge touches actual life." It is clear, however, that "the freedom of the teacher, like all freedom that is other than mere licence and anarchy, must exist within a framework of law, because it exists within the framework of an institution, and because, again, any institution involves some system of law." In the penetrating and dispassionate analysis which follows, Dr. Barker has many wise things to say about the character and conditions of freedom of the institutions within which the teacher works; and about the dangers to which, in our own time, such freedom is exposed. But it is to his treatment of the freedom of the teacher that I wish, now, to direct attention. In round terms he takes up the view that the institution cannot hold itself responsible for what its professors may say, for the assumption of such responsibility is likely to involve the institution in more danger than any indiscretion of one of its members. What controls are there, then, on the individual's caprice? In Dr. Barker's opinion there are two very strong ones. In the first place, the freedom of the professor must be qualified by the duties inherent in his membership of the institution: if it gives him freedom "he must not give it obloquy in return." In the second place, the freedom of the professor "is subject to the discipline of the profession." The special obligations which this implies are less definite than those of the doctor and lawyer; but they are there nevertheless. "He has embraced a profession devoted to the dispassionate search for pure truth." He seeks such truth by a rigorous method of inquiry. "The temper of his mind must be steeled into a resolute disposition to see every side and weigh every factor." And, finally, he is training young minds; and what he is, what he does, affects the growth of those minds. "If there is a discipline which is a special obligation of the soldier,

there is also a discipline which is a special obligation of the professor who serves under the banner of truth."

This treatment opens up many lines of thought; but two seem to me to be of great importance to all teachers.

(1) The discipline of the profession, of which Dr. Barker speaks, is by no means always operative even within the class-room itself. Moreover, the most devastating instances of its absence arise, not from lack of good-will but from lack of understanding. The kind of instance I have in mind is that of the teacher who is over-anxious to do the best for his pupils. His subject is presented with all possible clarity and precision. Whatever preliminary polishing *can* be done *has* been done; and the whole is passed on to the student in such a manner that the barest minimum of effort is called for from him. What is lacking is just that "delicate web of doubt" without which intelligent learning is impossible. It is not enough that we should be enlisted under the banner of truth; we must be able to re-create in the minds of our pupils the sense of joyous intellectual adventure which fired the discoverers of that truth. The issue has been put in another way by the late Dr. Rivers. In his essay on "Education and Mental Hygiene" (*Psychology and Politics*, 1923, 97–105) he distinguishes two kinds of teacher. "There is the teacher who lays down the law in an emphatic tone and expects to be believed with such assurance that his expectations are largely realised." This we may call the "impressive" type, who may succeed in making replicas of himself, but who certainly creates a kind of dependence on the part of his pupils. On the other hand, we have the teacher who gives his pupils a chance of seeing "how opinions are formed." This "catalytic" type aims rather at stirring his pupils to activity than at taking the active part himself in the solution of their problems. "It is far more attractive," wrote Dr. Rivers, "to be looked up to as a fountain of wisdom . . . than to run the danger of having one's views discredited by a critical pupil"; and it seems, alas, to be true enough that the fountains of wisdom outnumber the catalysts. A change of attitude is clearly needed if the "discipline of the profession" is to be a living reality.

(2) The second line of thought opened up by Dr. Barker's paper is the reflection that our teacher may be all that is desired in the class-room, and yet (outside it) be as credulous and irrational as any ordinary man. Dr. Barker fully realises that the professor must not be *solely* of the laboratory: he "must be a man, and not an automaton . . . some measure of outside interest and outside work is a condition of vitality, and even of balance." If one could only be sure that the "discipline" of the laboratory or the class-room would be

carried into the outside world, all would be well. But does this desirable state of affairs exist? In his admirable book on *Aspects of Science* (1923, p. 102), Mr. J. W. N. Sullivan points out that "the man who will conduct a laboratory experiment with meticulous precision and describe his results in an agony of honesty will be content to be a prejudiced observer and a slovenly and inaccurate thinker in all other matters." "This is the chief reason," he continues, "why men of science count for so little in public affairs . . . all the questions on which scientific men now adopt 'sides' as uncritically as any simple dupe of the daily press are amenable to scientific examination." What the world needs, and has the right to expect, from the academic mind is just this deliverance from "the method of charms and incantations" by which all such questions are at present treated. The problem of academic freedom will be well on its way to solution when professors show some signs of applying their professional standard of intellectual integrity to the world of affairs outside their institutions.

The following is a selection of references to recent work:

W. Eliasberg, "Recent work on the Psychology of Forming Concepts," *Psychological Bulletin*, 1923, **30**, 8, 427–37. An interesting summary of recent German work, with special reference to Ach's *Ueber die Begriffsbildung*.

B. T. Baldwin, "The Mental Development of Children," *Psy. Bull.*, 1923, **20**, 12, 665–83. An account, with full classified references, to recent work on the subject.

E. B. Holt, M. F. Washburn, and E. Hatt, "The Correlation of a Test of Control of Visual Imagery with Estimated Geometrical Ability," *American Journal of Psychology*, 1923, **34**, 1, 103–5. A previously developed test of control of visual imagery (*ibid.*, **25**, 293–5) was used, modified for a group-test. There was evidence of good correlation.

M. Jacobs, "The Comparative Individual Psychology of Dr. Alfred Adler," *Pedagogical Seminary*, 1923, **30**, 1, 16–23. An interesting summary of Adler's method as developed in his *Ueber den Nervosen Charakter*.

G. Stanley Hall, "The Gospel of Magnanimity," *Ped. Sem.*, 1923, **30**, 3, 252–63. One of the last essays of this wise and gifted writer.

G. C. Myers, "Some Whys of Whims: A Genetic Study," *Ped. Sem.*, 1924, **31**, 1, 78–83. Notes on some whims of a child, and an interesting analyis of them.

O. Lipmann, Cyril Burt, and L. L. Thurstone, "The Principles of Vocational Guidance," *British Journal of Psychology*, 1924, **14**, 4, 321–61. These papers were read at the 7th International Congress of Psychology, Oxford, 1923.

Shepherd Dawson, "Variations in the Mental Efficiency of Children during School Hours," *Brit. Journ. of Psy.*, 1924, **14**, 4, 362–9. Mental efficiency was measured at different times of day under actual school conditions by the rate at which the children performed simple arithmetical operations.

F. M. Ritchie, "Some Effects of Prolonged Unvaried Mental Work," *The Forum of Education*, 1924, **2**, 1, 48–61; and 2, 83–98.

E. J. G. Bradford, "Selection by Examination," *Forum*, 1923, **1**, 3, 187–94.

VERNON BROWN, "The Suggestive Force of Different Question Forms in School Work," *Forum*, 1923, **1**, 3, 203–13. In connection with the above papers on examinations see also the editorial discussion on "Some Principles of Examination," *School Science Review*, 1923, **5**, 17, 1–5.

V. T. SAUNDERS, "A Plea for the Reconsideration of 'Definitions.'" *School Sci. Rev.*, 1923, **5**, 18, 63–70. An article of the greatest practical interest to the teacher. I agree whole-heartedly with the author's defence for including so many references to writers on the philosophy of science: "The best, the very best, in education is only just good enough. It behoves the teacher, then, to examine his subject to its deepest foundations." Only thus can we judge (1) what definitions will leave the pupil with the least to unlearn later; and (2) at what stage it is desirable to introduce analytical definitions at all.

E. J. HOLMYARD, "The Historical Method in the Teaching of Chemistry," *School Sci. Rev.*, 1924, **5**, 20, 227–33. A provocatively written essay with a lesson for all teachers of science; but certain arguments, based on the supposed "training value" of subjects, are used. These are criticised in the following paper.

DOROTHY TURNER, "Mental Training," *School Sci. Rev.*, 1924, **6**, 21, 37–43. A critical analysis of recent work on Formal Training was given by the writer of these notes in *Science Progress*, 1924, **18**, 71, 401–6.

M. C. MCGRATH, "A Study of the Moral Development of Children," *Psy. Monographs*, 1923, **32**, 2, 1–190. A compendious account of moral "tests" designed to help the examiner to find out whether particular delinquency is inherent or whether it is due to lack of moral training or background. If such tests, comparable to tests of intelligence, be possible, then moral education might become formative rather than merely corrective. The author believes that the child has inalienable right "to be trained to social conduct before he has a chance to err." Whatever we may think of the positive results described so far, it is clear that the problems raised are of the greatest interest. A full bibliography is appended. The complexities of the problem are well brought out in the short article mentioned below.

F. B. KNIGHT, "A Note on Moral Education," *Ped. Sem.*, 1923, **30**, 1, 31–4.

ALEXANDER PATERSON, "The Bad Lad," *Forum*, 1924, **2**, 1, 15–21. It is something of a relief to turn from these elaborate treatments of delinquency to this simply written article on the "Bad Lad." Mr. Paterson, who is one of His Majesty's Commissioners of Prisons, makes no pretensions to psychological treatment, but deals in a plain and practical way with the problems raised, and invites the earnest co-operation of teachers in helping to prevent the troublesome lad who has got into conflict with the law from being converted into a criminal.

L. K. FRANK, "Suggestions for a Theory of Learning," *Psy. Rev.*, 1923, **30**, 2, 145–8. The author suggests that, since learning is a process of habit-formation, it may be studied and accounted for in terms of conditional reflex processes. This is criticised in a later volume of *Psy. Rev.*, 1923, **30**, 5, 402–6, by J. PETERSON, who says it shows "The usual neglect of the really essential aspects of the problem." It is true that Dr. Franks avoids the assumption of arbitrary faculties; but he does not touch the real problem—namely, of how ineffective initial acts are eliminated in later and more successful attempts.

F. O. EFFRIGHT and M. F. MEYER, "The Equation of the Learning Function," *Amer. Journ. of Psy.*, 1923, **34**, 2, 203–22.

H. L. KOCH, "The Influence of Mechanical Guidance upon Maze Learning," *Psy. Mon.*, 1923, **32**, 5, 1–113.

L. T. SPENCER, "The Effects of Practice without Knowledge of Results," *Amer. Journ. of Psy.*, 1923, **34**, 2, 107–11. Judd had found (*Psy. Rev. Monographs Supplement*, 1905, **7**, 185) that practice brings little

if any change when the habit formation is built up without the guiding datum of evidence of success or failure. Re-examination of his work and a repetition of the experiment show a certain slight improvement.

W. R. Wilson, "Principles of Selection in Trial and Error Learning," *Psy. Rev.*, 1924, **31**, 2, 150–60.

H. Cason, "Purposive Psychology and the Conditional Reflex," *Psy. Rev.*, 1924, **31**, 3, 253–5. The above two papers attempt to answer McDougall's challenge to the comparative psychologist to explain the shortening of the trial and error series in terms of mechanical bodily movement.

W. H. Winch, "Should Poems be learnt by School-children as 'Wholes' or in 'Parts'?" *Brit. Journ. of Psy.*, 1924, **15**, 1, 64–79. It has been shown that where the subject-matter is difficult, or the mental ability of the child low, the generally more effective "whole" method is inferior to learning in "parts." Dr. Winch experimented with children under the usual conditions of school life. He concludes that there is cumulative evidence that the span of apprehension and immediate memory, which is known to vary with mental development, is an important operative factor; and that, for school-children of the grades experimented on (Standards V. to VII.) a "part" method of memorizing poetry of the usual form is decidedly superior to a "whole" method.

L. M. Terman, "Mental Test as a Psychological Method," *Psy. Rev.*, 1924, **31**, 2, 93–117. The growth of tests marks also the development of experimental methods, and throws light upon problems of general psychology.

E. L. Thorndike, "The Measurement of Intelligence: the Present Status," *Psy. Rev.*, 1924, **31**, 3, 219–52.

J. H. Wilson, "Comparison of Certain Intelligence Scales," *Brit. Journ. of Psy.*, 1924, **15**, 1, 44–63. An interesting comparison between the Terman Group Test; the Otis Group Intelligence Scale, Adv. Exam. A; the National Intelligence Tests, Scale A2; the Northumberland Mental Tests, 1; and the Simplex Intelligence Scale.

R. R. Dobson, "An Investigation of Group Intelligence Tests," *Brit. Journ. of Psy.*, 1924, **15**, 2, 162–8. The main conclusions are that the tests do seem to select the brightest pupils; that there is an increase in age performance in all tests up to the age of seventeen years, further advance only occurring in persons of very high ability; and that the possibility of comparing the intelligence of young with old by total scores is questionable.

David W. Oates, "The Nature and Validity of Subjective Tests of Intelligence," *Forum*, 1924, **2**, 2, 103–21. The criterion most frequently used in estimating the value of a test is the personal estimate of intelligence by the teacher. But the experts differ widely in their judgment of the value of this criterion. This able and interesting investigation into the validity of teachers' estimates of children's intelligence throws light on some of the difficulties encountered, and points the way to further advance. A careful analysis of this kind has long been needed.

ARTICLES

THEORIES OF MAGNETISM

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HELMHOLTZ has been credited with the remark, "Our ignorance concerning magnetism is the disgrace of the nineteenth century." It is interesting to survey the work to which he referred and also the later practical and theoretical investigations, in order to see whether the advances in this field have kept abreast of the great advances in related branches of physical science.

It was only with the isolation of the electron that progress was made in the theory of electric conductivity. The same light would be shed on magnetic phenomena by the isolation of the ultimate magnetic particle, if it exists. The existence of such a unit is a direct consequence of the Quantum Theory, and a smaller unit has long been suspected from empirical calculations. In order to follow these calculations it is advisable to begin with the earlier theories, which have been developed into modern forms to fit in better with the newer ideas of atomic structure.

Pre-Electronic Theories.—A satisfactory theory of magnetic induction was begun by Poisson, and developed further by Green and Kelvin. This theory, which deals with a continuous magnetic substance, is essentially a statistical theory, so that from its very nature it cannot lead us to ultimate causes.

Following on Œrsted's discovery of the magnetic field due to an electric current, Ampère formulated a brilliant theory of the ultimate magnetic particle. He supposed that the molecules of a magnetic substance consist of closed resistanceless circuits round which minute currents are flowing. From the nature of the circuits the currents will remain constant. The action of a magnetic field on a large number of such currents would be to rotate the molecules, so that their axes coincide with the direction of the external field.

The next great advance was due to Faraday's discoveries of electromagnetic induction, and of the behaviour of glass in a magnetic field, which led him to the study of diamagnetism. From Faraday's results Weber developed magnetic theory

into a form which can be regarded as the true starting-point of all modern theories. According to Weber's theory the molecules of a magnetic substance are permanent magnets, which have their axes at random in the unmagnetised state, but can rotate about their centres when an external field is applied. There is a molecular magnetic field which is constant in magnitude throughout the body, but the direction of the field for any molecule is that of the magnetic axis of the molecule in its equilibrium position. From these assumptions Weber showed that the magnetisation would increase uniformly with the impressed field until the magnetisation reached a value which was two-thirds of its maximum value, when the rate would diminish, the maximum value being approached asymptotically. The theory does not account for residual magnetism. Maxwell modified Weber's theory by introducing a permanent alteration in the equilibrium position of the axis of the molecular magnet when the impressed field exceeded some fixed value. He recast Weber's thory of diamagnetism and calculated the diamagnetic susceptibility of a substance per unit volume in terms of the self-induction and area of the elementary magnetic circuit.

Ewing attacked the problem of permanent magnetisation from a different point of view by giving to the molecular magnet the bulk properties of the substance, treating each element as a minute bar-magnet with an interpolar distance of the same order as the distance between the molecules. The forces opposing magnetisation were the mutual actions of these small magnets. With a model of this type Ewing was able to reproduce qualitatively most of the hysteresis phenomena. There is no doubt that any explanation of ferromagnetism must take into account the mutual actions of the elementary magnetic particles ; but the attribution of bulk properties to the ultimate unit is unlikely to lead to a quantitative solution of any completeness.

Electronic Theories.—The first real proof of Ampère's theory that magnetism was a rotatory and not a linear phenomenon was deduced by Koláček from the Hall Effect. If magnetism is supposed to be linear, then an electric current must be rotatory. The existence of the Hall Effect is inconsistent with the latter supposition, since it would impose a symmetry about the direction of the current, even when a transverse magnetic field is imposed.

The electronic theory of magnetism was built up by Langevin, who was helped a great deal by the researches of Pierre Curie on paramagnetic and diamagnetic substances. For paramagnetic substances Curie found that the susceptibility was independent of the field strength, and inversely proportioned

to the absolute temperature. He found that the ferromagnetic bodies, nickel, soft iron, and magnetite, obeyed the laws of paramagnetism when they were heated beyond the critical temperatures at which they lost their ferromagnetic properties. For diamagnetic substances Curie found that, in general, the susceptibility was independent of the temperature and state of the body. Bismuth was a notable exception. Many exceptions have been found to Curie's laws, but they were the inspiration of Langevin's theory. In this theory an atom is pictured as containing a number of electrons, rotating in closed orbits. There may be other electrons which execute small oscillations about equilibrium positions. A symmetrical distribution of several orbits may be such as to have no resultant magnetic moment. The normals may also be grouped round a symmetry axis of the atom, and have a resultant magnetic moment. In all cases an impressed field will produce changes manifested as diamagnetism. In the case of atoms with a resultant magnetic moment, further changes will take place which will make the body behave paramagnetically. In special cases the mutual influence of the paramagnetic atoms will cause hysteresis, and the body, as a whole, will be ferromagnetic.

The diamagnetic effect is similar to that calculated by Maxwell from Weber's theory. If a single orbit be imagined with its magnetic axis in the same direction as the external field, the growth of the field will retard the motion of the electron in the orbit, so that when the field is established the moment of the orbit is diminished. The change of moment will depend on the angle between the normal to the orbit and the impressed field. Lorentz has shown that diamagnetism can be accounted for, even if the electrons are at rest when the external field is applied. In so far as the constitution of the atom is unaffected by temperature changes, the diamagnetic susceptibility will probably also be unchanged. In the case of bismuth, mentioned above, the big volume change on melting is probably accompanied by electronic rearrangement.

In the case of the non-compensated orbits the diamagnetic changes are dwarfed by the tendency of the axes to come into alignment with the magnetic field. The heat motions of the atoms prevent parallelism, the inverse proportionality of susceptibility to absolute temperature being due to the decrease of heat motion with temperature. The application of the magnetic field will convert the mutual potential energy between the orbit and the field into kinetic energy of the atom or of its parts. If we neglect other positional forces in the atom the energy can be supposed to change into the heat motion of the atom. By the application of the second law of thermodynamics

it can be shown that the intensity of magnetisation is, in this case, a function of $\frac{H}{T}$, the external field divided by the absolute temperature. In the case considered the intensity of magnetisation will be proportional to the impressed field, so that we have deduced Curie's empirical law. Langevin considered the case of a paramagnetic gas, and calculated the form of the function of $\frac{H}{T}$ which is required from thermodynamics. He obtained the equation

$$\frac{\sigma_m}{\sigma_{m_0}} = \frac{\sigma_{m_0} H}{3RT}$$

$\sigma_m = N\mu$ where N is Avogadro's number and μ the moment of the magnetic element. σ_{m_0} is therefore the maximum magnetic moment of a gramme-molecule.

σ_m is the magnetic moment per gramme-molecule calculated from the intensity of magnetisation found at a temperature T degrees absolute when H is the external field.

R is the gas constant.

Thus for Curie's constant the value $\sigma_{m_0}^2/3R$ is obtained.

Confirmation of this theory was sought by applying it to oxygen. Keesom found that liquid oxygen does not obey Curie's law, but obeys the law

$$\chi (T + \Delta) = \text{constant},$$

where χ is the magnetic susceptibility and Δ is a numerical constant, whose value is found to be 71.

Onnes found that the susceptibility of mixtures of liquid oxygen and liquid nitrogen did not obey the additive law. The susceptibility of oxygen apparently increases on dilution. Thus we cannot apply Langevin's theory to oxygen in the liquid state. This increase of susceptibility with dilution has also been noticed for small quantities of iron dissolved in diamagnetic metals. From these results Onnes was led to search for compounds of small paramagnetic concentration, *i.e.* in which the atoms responsible for the paramagnetism are surrounded by a large number of diamagnetic atoms. With his co-workers Jackson and Woltjer, he has found four substances which obey Curie's law down to the lowest temperatures attainable. They also faithfully reproduce Langevin's curve. In particular the curve of $Gd_2(SO_4)_3 . 8H_2O$ shows that about 84 per cent. saturation has been reached. These results must be taken as conclusive proof of the validity of Langevin's hypothesis. There are many substances which obey the modified Curie law $\chi (T + \Delta) = \text{constant}$, and attempts

have been made to connect Δ with the magnetic dilution by the finding of the susceptibilities in different directions in crystals. These attempts have not been very successful. Langevin's theory does not explain the behaviour of gaseous oxygen, but this may be due to the presence of O_4 molecules. The formation of O_4 molecules has been suggested by Dolezalek from chemical and other evidence, and was recently independently suggested by G. N. Lewis to explain the magnetic phenomena. He assumes that the paramagnetic properties are due to double bonds which exist in O_4 but not in O_2.

Weiss extended Langevin's theory to ferromagnetic substances by the introduction of a molecular field arising from the neighbouring atoms. The H of Langevin's theory is then the sum of the impressed and molecular fields. Many of the ferromagnetic phenomena can be explained by the existence of this intrinsic field, but Weiss himself concludes that the mutual forces are neither magnetic nor electrostatic.

The experimental discovery of the Richardson effect—that is, the finding of the momentum due to magnetisation—renders the theory of rotation of the elementary magnets even more probable. The moment of momentum found is only half the amount predicted by the theory. Richardson has suggested that the true magnetic element consists of two parallel or coplanar electron orbits, and that the rotation of the nucleus must also be taken into account.

Ehrenfest proposed a model in which the directions of the normals to the orbits were unchanged by an external field, the resultant magnetic moment being due to a change of sense of rotation of the orbits. This theory will only account for 50 per cent. of the saturation value of gadolinium sulphate.

Several other quantum theories have been put forward. Typical of these is Reiche's theory. The initial postulates are that each magnetic element shall be capable of free rotation about a fixed point, that its rotation about its magnetic axis is independent of thermal agitation, and that its moment of inertia, J, about any equatorial axis through its centroid, is the same for all such axes. The expression obtained for the susceptibility is—

$$\chi = \frac{\pi^2 N \mu^2}{h^2} J \frac{\frac{5}{4} e^{-\sigma} + \frac{1}{3} \sum_{m=2}^{n=\infty} e^{-\sigma n^2}}{\sum_{1}^{\infty} n e^{-\sigma n^2}}$$

$$\text{where } \sigma = \frac{h^2}{8\pi^2 JRT}$$

where μ is the moment of the magnetic element, h is Planck's constant, and the other symbols have their usual significance.

The values calculated from this formula are in good agreement with the experimental data for manganese sulphate, and similar compounds, but no account is taken in the theory of mutual actions of the elements, and in J and μ it possesses two adjustable constants. At low temperature, when σ is large, Reiche's expression becomes

$$\chi = \frac{5}{4}\frac{N\mu^2}{h^2}\pi^2 J,$$

and at high temperatures $\chi = \dfrac{N\mu^2}{3RT}$,

which is Langevin's form.

The 1/3 in Langevin's theory is the average value of $\cos^2\theta$. From the point of view of the Quantum Theory all values of θ (*i.e.* the angle between the normal to the orbit and the impressed field) are not possible. Instead we get a series of cases depending on the value of the inner quantum number. Pauli has thus modified the Langevin form by introducing the factor

$$\frac{(n+1)(2n+1)}{2n^2} \text{ in which } n \text{ is an integer.}$$

The Quantum Theory leads directly to an ultimate magnet unit, usually called the Bohr magneton. The moment of momentum of the electron in its orbit must be an integral multiple of $h/2\pi$. Thus the magnetic moment of the orbit must be an integral multiple of $eh/4\pi m$. This latter is thus the ultimate unit, which must bear the same relation to paramagnetic phenomena as the electron itself does to current electricity.

The maximum magnetic moment of the gramme molecule must therefore be an integral multiple of 5593 gauss. cm. Magnetic susceptibilities must, therefore, provide a test of the validity of spatial quantisation, the value of n in Pauli's factor being the number of magnetons per molecule (or atom).

Now, although the equipartition theory of Langevin does not bind us to the existence of any unit, the study of the variation of the magnetic susceptibility of magnetite with temperature led Weiss to suppose that a unit existed, and, from measurements of the saturation values at low temperatures of iron and nickel, he found a value 1123.5 gauss. cm. for the unit value per gramme atom. Later he renounced the possibility of measuring the unit to one part in ten thousand, and has adopted from further data a value 1126 gauss. cm. This is almost exactly one-fifth of the value of the Bohr magneton. The Weiss unit is purely empirical and a great deal of very sound experimental work has been performed in order to demonstrate its existence.

The work is mainly the determination of the magnetic susceptibilities of the salts of metals of the iron group. In many cases there is a variation of atomic moment with concentration, in which the value of the true atomic moment is obtained as a limit. Unfortunately, it is necessary in some cases to assign different moments to the same radicle. The values found for the metals themselves are much smaller than those found for the metals as radicles, both in solution and in the solid salts. These results have been tested by several workers in order to see how far the Bohr magneton fits the facts. The following table from a paper by Epstein shows a comparison.

Radicle.	Weiss Magnetons m.	Bohr Magnetons n.	m'.	n'.	$\chi \times 10^4$. Observed.	Calculated.
$Cr+++$	19	3	15·2	3·04	5·0	4·8
$Cr++$	24	4	20·2	4·04	7·7	7·7
$Mn++$	29	5	25·2	5·04	11·5 and 11·6	11·3

The m' is the value obtained instead of m when Pauli's correction is applied by estimating n. These are only a few of the numbers so treated, but are sufficient to indicate the kind of agreement between arithmetic and observation.

The establishment of a whole number rule is bound to be difficult, especially in cases when the accuracy of the experimental determination is unknown. The smaller unit will have the advantage in all such comparisons. There is no doubt that all the results could be considered as a justification of a unit equal to half a Weiss magneton, and there is no reason why an empirical highest common factor should not be so divided. The empirical unit has evidently outworn its usefulness, and can lead us no further.

The only justifiable conclusion from the results is that the magnetic moment of the atom is in general independent of the rest of the molecule. The method of calculation involves several assumptions, so that it is quite possible that better agreement between the results and the Quantum Theory may be obtained by calculating on a different basis.

A very direct proof of the validity of spatial quantisation is obtained from the classical experiment of Gerlach and Stern. A beam of uncharged silver atoms is sent normally across a magnetic field, in which the strength of the field varies rapidly in the direction of the field. If there is a one quantum orbit in the silver atom, this will place itself at right angles to the magnetic field, with the unit of moment of momentum $h/2\pi$. The electron may be revolving in either sense, so that the beam will be split up into two parts. This is tested by setting

up a plate normal to the beam after it has traversed the field, and a double trace is found. According to the classical theory, in which all orientations are possible, the stream should be broadened only. The double trace is thus a direct proof of the validity of the quantising of direction of the orbits in a magnetic field. From the velocity of the particles and the field gradient the magnetic moment of the atom is calculated to be one Bohr magneton. The experimental error is 10 per cent., but the verification in the experiment of the theory by which the Bohr magneton is calculated justifies this method of stating the result. Landé proposed that, although the angular momentum is $h/2\pi$ for these silver atoms, the magnetic moment is not one but two Bohr magnetons, but that the axes are at 60° to the field, so that the projection of the moments is still ± 1. The experiment has been repeated with copper, gold, and thallium. The beam separation seems to hold in these cases, and both copper and gold are estimated to have a moment of one Bohr magneton.

The Position of the Magneton in the Atomic Models.—The most striking fact about the property of paramagnetism is that, although it is a function of the motion of an electron in an orbit, it is found only in a very limited number of atoms, nearly all of which belong to two definite sets in the table of elements. The first set is in the fourth period, in the transition stage between symmetrical configurations of 8 and 18 electrons respectively in Kossel's table, beginning with scandium and ending with copper. The second set is the family of rare earths. Before considering the theory of the placing of the orbits in these cases, it will be advantageous to compare this peculiarity of certain electron orbits with that exhibited by another very limited and entirely different set of orbits.

When a superconducting element at low temperature is subjected to a magnetic field the induced currents set up are apparently of atomic dimensions, and persist for very long periods. There appears to be an electron or electrons in the atom which respond far more than the rest of the atom to the external force and whose orbital motion is subjected to no interference from the other electrons. Thus the difference between the orbits of a superconducting atom, say gallium, and the inactive atom preceding it, zinc, is the addition of a surface electron, which is almost entirely screened from nuclear action.

The characteristic change which leads to paramagnetism is the addition of inner orbits after the completion of the external shell. According to Bohr's theory of atomic structure these paramagnetic orbits are large circular orbits.

The scheme is shown in the following table, which shows the number of electrons in the various orbits.

Atomic Number.	Symbol.	K.	L.		M.			N.			
		1_1.	2_1.	2_2.	3_1.	3_2.	3_3.	4_1.	4_2.	4_3.	4_4.
18	A	2	4	4	4	4					
19	K	2	4	4	4	4		1			
20	Ca	2	4	4	4	4		2			
21	Sc	2	4	4	4	4	1	2			
22	T	2	4	4	4	4	2	2			
23	Va	2	4	4	4	4	3	2			
24	Cr	2	4	4	4	4	4	2			
25	Mn	2	4	4	4	4	5	2			
26	Fe	2	4	4	6	6	2	2			
27	Co	2	4	4	6	6	3	2			
28	Ni	2	4	4	6	6	4	2			
29	Cu	2	4	4	6	6	6	1			
30	Zn	2	4	4	6	6	6	2			
31	Ga	2	4	4	6	6	6	2	1		

The calcium-scandium change indicates the incidence of paramagnetism, the zinc gallium change the incidence of superconductivity. It is true that the 4_2 orbit is not a large orbit, as indicated above. But we shall see that in paramagnetism also some change must be made in this scheme, although the general trend is certainly correct.

The 3_3 orbits which indicate the presence of paramagnetism are large circular orbits, which will probably be affected by external forces on account of their nearness to the surface of the atom.

The gradation of paramagnetism in the series requires some further explanation. From the experimental results the simple law is obtained that the magnetic moment of an ion depends only on the number of electrons in the ion. Thus the ferric ion has the same moment as the manganous ion, and the manganic ion has the same moment as the chromous ion. The maximum moment is that of ferric and manganous ions.

If there be a whole number law, since the maximum value is 5 magnetons, the graph, atomic number against magneton number should be two straight lines intersecting at 5, those on the lower atomic number side rising, the higher atomic number side falling. Epstein has shown that this result can only be obtained if we take as abscissæ the number of non-nuclear electrons. That is, the apex is a double point 23 (Fe^{+++} and Mn^{++}), 5; another double point is 22 (Mn^{+++} and Cr^{++}) 4.

Another difficulty is that the values of the magneton numbers obtained experimentally give greater moments of momentum than the Bohr orbits to which they are assigned, and it is difficult geometrically to get only integral values for the sums of the component vectors on the resultant axes. The experimental data for the rare earths are too sparse to admit of a real test of the theories.

Pauli has also modified Langevin's theory of diamagnetism by the introduction of quantum criteria. From his results, and from the known values of atomic radii and sizes of orbits, he predicted that the value of the magnetic susceptibility of helium at 0°C. would be -2×10^{-10} and that of argon from $-2{\cdot}7$ to $-8{\cdot}1 \times 10^{-10}$.

An extension of his methods gives $-3{\cdot}9 \times 10^{-10}$ for neon. Results just obtained by Hector are:

Helium . . .	$\chi = -{\cdot}78 \times 10^{-10}$
Neon . . .	$-2{\cdot}77 \times 10^{-10}$
Argon . . .	$-7{\cdot}52 \times 10^{-10}$

which constitute a remarkable agreement between theory and experiment.

Ferromagnetism.—The problem of ferromagnetism is to discover under what conditions paramagnetic atoms can build up into ferromagnetic bodies. In paramagnetism we are dealing with a property of a limited number of atoms; in ferromagnetism with a very small number of compounds of those atoms; the problem seems to be one of the molecular structure or crystal structure. This was demonstrated by Maurain, when he found that thin films of iron and nickel were only ferromagnetic after they had attained a certain thickness. This lower limit was of the order 10^{-7} cms.

The question then arose as to the nature of the unit which rotated on magnetisation. This question was in great part answered by the Comptons and their collaborators. They find that there is no change, in either position or intensity, of the X-ray diffraction maxima when a strong external field is applied to a ferromagnetic crystal. This means that it cannot be the atom which rotates, unless it is so nearly isotropic that its rotation does not alter the space distribution of its electrons, which is highly improbable. The ultimate rotating particle is thus probably an electron orbit (or orbits). The size and position of such orbit or orbits are a matter of conjecture. A great difficulty, yet unexplained, is the change of length on magnetisation. To explain this it is probable that the electronic orbit alone is insufficient. S. R. Williams has suggested that the nucleus may be a partial cause. The nucleus is small, but a habit is growing of burying our difficulties therein.

The elucidation of crystal structure by X-ray methods would seem to promise very well towards a solution of the problem of ferromagnetism, but so far our hopes have not been realised. The case of metallic iron illustrates the great difficulties with which we meet. From a study of the variation of magnetic properties with temperature Weiss had found that

iron exists in four different phases when it is in the solid state. These he called α, β, γ, and δ iron, the α iron being the iron in its normal ferromagnetic state, β (paramagnetic) being formed from α by heating above the magnetic critical point. β changes at higher temperature to the paramagnetic γ form with a different susceptibility, and the last change before melting is to another paramagnetic state, δ. Westgren found that α and β iron have the same crystalline form, body-centred cubic, the change at the magnetic critical temperature having no influence on the position of the X-ray diffraction maxima; γ iron is face-centred cubic, whilst δ iron is body-centred.

It is, therefore, impossible to associate ferromagnetism with the body-centred cubic lattice. This conclusion is further confirmed by the structure of cobalt and nickel. Cobalt may exist either in a hexagonal close-packed structure or in a face-centred cubic lattice, nickel only in the face-centred cubic arrangement. The structure of the Heusler Alloys of manganese, aluminium, and copper has been investigated by Young. Their ferromagnetism is certainly a consequence of the paramagnetism of manganese and possibly of copper also. These magnetic alloys show both the face-centred and body-centred cubic arrangement. Ferromagnetism is certainly strongest when the atoms are closest together, generally in the body-centred cubic form; but ferromagnetism is associated with several forms. The structure of hæmatite, Fe_2O_3, has been investigated. It is isomorphous with corundum (Al_2O_3). The magnetic properties are peculiar, the crystal being paramagnetic along the trigonal axis of the system and ferromagnetic in the plane perpendicular to this. The iron atoms are arranged in doublets, the axes of the doublets being in the trigonal axis of the crystal, the " centres " of the atoms in a pair being about 2·65 angstroms apart, and the closest distance between atoms of different pairs, along the trigonal axis, is 4·68 angstroms.

Each iron atom is surrounded trigonally by three atoms from neighbouring doublets, the interatomic distance being 2·85 angstroms. These three are not in the same " horizontal " plane as the atom they surround, but in a plane ·37 angstroms above or below it. The absence of ferromagnetic properties along the trigonal axis may be due to the fact that the distance between the pairs is beyond the range of ferromagnetic bonding influence; or the axis of the magnetic element may be constrained into a nearly " horizontal " plane by the influence of the three atoms which surround it at the shorter distance. In this case the ferromagnetism would appear only in a " horizontal " plane since each doublet has six nearest single atom neighbours, 3 in a " horizontal " plane with centres ·93 angstroms above the waist of the doublet, and

3 in a "horizontal" plane ·93 below the waist, and therefore the resultant moment, due to bonding with these closest neighbours, would for the whole crystal be in a "horizontal" plane. The question will not be solved until we have further knowledge of the nature of the forces which bind together the atoms of crystals. In this connection some interesting calculations have been made by Ashworth, from experiments on the variation of susceptibility with temperature of iron and nickel, under hysteresis free conditions. Under these conditions Curie's law still holds below the critical point, although the Curie constant changes at this point. The ratio of the two values obtained is the kinetic energy per unit of temperature per gramme of two degrees of freedom. Thus the change from the ferro- to para-magnetic state is due to the gain of energy associated with two degrees of freedom.

This conclusion can be explained in terms of the crystal structure of iron, since it is well known that both body-centred and face-centred cubic structures are sometimes "ionised" forms—that is, that the atoms in them can be treated geometrically as points or spheres, the crystal binding forces being bulk forces, without any directional relation to any axes in the atom, and independent of atomic rotation. We could therefore look upon α iron as "unionised"—that is, in a comparatively static state in which atomic rotations are small, the bonding between atoms being in part due to magnetic attraction of outer orbits of the symmetrical system which is responsible for diamagnetism; so that the magnetic fields due to these outer orbits determine positions of stability for the inner "magneton" orbits. The change to the β state would then consist of "ionisation" without change of lattice, owing to the rupture of the magnetic bonding by increased temperature motion, the "bulk" forces of cohesion being now responsible for the stability of the lattice. The introduction of "bulk" forces of unknown origin is perhaps a doubtful advance, but the increase of degrees of freedom is nevertheless partly explained.

We see that considerable advances have been made since Helmholtz made the remark quoted at the beginning of this article. A great deal remains, particularly in ferromagnetism, for which we have as yet no satisfactory explanation. The greatest advance made is more in point of view than in theory. We are convinced that there is unlimited scope for human endeavour in the tiniest corner of the great field of natural phenomena.

A table of 140 references to recent papers on Magnetism is given by L. W. McKeehan (*Franklin Institute Journal*, **19**, 1924, May and June, pp. 583–601 and 757–786). A splendid summary of work done by the Weiss school is given by Cabrera (*Journal de Physique*, **3**, 1922, p. 443).

THE PRESENT POSITION OF THE GEBER PROBLEM

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IN the last two or three years a good deal of research has been carried out on Geber, both in this country and in Germany, and we are now in a position to arrive at some definite conclusions on what is one of the most important problems in the early history of chemistry. Briefly, the problem is as follows. Certain Latin works on chemistry, entitled *Summa Perfectionis, De Investigatione Perfectionis, De Inventione Veritatis, Liber Fornacum,* and *Testamentum Geberis*, have for several hundred years been ascribed to one Geber. They are of the greatest interest, since they contain a large amount of chemical information expressed in language which, for its time and its subject, is unusually clear and definite. But their origin and their author have been shrouded in mystery, and no one has been able satisfactorily to answer the questions :

i. Were these books actually written by " Geber " ?
ii. If so, who was Geber ? If not, who did write them ?
ii. Are they of European origin ? If so, what is their date ?
iv. If they are not of European origin are they, as has been suggested, derived directly or indirectly from Arabic works on chemistry ?

Hitherto the available evidence on these points has not been considered as a whole, and, while it is impossible to collect it all within the limits of this short article, a summary of the chief features will be made. In this way it will be possible to indicate what measure of reliance is to be placed upon the various conclusions which have been reached.

1. *The Identity of Geber.*—It will be convenient to consider, in the first place, the identity of " Geber," before passing on to the more important question of the origin of the *Summa* and other works. In several printed editions, but apparently in no manuscript, of the *Summa*,[1] Geber is described as an Arab. Thus in Russell's English translation[2] he is called " The Most

[1] Darmstaedter, *Die Alchemie des Geber*, Berlin, 1922, p. 3.
[2] London, 1678.

Famous Arabian Prince and Philosopher"; he is an Arab in the Nuremberg edition of 1541; in the Danzig edition of 1842 he is "King of the Arabs." Other editions also make him an Arab, while in a Bodleian MS.,[1] probably of the fifteenth century, entitled, *Liber practicus Geberis . . . de investigatione perfecta magisterii*, he is called "King of the Persians." A rare incunabulum, *Liber qui flos naturarum vocatur*, 1473, is attributed to "Geber, King of India." From these facts it is clear that Geber was considered to be an Oriental of some kind, and that the majority believed him to be an Arab.

In many medieval alchemical works an author named Geber is quoted, and very often one finds the name in the extended form, *Geber ebn Haen*; for "Geber," again, one occasionally meets with "Jeber."[2] Now *ebn Haen* is clearly a crude transliteration of the Arabic *ibn Ḥayyān*, and *Geber* or *Jeber* of *Jābir*. Hence there can be no doubt whatever that the Latin name Geber is intended to refer to an Arab chemist of the name Jābir ibn Ḥayyān. The general substitution of *g* for *j* is readily understood when it is remembered (*a*) that there is no *j* sound in Latin, and (*b*) that the Arabic *j* was and still is sounded as hard *g* in Egypt and many other countries [*e.g.*, in Egypt, *gabal* for *jabal* (mountain)].

The next step is to ascertain whether there ever was an Arab chemist called Jābir ibn Ḥayyān. Fortunately there is no difficulty here. Abū Mūsā Jābir ibn Ḥayyān was a celebrated chemist and physician, who lived during the latter half of the eighth century and flourished at the Court of the Caliph Hārūn al-Rashīd (786–808). Some account of his life has already been given in these pages,[3] and more information is to be found in articles by Prof. Ruska[4] and the present writer.[5] The Muslim historian Ibn Al-Qifṭī (thirteenth century) states, in his *History of the Sages*,[6] that Jābir excelled in the natural sciences, especially that of chemistry, and wrote numerous well-known books. He was, in addition, skilled in philosophy and esoterics, and was a Ṣūfī. Other writers give further details, some of which will be mentioned later. All later Muslim chemists mention Jābir with reverence, and many of them quote his works freely. His reputation was equalled by that of no other chemist of Islam.

2. *The Origin of the Latin Works ascribed to Geber.*—The undoubted identity of Geber with Jābir ibn Ḥayyān is of

[1] Western MS., 19039.
[2] Darmstaedter, *op. cit.*, p. 134.
[3] July 1923.
[4] *Sudhoff's Archiv*, Dec. 1923, *Arabische Alchemisten II* (Heidelberg, 1924).
[5] *Proc. Roy. Soc. Med.*, 1923, xvi. (sect. of *Hist. of Med.*, 46–57).
[6] Ed. *Lippert*, p. 160.

little importance if the Latin works are falsely ascribed to him. At the same time, the proof of this identity affords a clue to the source of the works, and renders it necessary to examine them closely for signs of Arabic origin. Moreover, it necessitates a study of those Arabic works of Jābir ibn Ḥayyān which are extant, of the process of transmission of Arabic chemistry to Europe, and of various Latin chemical treatises which, while not usually included with the *Summa* and the other four books mentioned at the beginning of this article, nevertheless pass under Geber's name. A courageous attempt at this task was made by Berthelot [1] in the last decade of the nineteenth century, but, while he made available much useful material, he was far too hasty in drawing conclusions. Some criticisms of his work have been published by various writers,[2] and it is now generally admitted that many of his judgments require drastic revision. Some Arabic manuscripts, containing works attributed to Jābir ibn Ḥayyān, are preserved in the *Bibliothèque Nationale* and at Leyden, and Berthelot published the texts of a dozen of these works, with translations by Prof. O. Houdas. A single glance at them shows that they certainly cannot be regarded as originals of the Latin works, and Berthelot therefore concluded that the latter must be of European origin; he regarded their ascription to Geber as false. It is, however, perfectly clear that this conclusion is open to criticism from two aspects. In the first place, is it certain that the Arabic works in question are themselves genuine? In the second, why should not the Latin works be translations from *other* Arabic works of Jābir?

On the first of these two points it may be remarked that Prof. Ruska has conclusively shown [3] that two of the Arabic treatises published by Berthelot are spurious, and that in a third the author twice plainly gives his name as Abū 'Abdullāh Muḥammad ibn Yaḥyā. It would seem, therefore, that we are justified in regarding the rest with a little suspicion, and that it is necessary to investigate them very carefully before using them in evidence. There are, however, many of Jābir's works extant which appear to be quite authentic, and from some of these we can glean very valuable information.

On the second point it is necessary to state that Wiedemann, Ruska, and Steinschneider have all expressed the opinion, on philological grounds, that the Latin works, *in their present form*, cannot be regarded as translated directly from Arabic originals.

[1] *La Chimie au Moyen Âge*, Paris, 1893, tomes i and iii.

[2] E. O. von Lippmann, *Die Entstehung und Ausbreitung der Alchemie*, Berlin, 1919, pp. 647–59; J. Ruska, *Sudhoff's Archiv*, Dec. 1923, *passim*; E. J. Holmyard, *Chemistry and Industry Review*, Oct. 5 and 12, 1923.

[3] *Loc. cit.*

With this opinion I am in general agreement, but I would modify it by saying that many isolated passages appear to me to be almost literal translations from the Arabic, while Arabic phrases and grammatical constructions occur frequently. It will be apparent that the only method of attack which seems likely to lead to a solution of the problem is to examine carefully those Arabic works of Jābir which have claims to be regarded as authentic, and to try to trace resemblances in the Latin works. This process, which is necessarily laborious, has already yielded some encouraging results, to which reference will be made later. As a preliminary, however, it will be useful to consider shortly some facts relating to the translation of Arabic chemical treatises into Latin, a task upon which many scholars were engaged in the twelfth and thirteenth centuries of our era.

3. *Early Latin Translations of Arabic Chemical Books.*—It was in Moorish Spain that European scholars first came into close contact with Muslim scholarship. Young men from all over Europe flocked to Spain to sit at the feet of the Muslim doctors, and they soon became fired with enthusiasm for the new knowledge. Translation of Arabic works into Latin rapidly became a serious business, sufficient to keep a large band of scholars continuously at work. Some of these men are well known—Adelard of Bath, Hermann of Dalmatia, and Gerard of Cremona will spring to our minds at once. In view of the fact that the natural sciences, including chemistry, were very popular among the Muslims of Spain, it is not surprising to find that among the translations were several of scientific and mathematical works. The Englishman Robert of Chester [Robertus Castrensis] in the twelfth century translated the Algebra of Al-Khowarizmi; some account of his work has been given by Prof. L. C. Karpinski of the University of Michigan.[1] Other translations due to Robert of Chester are one of the Qur'ān and one of a chemical work entitled *De compositione alchemiæ*, by "Morienus Romanus." He says he finished the latter translation on February 11 in the year 1182 (of the Spanish Era, *i.e.* A.D. 1144), and that it was the first chemical work in the Latin tongue. Now, although no Arabic original exists of this work in its entirety, many passages—some long and some short—are to be found in Arabic alchemical books, and there is no reason to doubt that the *De compositione alchemiæ* was certainly based upon, if not in the main literally translated from, a previously existing Arabic work. Whether the translation was really made by Robert of Chester or not appears to be uncertain. Prof. Ruska has brought forward[2]

[1] *Robert of Chester's Latin Translation of the Algebra of Al-Khowarizmi*, New York, 1915.

[2] *Arabische Alchemisten*, I, 34 ff. (Heidelberg, 1924).

reasons for doubting it, the main one being that the translator describes himself as a young man with but a meagre knowledge of Latin, whereas we know for a fact that Robert had in 1143 finished his translation of the Qur'ān into Latin—a translation that was printed at Basle in 1550, with a foreword by Melancthon. Ruska also has a few words to say on the authenticity of the "translation" itself, and regards it not as a true translation, but rather as an original work based upon an Arabic foundation. He appears not to know of the Arabic original of the passages referred to above. "Taken as a whole," he concludes, "the *Compositio Alchymiæ* is a falsification, or rather the final link of a chain of falsifications which had originated even as far back as the Greek writings of the seventh and eighth centuries."[1]

It is in any case a fact that translations of Arabic chemical works really were made, for we possess in a few instances both the translations and the originals, *e.g.* the *Book of Secrets* [Kitāb al-Asrār] of Rhazes [Abū Bakr Muḥammad ibn Zakariyya al-Rāzī, died 923 or 932], and the *Risālatu'sh-shams ilā al-Hilāl* [*Epistola solis ad lunam crescentem*]. We can easily imagine that, given the vast difference in character between Latin and Arabic, accurate translation was much more difficult than adaptation, and that the temptation to give "free renderings" must often have proved irresistible. In pronouncing upon the authenticity of professed translations, therefore, we must take this circumstance into account, and not reject as necessarily entirely spurious a Latin work which is obviously not a literal translation. In other words, it would be a mistake to conclude, from the fact that a Latin work is written in a good [mediæval] Latin style, or in a style reminiscent of the schoolmen, that its *matter* as well is *ipso facto* of European origin. This consideration has not always been borne in mind, and its neglect has led to the dismissal of all sorts of works as falsifications. The matter is one in which the exercise of a good deal of caution is requisite. A much safer guide than the argument from *style* is the argument from *content*; it is, however, unfortunately a guide which cannot be fully employed until a far more comprehensive study of Arabic chemical works has been made, and it is much to be desired that more scholars should undertake work in this field, where there is an enormous amount of material awaiting investigation.

4. *The Real Geber.*—We have, then, reached the position that (i) "Geber" must certainly be taken as a transliteration of "Jābir"; (ii) there actually existed a famous Muslim chemist, whose name was Jābir ibn Ḥayyān; (iii) Arabic works on alchemy were translated into Latin in the twelfth

[1] *Arabische Alchemisten*, I, p. 47.

and thirteenth centuries of our era; (iv) it must often have occurred that, instead of accurate translations, European scholars made free adaptations of the Arabic works; and finally (v) as a criterion of Arabic origin it is therefore better to judge mainly by content, and only in the second place by style.

Let us therefore inquire a little more fully into the life and works of the real Geber, or Jābir. It is impossible to do so exhaustively, for two reasons. In the first place, the space at present available is of course entirely inadequate, and, in the second, even now only a portion of the Arabic works ascribed to Jābir have been *read* by European scholars, not to say *studied*. Still, even in this limited space, and with our present imperfect knowledge, we can arrive at an estimate of his attainments as a chemist sufficient to enable us to form some conclusion on the problem of the origin of the *Summa* and other Latin works ascribed to him. It must be said, at the outset, that complications of a very perplexing nature have to be faced, inasmuch as the authenticity of some of the Arabic works themselves is, as mentioned above, rather more than dubious. This question will be discussed later.

Of Jābir's life we know a good deal—far more than we do of many much more famous men of so remote a time. Although there is no reliable evidence as to his birthplace, the best authorities agree in making him either a native of Tūs in Khorassan (N.E. Persia) or of Ḥarrān ('Irāq). Of the two, the former appears more likely, and we shall probably not be making a grave mistake if we regard Jābir as a countryman of the great poet Firdausi, who also was born at Tūs. The authorities all agree, again, in saying that he lived for part of his life at Al-Kūfa (where, indeed, his laboratory is said to have been found during the demolition of some houses nearly a couple of centuries later). He was a friend of Hārūn al-Rashīd's powerful ministers the Barmakides, to whom he dedicated several of his books, and he seems to have been established for a time at the Court at Baghdad. It is hardly necessary to mention that the absurd tale (still current in some Histories of Chemistry) that he was a native of Seville is due to a confusion of Jābir ibn Ḥayyān with Jābir ibn Aflāḥ al-Ishbili ("of Seville"), the well-known astronomer of the eleventh century.

Jābir is said to have been a pupil of the Sixth Imam, Ja'far al-Ṣādiq (A.D. 700–765), which, as far as time is concerned, is quite possible, since Jābir probably lived from 730 or 740 to about 810. But there is grave doubt as to whether Ja'far ever engaged himself in the study of chemistry at all. On this point Prof. Ruska has recently published a learned memoir,[1] which must receive our attention since it raises a

[1] *Arabische Alchemisten, II.* (Heidelberg, 1924).

question of considerable importance. Muslims are divided into two main branches, the Shi'as and the Sunnīs. The Shi'as were and are largely recruited from the Persians. They hold the Prophet's cousin and son-in-law, 'Alī ibn Abī Ṭālib, in an exaggerated reverence, and have always been more inclined to Ṣūfī-ism (Islamic mysticism) than the Sunnīs. Ja'far al-Ṣādiq, as a direct descendant of 'Alī,[1] was held in high esteem by the Shi'as, and Jābir, as a Persian, and possibly a Ṣūfī, may well have been in a close personal relationship with him. But there is little doubt that all extant works on chemistry ascribed to Ja'far are entirely spurious, and Ruska concludes that it is unlikely that Ja'far could have been an alchemist. His reasons are too long and complex to reproduce here, but one of his main points is that, while the study of alchemy was practised at Damascus, Baghdad and Alexandria, we can scarcely imagine it to have been countenanced in the very city of the Prophet, Medina, where Ja'far lived. It is, he says, absurd to imagine Ja'far—a pillar of the Shi'a community—busying himself with cucurbite, alembic and aludel, and with mercury and sulphur, or teaching the art of the transmutation of the metals to a pupil like Jābir. He would, therefore, deny Ja'far's connection with alchemy altogether, and is not content to stop there, but goes on to say that we must " therefore regard all books ascribed to Jābir, in which Ja'far al-Ṣādiq is named as his master and teacher, as falsifications of a later period."

It seems to me that this is going much too far, for the following reasons: (1) There is, to the best of my knowledge, no evidence that Ja'far spent the whole of his life at Medina. While I agree with Prof. Ruska that the open practice of alchemy at Medina in those days is scarcely conceivable, there is nothing absurd in supposing that Ja'far may have engaged in alchemy elsewhere. (2) As no one knows better than Prof. Ruska himself, the Persians were then, and always have been since, enthusiasts for alchemy. In the popular literature the alchemist is always a Persian, while the Ṣūfīs, in their mystical poems, very often employ the terms of alchemy. In short, considering that Ja'far was closely associated with the Persian Shi'as, I see no inherent improbability in assuming that he had some acquaintance with alchemy, theoretical at least, if not practical. (3) The connection between Jābir and Ja'far is, as stated above, probable enough, and Prof. Ruska's only reason for doubting it is his conclusion that Ja'far could not have been an alchemist. But (*a*) I regard this as unproven, and (*b*) Jābir was—the authorities tell us—a Ṣūfī, and it is the usual custom in that fraternity for a " pupil " to attach himself

[1] Ja'far was the son of Muḥammad al-Bāqir, the son of Alī Zain al-'Ābidīn, the son of Al-Ḥusain, the son of 'Alī ibn Abī Ṭālib.

to a "master." I suggest therefore, as a possibility, that Jābir was a disciple of Ja'far in the practice of Ṣūfī-ism. If so, he may have learned something of alchemy from him as well. What I am anxious to make clear is that, even if Ja'far was not a practising alchemist, there is no valid reason for denying the connection between him and Jābir, or for assuming that Ja'far was unacquainted with the terms and aims of the alchemists.

It has been necessary to explain this point at some length since the adoption of Prof. Ruska's criterion (*viz.* that all Jābir-books mentioning Ja'far as his teacher are spurious) would mean the rejection of a whole host of works which have every appearance of authenticity. True, early alchemical works always have to be regarded with suspicion, but I would enter a plea against a too summary rejection of books *en masse*. Prof. Ruska's careful and scholarly researches have rendered great service to the cause of the History of Islamic Chemistry, and with most of his conclusions I am in perfect agreement; in the present instance, however, I am unconvinced of the validity of his criterion. It is possible that he is correct, and in any case his opinion must carry great weight; but the evidence is not conclusive, and there is a good deal to be said against it.

The information concerning Jābir's life which is provided by the chief Muslim authorities I have described in the *Proc. Roy. Soc. Med.*[1] I have since found another account by Al-Jildakī (died about 1360), the celebrated encyclopædist of Muslim chemistry, in a manuscript of his *Kitāb al-Burhān fī Asrār 'Ilm al-Mīzān* (Book of Proof on the Secrets of the Science of the Balance).[2] It runs as follows: "The Great master Jābir ibn Ḥayyān ibn 'Abdullāh was born at Al-Kūfa, belonged to the tribe of Al-Azd [? *Al-Sada qabīlatan*], was a Tūsī by extraction [*Al-Ṭūsī munshā'an*] and a Ṣūfī by persuasion. He was a disciple of [*akhara 'an*] Ḥarbī [or Ḥirbā] al-Ḥamīrī of Al-Yemen, who was one of the *Mu'ammarīn*. Jābir explains this word [*lit.*, those of great life] by saying that Ḥarbī had reached the age of 400 years, having been born more than 200 years before the Hijra [A.D. 622], and living until the days of Hārūn al-Rashīd, 170 years after the Hijra. May the mercy of Allāh be upon him! When, from Ḥarbī, Jābir had acquired in his youth a good knowledge of the sciences, he departed to the Imām Ja'far al-Ṣādiq ibn Muḥammad al-Bāqir ibn 'Alī ibn al-Ḥusain (peace be upon them!), through whom he became an Imām. He then joined himself with the Barmakides, in whose company he carried out much experimental work. It was

[1] XVI, section of *Hist. of Medicine*, 46, 1923.
[2] Bodleian MS., *Hunt.* 68.

through him that the Barmakides obtained the benefits of wisdom and their high station and great power in the kingdom and [were enabled also to exercise their] unbounded liberality. It is even the case that dinars, on the model of the official ones, were struck in Ja'far [the Barmekide's] name, the weight of each being 100 dirhans. Through the medium of the Vizier Ja'far, Jābir was brought into relation with the Caliph Al-Rashīd, and wrote for him a book on the Noble Art [alchemy] entitled *The Book of the Blossom.*[1] In it he put the shortest methods, in what concerns both the manifest and the occult, with an elegant procedure and wonderful experiments. It was on Jābir's account that the second importation of Greek books from Constantinople was made. He became expert in the philosophic sciences, and wrote more than 3,000 books.[2] He died at an age exceeding ninety years. . . . May the mercy of Allāh be upon him ! "

Aidamir al-Jildakī had an unrivalled knowledge of the chemists of Islam, and many of his sources of information are now lost. The above account of Jābir, apart from its obviously legendary matter, is of interest in that it offers an explanation of some of the difficulties which surround the life and birthplace of the " Great Master." If he were born at Al-Kūfa of a family from Ṭūs, the designations " Al-Kūfī " and " Al-Ṭūsī " would both be correct. That there was an influx of Persians from Khorassan to Al-Kūfa, Al-Basra, and other towns in the newly founded Muslim empire is well known. A further point of interest is the statement that it was through Jābir that the " second " importation of Greek books from Constantinople was made. The first importation which Al-Jildakī has in mind was probably that made by the 'Umayyad prince Khālid ibn Yazīd († 704). It was not until the reign of Al-Ma'mūn (813–33) that the process reached its maximum development ; this Caliph sent a deputation to the Roman Emperor (Leo the Armenian) with a request for Greek books for translation into Arabic, and built the celebrated House of Wisdom (*Baitu'l Ḥikma*) at Baghdad in which the translators, together with astronomers and other scientists, were installed.

It seems, therefore, that while Jābir's main interests lay in chemistry, he was a widely-read scholar, and probably had some knowledge of Greek. His own list of his writings, which has come down to us at second-hand in the *Kitāb al-Fihrist*, shows that, in addition to books on chemistry, he wrote others on a variety of subjects—a fact which need not surprise us

[1] Or the *Book of Copper* (Venus). This is mentioned by Ibn al-Nadīm in his *Kitāb al-Fihrist*, in which he gives a list of Jābir's books, compiled from another list made by Jābir himself.

[2] Many of Jābir's " books " occupy no more than one or two pages.

when we remember the vast extent of the intellectual treasures now becoming available to the Muslims through their introduction to Greek learning *via* Jundī-Shāpūr, Ḥarrān, Alexandria, and other centres of Hellenic culture. Another point to be noticed is that, as both a Ṣūfī and a chemist, Jābir would be very likely to use chemical terms and imagery in his mystical writings; we must, therefore, be on our guard lest we regard as text-books of chemistry what may in reality be manuals of mysticism couched in chemical language. Fortunately many of Jābir's books contain, in quite unequivocal terms, easily intelligible accounts of chemical operations, substances and apparatus from which we can gather some idea of his technical ability and knowledge. This subject can best be treated in connection with the Latin works, for in this way it will be easy to see whether the latter show any advance over the Arabic works.

5. *The Content of the Latin Works compared with that of the Arabic ones.*—In this section, Russell's English translation of the Latin works will be quoted (London, 1678). Only a few of the most important points can be considered here.

(*a*) *Book of the Search of Perfection.*—A book of this title (*Kitāb al-Istitmām*) is mentioned in the list of Jābir's writings in the *Fihrist.* The Latin work deals with the structure of metals, "and teacheth how to perfect them." It says "all metallic bodies are compounded of argentvive and sulphur, pure, or impure by accident." Jābir (*Kitāb al-Īḍāḥ*, Cairo MS.) says: "The metals are all, in essence, composed of mercury, combined and coagulated with sulphur . . . they differ from one another only because of the difference of their accidental qualities." It should be noticed that one of Berthelot's main arguments against the authenticity of the Latin works was that they contained the sulphur-mercury theory of metals, of which (he claimed) the "œuvres arabes de Djâber n'offrent aucune trace."

The Book of the Search of Perfection mentions the following substances as useful in the process of "preparing" metals: salt, alums, atraments, glass, borax, vinegar. All were known to Jābir, as indeed would be expected. But the expression Jamenous Alum in the Latin book is noteworthy, Jamenous meaning "from the Yemen." Moreover, Jābir describes in many places the purification or "melioration" of vinegars by distillation. The proverb, "festination is from the Devil's part," is one of the most popular with Muslims.

All the processes for the "preparation" of the metals may be paralleled from the Arabic works, though they are in the latter not gathered together in one book.

(*b*) *The Sum of Perfection.*—The preface to this book may well

be a translation from the Arabic. The statement "what in other books written by us is diminished, that we have sufficiently made up, in the writing of this our Book," is in agreement with Jābir's remarks that he has scattered his information throughout several books. " Geber " says that search must be made for " Artifices which can follow Nature " ; Jābir remarks that chemists desire by artificial means to imitate Nature. The artificer, says " Geber," must be diligent in the work, persisting to the final consummation thereof. Jābir says that he who studies chemistry must have perseverance, and must not be deceived by appearances into bringing his operations to too hasty a conclusion. The arguments against those who disbelieve in the reality of alchemy (Latin work) remind us, that according to the author of the *Rutbatu'l-Ḥakīm* (eleventh century), Jābir had to contend seriously against this disbelief. This is borne out by the Arabic books, which do not, however, adopt so rigorous a method of " quæstio " and " respondeo."

The instructions which " Geber " gives for the construction and management of apparatus are similar to those given by Jābir in the *Kitab al-Khawāṣṣ* and elsewhere.

The fourteenth chapter of the *Summa* deals with Calcination. Jābir devoted a " book " to the same subject, part of which is fortunately extant. Compare the two passages which follow. (Latin):—" Calcination is the pulverisation of a thing by fire, through privation of the humidity consolidating its parts. The cause of the invention of it is that the adustive, corrupting, and defiling sulphureity, may be abolished by Fire. Yet it is diversified, according to the diversity of things to be calcined. For bodies are calcined, and spirits are calcined ; yea, other things also extraneous from the nature of these ; yet with a diverse intention." (Arabic, *Kitāb al-Taklīs*):—" The aim of the calcination of metals is to remove from them impurities, which are burnt up completely." " Each metal is calcined in a different way from the others. This is because some metals are found already pure ; in this case the object of calcination is to convert the body into a fine powder."

Practically all the operations, such as distillation, sublimation, ceration, etc., which the Latin works describe are mentioned more or less fully in the Arabic. The fact that " *Luna* returns, from the combustion of sulphur, into the nature of her proper body," is given by Jābir in the *Kitāb al-Khawāṣṣ* (Section 3): " If silver is beaten out very thin and covered with sulphur and then heated to the proper degree in the fire, it can be taken out and powdered like glass. If it is then returned to the 'judge of silver,' that is, the fire, and fused with borax, it returns to its original state."

(*c*) *The Invention of Verity.*—The preparation of nitric acid,

which is given in chapter xxiii, I have recently come across in a Cairo manuscript (Royal Library) of a work ascribed to Jābir.

These few examples may suffice to show us that there are marked similarities between the content of the Latin books and that of the Arabic ones. The list could even now be largely extended, and further research will no doubt add to it considerably.

6. *Conclusions.*—The present position of the problem, then, is very different from what it was a few years ago, and the following conclusions may be taken as definitely established:

i. " Geber " is Jābir ibn Ḥayyān.

ii. The reputation of Jābir as the greatest chemist of Islam is confirmed by a study of his works.

iii. The Latin works ascribed to Geber are probably not literal translations from the Arabic, but they are based on Arabic knowledge.

iv. There is a striking similarity between the content of the Latin works and that of Arabic works ascribed to Jābir. Whether this similarity will, through future research, prove to be great enough for us to conclude that the Latin works must, from the point of view of content, be regarded as genuine, time alone will show.

A NOTE ON THE NOMENCLATURE OF THE PROTEOCLASTIC ENZYMES

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The need for a logical system of naming and classifying the proteoclastic enzymes that will be acceptable to most workers in this field has long been felt, but so far little success in devising such a system seems to have been achieved. Different writers and different workers still use different names for the same enzyme, and much confusion has resulted from this practice. Recently two new systems of classification and nomenclature have been put forward that appear to be as unsatisfactory as anything that has been proposed previously. Dernby[2] retains the term *protease* as the general name for any enzyme that will hydrolyse a protein or an intermediate decomposition product of a protein. These proteases he then divides into three classes :

A. *Primary proteases*—corresponding to those frequently designated pepsinases—will hydrolyse native proteins as far as the peptone stage.

B. *Secondary proteases*—corresponding to those often called tryptases—will hydrolyse any protein, native or denatured, or any intermediate decomposition product of a protein, with the production ultimately of peptides or amino-acids.

C. *Tertiary proteases*—formerly called ereptases—will hydrolyse peptones or peptides into amino-acids.

This classification has been criticised by Oppenheimer[3] on two grounds. First, because the terms primary, secondary, and tertiary as used in the above connection are not rational,

[1] The suffix "clastic," as a substitute for the ambiguous termination "lytic" in enzyme terminology, was suggested by H. E. Armstrong (*Roy. Soc. Proc.*, **73** (1904), p. 500 ; *J. Chem. Soc.*, **57** (1890), p. 528). It has not yet received general acceptance, although, from the point of view of consistency, accuracy, and precision in the invention and use of scientific terms, its superiority is undoubted.

[2] *Biochem. Zeit.*, **133** (1922), p. 432.

[3] *Ibid.*, **136** (1923), p. 140.

since the so-called secondary proteases as well as the primary ones will act upon native proteins, although different products are formed in the two cases. And, secondly, on the ground that the classification implies that trypsin, for example, is a single enzyme, whereas Oppenheimer considers that the " preparation " called trypsin is a mixture of protein-splitting and peptide-splitting enzymes.

Oppenheimer himself suggests the following nomenclature, which, if found acceptable, he proposes to use in the forthcoming edition of his *Handbuch der Biochemie*. He would divide the proteases as follows :

A. *True proteases* will hydrolyse proteins as far as the peptide stage but will not decompose peptides:

1. *Pepsinases*—have their optimum activity in acid solution.
2. *Tryptases*—have their optimum activity between P_H6 and 8.

B. *Peptidases*, or *Ereptases*—will not attack proteins nor their higher intermediate decomposition products but will split peptides or peptones into simple amino-acids.

He further suggests that the term *peptase* should be discarded, as it is misleading.

Oppenheimer's nomenclature is also open to considerable objection. It is not consistent with the general principles that have for many years governed the naming of enzyme systems in the great majority of cases, and it is not easy to see what advantage this system has over any previous one. In 1919 the present writer published a paper [1] from which the following citation is taken :

" Some confusion exists in the terminology of the proteoclastic enzymes. Euler [2] calls them *carbamases* or *proteinases*, and considers that animal pepsin is a *pepsinase* and trypsin a *tryptase*. Vines [3] gives the name *peptase* to those enzymes which convert the higher more complex proteins into albumoses and peptones, and *ereptase* to those that split up albumoses and peptones into simple amino-acids.

" The custom is rapidly becoming general of giving an enzyme a name derived from that of the substrate on which it acts by changing the final letter or syllable into *ase*.[4] Thus, lactase is the enzyme which hydrolyses lactose, urease that which decomposes urea into CO_2 and NH_3. According to this

[1] *Biochem. Journ.*, **13** (1919), p. 124.
[2] *General Chemistry of the Enzymes*, 1912.
[3] *Ann. of Bot.*, **18** (1904), p. 289; **19** (1905), p. 171.
[4] This useful suggestion appears to have been put forward first by Duclaux.

nomenclature, the names used by Euler and by Vines[1] are obviously incorrect, for a tryptase would mean an enzyme that acts upon trypsin, and ereptase one that hydrolyses erepsin, and so on. It would be advisable, in order to bring the terminology of the proteoclastic enzymes into line with that of other enzymes, to apply the term *protease* to all those enzymes, whether animal or vegetable in origin, which will attack any of the proteins or their intermediate decomposition products. Those which attack the higher proteins only (*i.e.* those similar to pepsin) would be *peptases*.[2]

"Animal trypsin is anomalous in some respects and has both proteinoclastic and peptoclastic properties. It cannot be classed either as a proteinase or as a peptase, but only generally as a protease. It is not impossible, though, that trypsin may ultimately be found to be separable into a proteinase and a peptase.[3] Similar considerations apply to the vegetable ecto-proteases, *e.g.* of *Nepenthes*, which are considered to be mixtures of proteinases and peptases.

"This terminology of course in no way conflicts with, or renders unnecessary, the classical individual names pepsin, trypsin, and erepsin, for the three particular enzymes so designated. Thus pepsin and erepsin are two particular enzymes belonging respectively to the classes proteinases and peptases."

Apart from the objections discussed above, and anticipated in the citation, it is difficult on purely terminological grounds to justify Oppenheimer's classification of the true proteases, *i.e.* the writer's proteinases, into pepsinases and tryptases merely on account of the different relationships of their optimum activities to the reaction, *i.e.* the P_H, of the medium in which they act. It is confusing two totally different systems of classification. All divisions and subdivisions of a group of things should be made on the basis of variation in some one character (*fundamentum divisionis*) common to all the members of the group, in the present case, for example, the character of catalysing the hydrolysis of substances of a protein-like nature. Subdivision should then be made, if possible and desirable, into smaller groups, each of which should possess the common character with a difference (*differentia*), the subdivision being made on the basis of such a difference ; *e.g.* in the present case the obvious difference is the stage to which

[1] And also by Dernby (*loc. cit.*) and by Oppenheimer (*loc. cit.*).

[2] Or *peptidases*, after Oppenheimer (*loc. cit.*). Peptase, however, seems preferable, partly on account of euphony, but mainly on account of its wider denotation : a peptase would hydrolyse a peptone as well as a peptide.

[3] Vines, *Ann. Bot.*, 23 (1909), p. 1 ; Vernon, *J. Physiol.*, 31 (1904), p. 346, 32 (1904), p. 33. This is in agreement with Oppenheimer's views.

the hydrolysis is carried. The P_H[1] of the medium corresponding to optimum enzymic activity is surely more of the nature of a property (in the strictly logical sense of *proprium*) than of a differentia, and as such can hardly be regarded as a satisfactory basis of classification. But if such a system is employed at all, *all* the proteases should be classified on this basis. It is really an alternative system of classification rather than a supplementary one : all the proteases should be classified *either* on the basis of the substrates on which they act and the nature of the products formed *or* on the basis of the P_H values at which their activity is at an optimum. To combine both systems as Oppenheimer has done is contrary to the principles of logical division.[2] Such a course is analogous to classifying the chemical elements in each group of the Periodic Table according to the degree of their economic usefulness to man. As indicated, however, the former system certainly appears to have a sounder logical basis than the latter, and is the one that would be the more acceptable to the majority of workers.

The nomenclature suggested by the writer in 1919 appears adequate and is sufficiently general to accommodate itself to increasing knowledge of the properties of enzyme systems, such, for example, as the actual nature of the "preparation" called trypsin.

[1] No particular merit attaches to the old so-called neutral point: any other P_H value could be used with as much (or as little) justification as a means of differentiating between enzyme systems.

[2] *Cf.* J. S. Mill's *System of Logic*, Bk. I, chaps. vii and viii; F. W. Westaway, *Scientific Method: Its Philosophy and Practice* (London), 1912, chap. xviii.

SEX-LIMITED CHARACTERS IN BIRDS AND THEIR BEARING ON LAMARCKIAN THEORY, WITH NEW SUGGESTIONS CONCERNING THE GENETICS AND ORIGIN OF HEN-FEATHERING

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SOMATIC differences between male and female in mammals are known to be due to the fact that the normal development of secondary sexual characters in one sex depends on the presence of a hormone derived from the gonad of that sex. It is evident that they are inherited potentially by both sexes, that the difference is not one of heredity but of development. In considering the evolution of these characters the question is, how are we to explain the fact that they depend in this way on the sexual hormones ? Morphologically the characters have nothing in common except that they are generally external. Almost any external part of the body may show a special development peculiar to one sex or the other, having this peculiar relation to the sexual hormone. The only other common characteristic is that the somatic sex character plays some part in the behaviour of the animals in their sexual or parental relations. In the actions of the animals in courtship, fighting, or care of the young the organs (excluding colour characters) are regularly subjected to some special external stimulation. In the case of the antlers of stags the stimulus is the friction and laceration incurred in the process of fighting. The destruction of the skin must originally have caused the death of the bone which now takes place by heredity.

The Lamarckian theory of evolution is here the only one which corresponds to the facts. There are only two other possible explanations, namely, the theory of sexual selection and the theory of mutation. The mere survival of individual peculiarities cannot explain the limitation to one sex, or the greater development in that sex, as Darwin himself admitted. The sexual restriction or difference is explained by the influence of the sexual hormone, and there is nothing in the process of

selection to explain why the sexual hormone should influence the bones of the head any more than other bones of the body.

Selection cannot explain the development of the characters at puberty, or shortly before the attainment of sexual maturity, still less can it explain the annual recrescence of antlers in stags in correspondence with the annual season of sexual activity of the testes. On the other hand, no mutations are known which are related to the sexual hormones in the same way as somatic sexual characters are related to them. Unit characters which can be regarded as due to distinct factors in the chromosomes, and whose heredity follows Mendelian rules, may be considered to have probably arisen as mutations. Some of these are connected in heredity with sex, and some which are so connected have actually been observed to arise in the course of breeding experiments, as in the case of the fly Drosophila. A large number of such sex-linked characters are now known, and their heredity has been thoroughly worked out, but there is no resemblance between their relation to sex and that of somatic sexual characters. The heredity of sex-linked characters, such as colour-blindness, can be explained on the assumption that the factor, or rather the absence of a factor in this case, because the character is recessive, is in the X chromosome. The sex-chromosomes in the male mammal are indicated by the letters XY, in the female XX; the Y chromosome is inert, and has no effect in heredity. The defect in the male is therefore transmitted to the female. If in the female zygote, the defect occurs in both the X chromosomes, it will be operative in the individual developed from that zygote; if in only one, it does not show itself. A female showing the defect, if married to a normal male, can only transmit it to the male—that is to say, a colour-blind woman can only have colour-blind sons and normal daughters. In mammals a male can inherit no sex-linked character from his father; in birds a female can inherit no sex-linked character from her mother. There is no evidence that the actual development of a sex-linked character is in any way affected by hormones from the gonads.

A striking illustration of the difference between sex-linked and sex-limited or secondary sexual characters is afforded by the practical use of the former in poultry, in order to distinguish the sex of chickens as soon as they are hatched, whereas the ordinary somatic sexual differences cannot be observed until the chickens are about six weeks old. The silver or lighter colour of plumage as contrasted with the gold or brownish yellow colour is found to be sex-linked when the two are crossed. The female in birds, being heterozygous with respect to sex, transmits the sex-linked character, which in this case is dominant, only to her male offspring. Thus if a

Light Sussex hen is mated with a Brown Leghorn cock, the male chicks are all light-coloured, the female brownish, and this difference is evident in the down of the newly hatched chicks. This result is utilised in order to select the pullets with certainty, and get rid of the cocks, when the object in view is only egg-production.

The Lamarckian theory, which I have proposed as the explanation of the evolution of somatic sexual characters with all their physiological peculiarities, is as follows. There is evidence that the parts of the body which have been modified to form these characters are regularly subjected in the sexual behaviour of the animals to special stimulations, either of a direct mechanical kind, as in the butting of heads together in stags and other Ungulata, or of an indirect functional kind as in the erection of the feathers of the pea-cock's tail, or the male plumes of the bird-of-paradise. Secondly, there is evidence that stimulation of this kind produces in the individual hypertrophy of the living tissues affected. Thirdly, the hypertrophy produced in each generation necessarily causes waste substances to be given off from the growing tissues and, circulating in blood and lymph, to act upon the special parts of the chromosomes in the gametes of the same individual which correspond to the parts or organs stimulated. Fourthly, that, as the effect of this stimulation of the determinants in the chromosomes, there is developed after many generations a tendency to the hereditary development of hypertrophies, similar to those caused by the external stimulation. Fifthly, that the chromosomes throughout the evolution have been affected at the same time by the substances derived from the stimulated somatic tissues, and by the hormones from the testis or ovary, and consequently, when the parts of the body develop, they only manifest the power of increased growth, which has become hereditary, in the presence of the hormone of the testis or ovary. There are certain explanations and qualifications to be added. The most important is that the effect of mechanical irritation on living tissues is not the death or destruction of the tissues, though that may occur to some extent, but the reaction which shows itself in increased growth, as we see in the formation of a corn in our own epidermis. Another point is that it is not necessary to suppose that the hereditary growth reproduces the exact form of the injury caused by fighting, for instance, or even of the growth caused by the direct reaction; the hereditary effect is probably modified by the tendency to symmetry in the development of the organism, and is subject to the variations which produce species. Thus, the specific characters of antlers in the Cervidæ do not correspond to different peculiarities of fighting in the

ancestors. Lastly, it is not possible at present to explain the sexual colour-characters, such as those of the drake, except as being in some way due to the increased metabolism of sexual excitement.

The chief objection to my theory is the difficulty of believing that each different part of the same tissue, when stimulated to hypertrophy, gives off a mixture of waste substances peculiar to itself. It may be argued, as has been suggested, that the increase of one muscle would merely increase the total amount of excretory substances given off by the whole muscular system, and that on the theory the hereditary effect would be merely an increase, too slight to be significant, in all the muscles of the body. It is impossible to meet this objection satisfactorily at present; all that can be said is that the theory, if true, would explain facts that are unexplained on any other theory, namely, that somatic structures and characters, which are not in themselves sexual, but are used in sexual habits, are influenced in development by the sexual hormones.

The progress of knowledge in the case of Birds has shown that in this class the effects of castration are different from those which occur in mammals. Shattock and Seligmann found in the Plymouth Rock breed of fowls that the male plumage was not suppressed by castration, the only conspicuous effect being the reduction in size of the comb and wattles. In some breeds, *e.g.* Hamburgs and Leghorns, spurs occur rather frequently in the female, and in this case castration would not be expected to prevent the growth of the spur. This is not the case in the Plymouth Rock, and the systematic experiments of Pezeau (1918) showed that castration caused no change in the development of the plumage or the spurs. Many experimentalists have stated that the special male feathers are rather more developed in the capon than in the entire cock, but carried in a less erect position. Removal of the ovaries in ducks and hens, on the other hand, is followed by the development of the male plumage. Castration in the male causes rapid reduction of the comb and wattles to a size similar to that characteristic of the hen. In the castrated drake (Goodale, 1910) the male plumage persisted, but the eclipse, the condition after the breeding season in which the drake normally resembles the female, no longer occurred. The castrated duck, on the other hand, assumed the male plumage, which is so rich in colouring and so conspicuously different from that of the female. These experimental results closely resemble the cases which have been frequently recorded of female birds assuming the male plumage, especially in ducks, fowls, and pheasants. In such cases the single ovary normally present has been found on

post-mortem examination to be reduced to an extremely small nodule, in which development of ova had ceased.

These facts seem at first to lead to the conclusion that in dimorphic birds, such as Anatidæ and Gallidæ, the male characters of the plumage are the original characters, and are suppressed in the breeding female by the influence of the hormone from the ovary, in the absence of which they develop almost or quite completely; also that their development in the male is independent of the testicular hormone. Without further consideration this might appear enough to disprove the Lamarckian theory of the origin of the male characters. In reply to this, it must be admitted that we cannot suggest any external stimulation involved in the actions of the male which would cause the special coloration of the male plumage, though it is possible that the increased katabolism of the male associated with its greater excitement has something to do with the matter. But the greater development or hypertrophy of special feathers is a condition quite distinct from colour, and it is obvious that the sexual display of the male involving constantly repeated erection of the feathers constitutes a stimulation of the papilla from which the feather is produced. The greater size of the comb and wattles in the male Gallus behaves in relation to the testicular hormone much as mammalian characters do. We may then suppose that the hypertrophy of the special male plumage was originally caused by the stimulation of the feathers, but, for some unknown reason, is more strongly inherited than the male characters in mammals, so that it has become independent of the presence of the testicular hormone, but is suppressed in the presence of a foreign hormone, namely that of the ovary, which was never present when the stimulation of the male plumage occurred.

We have next to consider some still more remarkable facts concerning the relation of male plumage characters in birds to the hormones of the gonads. In fowls (Gallus) there are more breeds than one in which the male characters have disappeared in the male sex without suppression of fertility, giving rise to what is known as hen-feathering. This occurs in Sebright Bantams, Campines, and Henny Game. It also occurs as an occasional mutation in other breeds. The male plumage characters are, in the less modified breeds, such as black-red game, peculiarities of both colour and structure of the feathers. The colour of the hen, instead of being black on the breast and tail, and red or yellow on the neck and saddle hackles, is brownish yellow on the breast and a rather uniform speckled brown on other parts. The cock has long loose-barbed hackle feathers, tapering to a point, on neck and loins, and long curved tail-coverts, while in the hen the corresponding feathers

are short, blunt, and straight, with firm webs. In the Sebright Bantam the plumage in the cock is similar to that of the hen in both colour and form. In the larger size of the comb and wattles, and in the possession of spurs, crowing, and behaviour, the hen-feathered cock differs from the hen as much as the ordinary cock, and he is sexually potent, though perhaps less fertile than the normal male, for it is stated that the eggs often fail to hatch.

The extraordinary discovery was made by Prof. T. H. Morgan, of Columbia University, New York, that castration in these hen-feathered cocks causes the development of cock-feathering, which is the direct opposite of what we are accustomed to in mammals. This fact would cease to appear paradoxical if hen-feathering was due to a mutation which consisted in the production by the testis of a hormone similar to that normally produced by the ovary. The heredity has been studied experimentally from the Mendelian point of view, and is discussed by Prof. Punnett in his interesting volume, *Heredity in Poultry*. It is found that the hen-feathering in the cock is dominant to cock-feathering, so that a hen-feathered cock mated to a hen of a normal breed produces normal hens and hen-feathered cocks. On the other hand, when a hen of a hen-feathered breed (Sebright) was mated to a normal-feathered cock, some of the males produced were hen-feathered and some cock-feathered. The cock-feathered males bred true, but the hen-feathered produced both hen-feathered and cock-feathered sons in equal numbers. The Mendelian explanation which Punnett suggests is as follows :

In normal breeds we may assume that there is a factor which causes suppression of cock-feathering, and this is transmitted by the female only to the female. This factor, then, is that which causes the ovary to produce the hormone which suppresses cock-feathering. The female is heterozygous for sex, FM, and the male homozygous, MM. Call the suppressing factor H and suppose it linked with F, and we have—

$$\text{♀ FMHh} \qquad \text{♂ MMhh}$$
$$\text{Gametes : ♀ FH + Mh, ♂ Mh}$$
$$F_1 \text{ ♀ FMHh, ♂ MMhh}$$

Therefore, in each generation male feathering and female feathering as in the ordinary case. Punnett supposes that in hen-feathered breeds another factor occurs, which may be called H′. This factor must be one which is not sex-linked, and at the same time must have the property of causing the testis, while continuing to be fertile, to develop the same hormone as the ovary. The results of cross-breeding do not agree with any formula which locates this factor in either of

the normal sex-chromosomes ; it must segregate independently. In Prof. Punnett's case the Sebright hen must have been heterozygous for this factor having the constitution—

$$FMHhH'h'$$

the normal male with which she was crossed being—

$$MMhhh'h'$$

$$\text{Gametes}: \text{♀} FHH' + FHh' + MH'h + Mhh' : \text{♂} Mhh',$$

F_1	$FMHH'hh'$,	$FMHhh'h'$,	$MMhhH'h'$,	$MMhhh'h'$
	♀ Transmitting.	♀ Non-transmitting.	♂ Hen-feathered.	♂ Cock-feathered.

This would agree with the results as regards the males, for the cock-feathered birds did not transmit the hen-feathering, while the hen-feathered males were heterozygous, and, when mated with hens of a normal breed, produced hen-feathered and cock-feathered males in approximately equal numbers.

It is possible to simplify Punnett's formulæ. If we use the letters W, Z to signify the sex-chromosomes in the bird, the female is W Z, the male W W, and we can assume that the presence of Z causes the development of the ovary which constitutes the female sex and with it the hormone which in normal cases suppresses cock-feathering in the hen. We must also assume that the presence of two W's constitutes a male. We thus eliminate the factors H h since H becomes identical with Z, which in normal breeds is transmitted only to the female. Now suppose that by some process of non-disjunction, such as that which has been shown to occur in Drosophila, the heterozygous Sebright hen, with which Punnett experimented, had an additional Z chromosome which segregated independently, that in fact his H' factor was this additional Z. A normal breed then would have the ordinary sex-chromosomes, Female W Z, male W W. In the Sebright which was homozygous for hen-feathering, the sex-chromosomes in the hen would be W Z Z Z, in the male W W Z Z. In the heterozygous Sebright hen the chromosomes would be W Z Z and when a hen of this kind was crossed with a normal cock the segregations and combinations would be :

$$\text{Gametes}: \text{♀} WZ + Z + W + ZZ, \quad \text{♂} W.$$

F_1	WWZ	WZ	WW	WZZ.
	♂ Hen-feathered.	♀ Non-transm.	♂ Cock-feathered.	♀ Transmitting.

Goodale has shown experimentally in the Brown Leghorn that, although the removal of the testes does not suppress the

male plumage, this latter effect is produced, as might be expected, when portions of ovarian tissue are grafted into the castrated specimen.

T. H. Morgan has attempted to prove that the difference between the testes of hen-feathered cocks and normal cocks is visible under the microscope, the normal testis being destitute of the interstitial tissue-cells which are present both in the normal ovary and in the testes of hen-feathered cocks. Mr. Pease (1922) was, however, unable to confirm this.

The question arises whether these remarkable facts are enough to disprove the Lamarckian theory of the origin of somatic sexual characters. In my opinion they are not. The only difference in birds is that the special male characters, although not dependent on the presence of the testicular hormone, are unable to develop in the presence of the ovarian hormone. If the origin of the characters were not connected with the sexual organs there is no reason why the development of the special plumage should be affected by castration at all in either sex. The heredity, once established, is subject to mutation, like other hereditary factors, and in this case an extraordinary mutation has occurred which consists in the testis, while remaining fertile, producing the ovarian hormone. As suggested above, this mutation might be the result of a mere abnormality in the segregation of the sex-chromosomes. It may be suggested that the male plumage might, with equal probability, be explained as due to a mutation dependent on the absence of the ovarian hormone. But a little thought will show that this is a very different thing. Moreover, no such mutation has occurred under observation; we know of no case in which a sexual difference has appeared as a mutation, and has been found to depend on the hormones from the gonads. In the case of the Pouter pigeon, and the so-called Carrier, the inflation of the crop in the one, and the slightly greater size of the wattle in the male of the other, are associated with the sexual habits of the males, and cannot be regarded as mere mutations.

Another point which makes hen-feathering in cocks still more extraordinary is that, if it must be regarded as due to the secretion by the testis of a hormone similar to or identical with that from the ovary, the sexual instincts and the larger size of the comb and wattles are not affected, although these characters are also connected with the hormones of the gonads. This fact, however, is capable of some explanation. When a normal cock is castrated the sexual instincts, the habit of crowing, and the greater size of the comb and wattles are all suppressed, though the male plumage is not. If we suppose that these characters depend on a hormone, which is more

directly derived from the spermatic tubules and spermatic cells, we can understand why they are suppressed by castration and are not absent in hen-feathered cocks in which the spermatic tubules are developed and functional.

Prof. T. H. Morgan (Carnegie Institution, *Publication* No. 285, 1919), who first discovered the fact that castration of the hen-feathered cock causes normal cock-feathering to develop, considers that this fact leads to conclusions which make both the theory of sexual selection and the Lamarckian theory unnecessary as explanations of somatic sexual characters. " If the secretions of ovary and testis are some particular kind of substance the conditions that led to their appearance (*i.e.* the appearance of such characters) need not have been very complex. The hereditary genes generally have more than a single effect on the characters of the animal. The secondary sexual characters may then be only by-products of genes, whose important function lies in some other direction. If, for example, the secretion produced by the male cells of the male have an important influence on his output of strength or energy or activity, their secondary influence on certain parts of the body would not call for any further explanation on the modern view of natural selection. If the secretions of the ovary of the female bird have some direct relation to her physiological processes that are important in the development of the oviduct, for instance, it would be a matter of no importance from an evolutionary point of view if that same secretion suppresses in her the development of the high colour shown by the male."

Prof. Morgan's experimental work has resulted in very great advances in our knowledge in certain directions, but the above remarks seem to me to show that he has failed to appreciate the essential points of the question of the evolution of secondary sexual characters. If the characters are by-products of genes which give rise to gonadial hormones why are the characters different in different animals and absent in others? Prof. Morgan apparently supposes that these hormones are different in each genus or family, or even in each species, so that, for example, the stag has a hormone which produces antlers, man one that produces a beard, so that the injection into man of extract of stag's testes would cause the development of antlers on his forehead. All the evidence tends to show, on the contrary, that the gonadial hormones are not specific, but closely similar if not identical in different birds or mammals. Pezard,[1] in his experiments on castration of cocks, found that injection of testicular extract from cryptorchid swine into

[1] " Caractères sexuels secondaires chez les Oiseaux," *Bull. Biologique de la France et de la Belgique*, tome lii, 1918.

castrated individuals caused the comb and wattles to grow to their original size, and restored the aggressive male behaviour and challenging crow. The suppression of the male plumage in birds by the ovarian hormone does not explain why the plumage is so different in the pheasant, the peacock, and the bird-of-paradise, and why in so many species there is no male plumage to suppress. If the special developments were due to special properties of the sexual hormones, peculiar to each case, there would be no reason why these special developments should show any adaptive relation to the sexual habits of their possessors. In the frog, for instance, there would be no reason why the by-product of the hormone should be a pad on the thumb where it comes into contact with the female in amplexus, rather than a pad on the hind-foot, which does not touch the female at all. It may be said that the response to hormone stimulation would naturally be different in different animals, but that brings us to the crux of the whole question, namely, that we must either suppose the genetic constitution with regard to these characters the same, for instance in birds, and the gonadial hormones different, or the hormones the same and the constitutions different. All the evidence tends to prove that the latter is the truth. It has been shown that the hormone of the testis may be similar in its action to that of the ovary, but it has not been discovered that hormone of either testis or the ovary in one species is different from that of another in such a way as to determine the specific somatic sexual characters. It has been suggested, in other cases, that variations in internal secretions may be the explanation of evolutionary changes, but it must be remembered that in the thyroid, for example, the secretion appears to be essentially constant, and that the effect on different animals corresponds, not to the source of the secretion, but to the characters of the animal to which it is applied. The thyroid of a sheep, for instance, is necessary to its own development, but, when supplied to a tadpole, its effect is to cause the development of the normal legs of a frog, not the wool or the legs of a sheep.

There are cases in birds in which special nuptial plumes are equally developed in both sexes. Here there is no sexual dimorphism, yet the characters are clearly connected with sexual relations, for they are only developed in the breeding season. The Grebes (Podicipedidæ) and the Herons (Ardeidæ) supply typical examples. The habits and behaviour of the Great Crested Grebe have been observed and described in detail by Mr. Julian Huxley. In courtship the male and female face each other, erect the double crest and ruff, and shake their heads repeatedly at frequent intervals. In pairing, either member of the pair may assume the passive female attitude,

or the active male attitude, and in the Moor-hen an act of pairing in the normal attitude is immediately followed by another, in which the position is reversed. Mr. Huxley[1] remarks : " It would appear, in such cases, that the similarity of male and female internal secretion is so great that quite slight changes in nervous or metabolic activity can cause the nervous centres of the opposite sex's mode of behaviour to become activated." We have, then, to consider in the first place whether the similarity in nuptial plumage in the two sexes is to be explained, like the similarity of plumage in the hen-feathered breeds of domestic fowl, by a similarity of internal secretions from testis and ovary. We see at once that the conditions are not similar, but opposite to each other. In the hen-feathered cock the hypertrophied plumage of the male is suppressed by a secretion from the testis, similar to that from the ovary. In the Grebe hypertrophied plumage is developed in the female, as well as in the male, so that we should have to suppose that in this case the ovary had ceased to produce a hormone like that of the ovary in other birds. But there is no evidence of this. We might suppose that the development of the characters in the female had become independent of hormones from the gonads, which is possibly the explanation of the presence of antlers in the female reindeer. But in the actions of the birds in courtship we have, on the Lamarckian theory, a more probable explanation of such cases as that of the Grebe. We have only to assume that the two sexes, which are monogamous and take equal shares in the tasks imposed by breeding, have always practised similar actions in courtship. These actions involve the stimulation of the erected feathers, and if this results in hypertrophy which has become hereditary, the hypertrophy will have been developed and inherited in the presence of either the testicular or ovarian hormone—that is, equally in both sexes. But in the absence of either secretion, in winter, and in the immature stage of life these plumes are not developed.

With regard to the psychical factor in the sexual behaviour, it must be remembered that the sexual instincts, like the structural characters, are not genetically the exclusive possession of one sex or the other. Cows, for instance, obviously exhibit the instincts and reactions of the bull in a rudimentary degree. It is not necessary, therefore, to suppose that the male and female internal secretions are more similar in the Grebe and other such cases than in other birds, but merely as in the case of the nuptial plumes and behaviour in courtship that the instinct of the pairing action in either sex is excited

[1] *Essays of a Biologist*, 1923.

almost equally by the ovarian hormone and by that of the testes.

Similar reciprocity of habits and courtship and similarity of nuptial ornaments are mentioned by Mr. Huxley in Egrets and Herons in Louisiana. The ornamental plumes, as in the Grebes, are present only in the breeding season.

In insects, in which somatic sexual characters are often so conspicuous, removal of the gonads makes no difference to their development. Goldschmidt, however, is obliged, in order to account for his intersexes, to assume that the sex-chromosomes in the somatic cells give off hormones which determine sex and somatic sex-characters. The Lamarckian theory of the origin of somatic sex-characters is therefore not untenable in the case of insects. The so-called parasitic castration in Crustacea seems more consistent with the hypothesis that the parasitic Cirripede, which is predominantly female, *i.e.* has a large ovary, gives off an ovarian hormone to the host and thus causes the suppression of male characters, and the development of female. Here also the origin of the characters is more satisfactorily explained by the Lamarckian theory than by any other.

GERM-THEORIES OF TRANSFERABLE DISEASES FROM THE SEVENTEENTH CENTURY TO THE TIME OF PASTEUR

By CRANSTON WALKER, M.D., B.Sc.

In the year 1658 a versatile Jesuit described very minute bodies which he had observed with lenses, and which he concluded to be worms, in water, milk, cheese, vinegar, decaying vegetation, and in blood and pus from plague patients. He considered these worms to be the cause of putrefaction and of disease. These observations were published in Rome under the title "*Scrutinium physicum medicum contagiosæ luis, quæ dicitur pestis*, by Athanatius Kircher, S.J." [1] In the following year they were also issued in Leipzig. Kircher had held three professorships at Würzburg, was a prolific writer, and was credited with vast learning. His novel view of disease-causation therefore received considerable attention; but, although it was supported by several eminent men of science, including Borelli, it did not gain wide credence.

Christian Lange, Professor of Pathology at Leipzig, wrote a preface to the 1671 edition of Kircher's book, and expressed his conviction that puerperal and other fevers were due to putrefaction caused by minute worms. On the other hand, Nathaniel Hodges, an authority on the Plague of London, writing in 1666 of "the learned Kircher's late experiments," respectfully states: "In spite of good opportunities and most careful and industrious attempts, it has not been my happiness to discover any such minute worms or insects; and I can only learn of very few plague patients who vomited worms or gave other indication of harbouring verminous matter." The worms imagined by Kircher were much larger than bacteria. They had a fairly complex anatomy, and were capable of inflicting mechanical damage. His words could be applied more closely to *Acaris scabei* or to the Trichina worm than to bacteria.

Athanatius Kircher was born in 1601, near Fulda in Hesse-Nassau, removed to Rome in 1635, and died there in 1680. He wrote extensively on philosophy, mathematics, mechanics, physics, geology, meteorology, natural history, ethnology,

philology, ancient languages, hieroglyphics, and other subjects. He is still dimly famed as having devised a calculating machine, invented the " magic " lantern, and discovered hypnotic phenomena in animals. In each case his priority is disputed. With imaginative gifts and considerable scientific and literary skill, he combined an easy credulity and an unworthy taste for magic; so that his works contain numerous errors, and we are still burdened with an undignified name for the optical projector. His germ-theory was based on erroneous observation. It seems certain that he could not have detected bacteria with the instruments at his command, which are said to have been simple lenses magnifying not more than thirty-two diameters. Probably many of his supposed disease worms were tissue cells, blood-, and pus-corpuscles. In the year that Kircher's book on contagion was published Swammerdam described the red blood-corpuscles in his *Biblia Naturæ*; this work was, however, only published eighty years later, at the instance of Boerhaave. The first accurate description of blood-corpuscles to be generally available was that of Leeuvenhoek, published in 1674.

Of different temperament from Kircher, careful in conclusion and meticulously accurate in observation, was Anton Leeuvenhoek of Delft (1632–1723).

Leeuvenhoek did not use a compound microscope, but himself made and mounted simple lenses of such excellence that they served for a magnification of 160 diameters. Using these instruments with extraordinary skill, care, and visual acuity, Leeuvenhoek achieved a long series of observations which compel profound admiration, even in those who traverse the same ground equipped with every modern optical luxury. In 1785 Leeuvenhoek, in letters to the Royal Society, described excessively minute organisms which he had observed in exposed water, infusions, teeth-scrapings, in the intestines of many animals, and in diarrhœic stools. In 1683 he gave wood-cuts illustrating their forms. He also described different types of motility, including the somewhat gnat-like motion, which was later investigated by Robert Brown, named after him, and in our own time explained by Perrin. Leeuvenhoek was, without doubt, the first observer of bacteria. His reports aroused great interest, the minute organisms were observed in putrefying materials by many others, and, though Leeuvenhoek himself did not publish speculations, a casual connection between the organisms and putrefaction and disease was freely mooted; particularly by Andry (1701) [2], who, considering the bodies to be worms, set forth a germ-theory of disease in which Leeuvenhoek's organisms constituted the material basis for Kircher's speculations. The theory, though so near the

truth, after some preliminary excitement, was disapproved. It appears to have suffered from the uncritical advocacy of enthusiasm, and, gaining disrepute, remained in almost complete oblivion for a hundred and fifty years. Mankind seems to have been a little obtuse to the idea of a *contagium vivum* or *animatum*, and the doctrine of contagion itself found learned opponents even in the nineteenth century (Maclean).

In Daniel Defoe's *Journal of the Plague Year* [3], published in 1722, occurs the following striking passage (p. 86, Everyman edition): " I look upon with contempt . . . the opinion of others, who talk of infection being carried by the air only, by carrying with it vast numbers of insects and invisible creatures, who enter in the body with the breath, or even at the pores with the air, and there generate or emit most acute poisons or poisonous ovæ, or eggs, which mingle themselves with the blood and so infect the body."

Defoe was not romancing; he was trying to give a trustworthy account of the plague visitation and its effect on mankind. He used imagination to interpret documents, and to bridge gaps in his information; but trusted that where his narrative was unsupported by evidence it was still not far from truth. It is, therefore, unlikely that this germ-theory of plague, which he mentions only with contempt, was a pure fancy of his own. The epidemic which he describes took place in 1665. Defoe was probably in London, but was only six years old. The *Journal* was published when he was sixty-three. In the meantime there had been no plague in England, but numerous outbreaks had occurred in different parts of Europe.

In 1707 plague spread from Russia to East Germany, thence, about the year 1713, to Bavaria, Austria, and Styria. Infected ships from Sidon carried plague to Marseilles in early 1720, whence it spread over the greater part of Provence.

At Marseilles the plague raged with impressive virulence. The prosperous city was " within less than two weeks [rendered] the most dreadful scene of Human Misery that ever Destruction formed in any City of the World," so wrote one of its counsellors [4]. At one time 2,000 dead were lying rotting in the streets, and dead innumerable in the houses. This was only an arrear from a hitch in the organised disposals (all the disposers being plague-struck or dead), the sheriffs feared an accumulation of 15,000 in a week if the removal efforts were relaxed [5]. By October 20, 60,000 were dead and 14,000 left in the town [6]. Consternation and alarm spread over Europe, as may be still traced in pictures, prints, and literary allusions.

Defoe followed closely the events in Marseilles and based much of his book upon them. A germ-theory of plague was brought to discussion, and speculation was supported by refer-

ence to Lancisi's [7] work of two years earlier, in which he contended that malaria was caused by minute animalcules, which, besides mosquitoes, emerge from marsh water and attack the bodies of men. Varro [8], a contemporary of Cicero, Terentius Rusticus [9], Columella, and others had expressed a similar idea, but the writing of Lancisi was fresh to mind.

Goiffon, director of the Bureau de la Santé at Lyons, wrote in 1720: "Des insectes venimeux apportés de quelque contrée étrangère avec des marchandises, d'où ils se répandront dans les airs d'une ville, produiront tous les funestes effets qu'on remarque dans la peste." These "insects" are invisible, and may be as small, compared with a mite, as is a mite compared with an elephant; nevertheless, a sufficiently powerful microscope might discover them. Further, "Toute prévention à part, tant qu'on voudra établir la cause de la peste dans des sujets inanimés, on n'expliquera jamais avec satisfaction aucun des symptomes, au lieu qu'en supposant des vermisseaux, des petits vers, des insectes, des petits corps animés, l'on comprend sans tant de peine et de difficulté la multiplication de la cause de la peste . . . et son renouvellement après plusieurs années d'extinction ou de cessation."

Eggerdes [10] again, writing on plague in 1720–8, after first considering and rejecting several other theories, concludes that plague arises from a material poison-entity carried from place to place in fomites, that such poison must be organised and able to reproduce itself to infinity.

Such was the historical basis for Defoe's remarkable passage. Nor was the undeserved contempt with which he introduces it without equal foundation. Pestalozzi, a colleague of Goiffon's, wrote: "En voilà bien assez, Monsieur, en voilà trop! Ne faisons pas naître dans notre brillante imagination des insectes volants, qui se multiplient à l'infini, dont les générations ne cessent jamais, qui pullulent dans l'air, dans les marchandises et dans les corps animés! Qui pourrait résister à cette funeste engeance, où est-ce que cette vermine ne serait pas portée sur les ailes du vent? et quelle serait la retraite, où serait l'asile pour s'en garantir?" and stated a germ-theory succinctly in his derision. The "inconceivable insects" became a butt for popular ridicule. Bacteriology, even to-day, does not entirely escape.

Between 1725 and 1835 suggestions of a germ-theory of disease were few and were ill received. Linnæus [11] gave the idea his support by classifying Leeuvenhoek's minute organisms together with the supposed *contagia viva* of fevers in one group, which he named "chaos." He also suggested, in 1760, that these animalcules had periods of eating, sleeping, and repro-

ducing, and that the periodicity of certain fevers was connected therewith.

In the year 1762 Marcus Antonius Plenciz [12] of Vienna made a clear and logical statement of a germ-theory of infection. He maintained that the enormous increase of the infecting material, its carriage through the air, the latent period of many fevers, all pointed to a living agent. He asserted the constancy of character of each infective disease—a fact then not generally agreed upon—and argued that each separate disease had its own special excitor and no other. "Sicut enim ex certo contagioso miasmate certus et determinatus affectus et non alius evolvitur et propagatur." Meteorological conditions, then so freely evoked in pathological explanation, were only of importance in so far as they favoured the life of the infecting agent, or disfavoured that of its victims. The task of therapeutics was to find specific medicaments which were directly opposed to the infecting agent of each disease.

Reimarus [13] also, in 1795, expressed the view that the *materia morbi* of infectious diseases was something living, because of its power of increase, and "Dieses mit einigen Schriftstellern Insecten zu nennen scheint das Feine desselben nicht zu treffen"; they might be infusorian, or, for those days, ultra-microscopic.

In the latter half of the eighteenth century observations on micro-organisms gradually accumulated; their morphology was brought into some scientific order by Otto Müller of Copenhagen, while the experiments of Needham, Bonnet, and Spallanzani on spontaneous generation directed attention to their life processes and to their connection with fermentation. In the early nineteenth century micro-organisms received increasing attention, and in the year 1837 investigations of great importance were published. Cagniard-Latour [14] and Schwann [15], each independently, showed that yeast was a living organism and that alcoholic fermentation was intimately connected with its life and growth. In the same year Donné, as a result of observations and experiments on micro-organisms in syphilitic pus, raised the question whether such bodies might be the cause of syphilis and the means of its transmission. Bassi [16] achieved proof of the dependence of a disease on micro-organisms by demonstrating that a silk-worm disease was caused by yeast-like bodies, and was propagated by their being transferred from the sick worm to the healthy.

In the year 1840 Jakob Henle [17], Professor of Anatomy at Göttingen, impressed by the work of Cagniard-Latour, Schwann, and Bassi, set forth his conviction that all contagious and "miasmatic-contagious" diseases were caused by minute living organisms. His conclusion was based on a careful survey

of the facts of infection, and on logical deductions therefrom. "Er hält es für naheliegend das Contagium sich mit einem vegetabilischen Leib zu denken, da man täglich mehr die grosse Verbreitung, die rasche Vermehrung, und die Lebensfähigkeit der niederen Pflanzenwelt kennen lernt," and gave to the vague, thousand-year-old notion of a material contagium, invisible and continuous behind the apparent and discontinuous process of contagion, a local habitation in the bodies of minute plants. For proof that an organism caused disease Henle laid down three requirements at least, similar to those which Koch subsequently enunciated in greater detail. The organism must be found constantly in association with the disease, and in the living tissues ; it must be isolated ; it must then be tested by experiments, including animal inoculation. The correctness of Henle's views and methods was not demonstrated by himself ; the technique of the time was inadequate, and another twenty-five years elapsed before experimental proof began.

In the meantime the common pathogenic fungi were identified. That of Favus by Schönlein, and that of white-mouth by Langenbeck, both in 1839 ; the fungus of ringworm by Gruby and Malmsten in 1845 ; and that of *Pityriasis versicolor* by Eichstedt in 1846.

The first demonstration that a bacterium was a cause of disease was due to the influence of Pasteur. It was Pasteur's work, especially that on butyric acid fermentation, which about the year 1861 induced Davaine to investigate again the microscopic rods which in 1850 he and Rayer had discovered in the blood of cattle suffering from anthrax, and whose immense significance he had then completely overlooked. The bacilli had been observed independently by Pollender [18] in 1849 (published in 1855), and also by Brauell (1857–8), who considered them to be peculiar to anthrax, and even as early as 1838 by the veterinary Delafond. In 1860 Delafond, shortly before his death, had observed the bacilli to grow in warmed blood. By 1863 Davaine was able to produce a mass of evidence, further amplified by 1865 [19], that the bacilli and they alone were the cause of the anthrax disease. This conclusion was challenged, and occasioned much controversy in the succeeding years. Notable contributions in support of the bacterial thesis were made by Klebs and by Koch. Meanwhile, Pasteur was engrossed in the discovery and elucidation of two bacterial diseases in silkworms. When completely successful, with only a few opponents unsilenced, he was struck with paralysis and hardly expected to survive. It might well seem in 1868 that his labours for mankind were complete and ended. His most striking work, however, and perhaps his greatest, was still to come, and an important departure may be noted when, with incomplete

bodily recovery, in 1876, he took up the investigation of anthrax.

For two hundred years germ-theories of transferable disease had been discussed and rejected at intervals, before the truth contained in them began steadily to gain recognition. One seeks reasons for this delay, but, when an attempt is made to explain historically why something did not happen, the important reasons are probably contained in what the explainer does not say. The following suggestions, therefore, are put forward only in the most tentative fashion.

From Leeuvenhoek to the early nineteenth century the evidence cited for a germ-theory was epidemiological only. Experiment was lacking. And investigation of micro-organisms was delayed. O. G. Müller's *Animalcula infusoria*, the first work of importance since Leeuvenhoek, appeared in 1786; but Ehrenberg's more valuable *Die Infusions-tierchen als vollkommene Organismen* was fifty-two years later in publication.

In no disease was it generally agreed, until recent times, that propagation was solely or even chiefly by infection of the healthy from the sick. On the contrary, many authorities held that such a transference of disease was impossible—and this even after inoculation with smallpox to obtain an immune population had attained celebrity. They regarded the doctrine of contagion with sorrow and even anger, as being most misleading and mischievous, diverting attention from the truth, whose recognition must be the preliminary of effective action, that the victims of an epidemic were all infected from the same source. This source was usually suspected to be the air, and an immensity of effort was devoted to collection and consideration and meteorological and similar data.

The words " miasma " and " miasmatic-contagion " were used with ill-defined significance of a hundred different varieties, inviting as many varieties of misunderstanding. Incisive definition might have revealed the truth, as a dissector lays bare a structure or a sculptor reveals his conception in a block of marble.

A number of miasma-theories were entertained, though they were seldom clearly designated. The air might be altered, or might be merely the vehicle of the deleterious agent. A great many writers believed, or were willing to believe, that during epidemics the air was charged with multitudes of invisible particles which entered the body with the breath or perhaps in other ways ; caused a kind of fermentation, manifested as fever ; multiplied greatly ; and were given off again in the breath, dejecta, etc., repolluting the air. The final step of considering these particles to be living was omitted, perhaps because the

minuteness and simplicity of bacteria were not guessed at; and perhaps also because the study of fermentation had not yet shown clearly the potency of intangible quantities of enzymes.

A living thing which was without organic parts and almost without magnitude and yet powerful had to be demonstrated before it was imagined. The early germ-theorists suspected something more elaborate, farther removed from mathematical "particles." Except in the field of immunity, pathologists have seldom framed conceptions beyond the reach of sense-experience, such as have proved so immensely fruitful in physics and chemistry.

A single explanation was applied to a multitude of different diseases, whose common feature was merely that the cases were sufficiently numerous, contemporary, and contiguous to impress the imagination. The explanations often contained a good deal of truth, but truth presented inconveniently, as we see when lime-kiln coma, influenza, and malaria were, not incorrectly, described as miasmatic diseases. The air-borne particles have proved to be ultra-microscopic, microscopic, and macroscopic respectively, and of very different natures.

It is not surprising that the facts of malaria-transmission were not guessed at, but it does seem a little strange that the discovery of the microscopic and elusive parasite should have preceded and led to the incrimination of the easily visible one.

Acceptance of the germ-theory was also delayed far into the last century by the doctrine, maintained by many authoritative chemists up to and including the illustrious Gustav Liebig, that on the cessation of life its material basis fell automatically into dissolution. Involved with this conviction was the belief that the simplified materials then automatically combined themselves to make simple living organisms, a doctrine which Leeuvenhoek himself had refused to accept.

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ABRAHAM BENNET, F.R.S. (1750—1799)

By F. W. SHURLOCK, B.A., B.Sc., F.Inst.P.
Principal of the Derby Technical College

The inventor of the gold-leaf electroscope conferred a great and lasting benefit on electrical science. In the eighteenth century, experiments on frictional electricity excited the interest of natural philosophers in much the same way that experiments on radiotelegraphy do now, and the great problem was to find a satisfactory method of ascertaining the sign and measuring the amount of the charges employed, especially when these were small.

The instruments devised for this purpose were called *electrometers*. At first a single thread was employed ; subsequently John Canton fastened two small balls of cork or pith to the extremities of a linen thread which he suspended by the middle. But Canton's electrometer possessed various defects ; the threads twisted and the balls had an unfortunate habit of adhering when first charged and then separating suddenly ; moreover, the instrument was disturbed by air-currents.

Various attempts were made to remedy these defects and to increase the sensitiveness of the instrument. Cavallo, for instance, reduced the size of the apparatus, suspended each ball by a separate piece of thread the upper part of which was formed into a loop which moved in a ring of brass wire, and substituted silver wires for the linen threads. He also enclosed the instrument in a glass bottle.

Leaf-gold and leaf-brass were commonly used at this time to demonstrate the attraction of light objects by charged bodies. Abraham Bennet, about the year 1786, replaced the thread and balls of Canton's electrometer by strips of gold-leaf, thus obviating the defects and greatly increasing the sensitiveness of the instrument.

Bennet's *gold-leaf electrometer* became at once an important instrument of research and remained so until the invention of Lord Kelvin's quadrant electrometer. Since the use of instruments of the latter type became common Bennet's instrument has been known as the *gold-leaf electroscope*. In recent years, and in its modern forms, it has again become

prominent as an instrument of research, as it has proved admirably suited for the study of ionisation and radioactivity: nor should it be forgotten that it has throughout been indispensable to teachers of electrostatics.

Bennet has, however, suffered the common fate of the pioneer and, in spite of the benefits he has conferred on electrical science, his memory has fallen into undeserved oblivion and his invention has even been attributed to others. In the *Dictionary of National Biography*, for example, the invention of the gold-leaf electrometer is credited to George John Singer (1786–1817), whilst no account is given of Abraham Bennet. In these circumstances it may be of interest to record what little can be gleaned concerning him in the neighbourhood where he lived and where his scientific work was done.

The available sources of information are very meagre, and we have chiefly to rely on Bennet's memorial tablet on the south wall of the nave of Wirksworth church, and his book, *New Experiments on Electricity*, which was published in Derby in the year 1799.

The inscription on the tablet is as follows:

TO THE MEMORY OF

THE REV. ABRAHAM BENNET, F.R.S.

WHO WAS XXIII YEARS CURATE OF WIRKSWORTH

RECTOR OF FENNY BENTLEY;

DOMESTIC CHAPLAIN TO

HIS GRACE THE DUKE OF DEVONSHIRE;

PERPETUAL CURATE OF WOBURN

AND LIBRARIAN TO

HIS GRACE THE DUKE OF BEDFORD.

HE WAS AUTHOR OF A WORK ENTITLED

"NEW EXPERIMENTS ON ELECTRICITY"

WHICH ESTABLISHED HIS REPUTATION FOR SCIENCE

AMONGST THE PHILOSOPHERS OF ALL COUNTRIES;

HE DIED AT WIRKSWORTH ON THE VI DAY OF MAY MDCCXCIX

AGED XLIX YEARS.

Wirksworth is an ancient market town and is situated in a valley among the hills of the Low Peak of Derbyshire, about

thirteen miles to the north-west of Derby and about four miles from Matlock. In Bennet's day there were, of course, no railways, and the road from Derby to Matlock passed through Kedleston and Wirksworth, as the present main road to Matlock and Manchester did not then exist. The town was the centre of a famous lead mining industry, with which were associated many curious customs and rights ; the industry has now fallen into decay. There is evidence that the mines were worked in Roman and Saxon times, and it is said that in the eighteenth century the vicar's tithes of lead-ore amounted to £1,000 per annum. There is a fine church with an interesting history which is described in Cox's *Derbyshire Churches*, as is the church at Fenny Bentley.

Fenny Bentley lies about eight miles south-west of Wirksworth across the hills in the direction of Ashbourne. In Bennet's time it was a village with a population of less than two hundred, whilst the population of Wirksworth was rather less than three thousand. It appears then that Bennet came to Wirksworth as curate in the year 1776, at the age of twenty-six, and became rector of Fenny Bentley in 1796, so that he only held the latter post during the last three years of his life. It was at Wirksworth that his electrical work was done.

Adjoining the churchyard at Wirksworth was the Free Grammar School, founded in 1576 by Anthony Gell, Esquire, "for the education, bringing up and instruction of children and young men in grammar and other literature." The Rev. A. Bennet, M.A., appears in a list of masters, but nothing seems to be known of his university career. His name cannot be traced in the register of either Oxford or Cambridge.

The wording of the title-page of Bennet's book, *New Experiments on Electricity*, is reproduced on the following page, as it gives a clear statement of the subject-matter.

The work is dedicated to the Rev. Dr. Richard Kaye, F.R.S. (afterwards Sir Richard Kaye, Baronet), Dean of Lincoln and Trustee to the British Museum. Dr. Kaye, it may be noted, was vicar of Wirksworth from 1787 to 1790. The book was an octavo volume of some 158 pages in plain boards, and was published by subscription. There is an interesting list of over four hundred subscribers. Among them are no less than twenty-two Fellows of the Royal Society, of whom seven signed Bennet's certificate for election to the society, viz. : Josiah Wedgwood, Erasmus Darwin, James Watt, Joseph Priestley, William Withering, James Keir, Matthew Boulton, Samuel Galton, and Richard Kaye. He was elected on March 19, 1789. Other subscribers who were Fellows of the society were Sir Joseph Banks, Dr. Blagden, Henry Cavendish, and Mr. Nairn.

NEW EXPERIMENTS
ON
ELECTRICITY,

WHEREIN

THE CAUSES OF THUNDER AND LIGHTNING

AS WELL AS THE CONSTANT STATE OF

Pofitive or negative Electricity in the Air or Clouds, are explained;

WITH

Experiments on Clouds of POWDERS and VAPOURS ARTIFICIALLY DIFFUSED IN THE AIR.

ALSO

A DESCRIPTION OF
A DOUBLER of ELECTRICITY,

AND OF THE MOST

SENSIBLE ELECTROMETER YET CONSTRUCTED.

WITH OTHER

New Experiments and Difcoveries in the Science,

ILLUSTRATED BY EXPLANATORY PLATES.

By the Rev. A. BENNET, F.R.S.
Curate of WIRKSWORTH, *Derbyfhire*.

DERBY:
PRINTED BY JOHN DREWRY.—M,DCC,LXXXIX.

The Derby Philosophical Society appears in the list of subscribers. This society was founded by Erasmus Darwin in 1784, and Abraham Bennet was one of its members. Happily the history of the transaction can be traced in the Journal of the society. The following entries, made by the secretary, Richard Roe, appear:

				£	s.	d.
1788	Ap.	5.	Paid subscription for Bennet on electricity to Dr. Darwin		2	6
	Oct.	3.	Cash p. Mr. Bennet	1	1	0
1789	Aug.	6.	Postage from Mr. Bent.			4½
	Sept.	3.	Carr. from Mr. Bennet 6*d.*; por. 2*d.*			8
	Oct.	3.	Paid Mr. Bennet the remaining half subscription for his book		2	6
	Dec.	29.	Postage from Mr. Bennet			2½

From these entries it is clear that Bennet was an active member of the society. We find him paying his subscription as well as carriage (doubtless by carrier's cart), porterage, and postage. We also learn that the price of his book was five shillings, which the society paid in two instalments, one before and one after publication. The society purchased books of natural history and philosophy which were circulated to its members, and it was Dr. Darwin's practice to acquire such books for the society from time to time. Quite a number of members of the Philosophical Society were subscribers to the book also.

Eleven subscribers bear the name of Wright. It is clear from the names that they are mostly if not entirely members of the family of Wright of Derby, and it is not unlikely that the "Mr. Wright" is the painter Joseph Wright himself.

The list further includes the names of the Dukes of Devonshire, Northumberland, and Portland, Lord G. Cavendish, Lord Willoughby de Broke, Sir Richard Arkwright, and a number of names well known in the county of Derbyshire, e.g. Fitzherbert, Harrington, Harpur, Hope, Hurt, Mundy, Sacheverell Pole, and Strutt.

Finally we may mention His Royal Highness, Ferdinand, Archduke of Austria; Rev. William Hodson, Fellow of Trinity College, Cambridge; Rev. Christ. Hunter, Fellow of Sidney College, Cambridge; Freemasons' Lodges, No. 47, and No. 189 Macclesfield; and Mr. Volta, Professor of Nat. and Exp. Philosophy.

Bennet's book gives an excellent idea of the scope of the electrical experiments which were being made towards the end of the eighteenth century, immediately before the discovery of current electricity. It consists of an Introduction and eight sections.

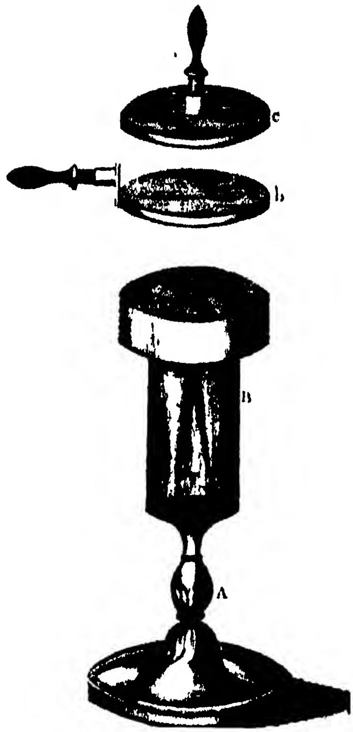

FIG. 2.—GOLD-LEAF ELECTROMETER AND SIMPLE DOUBLER

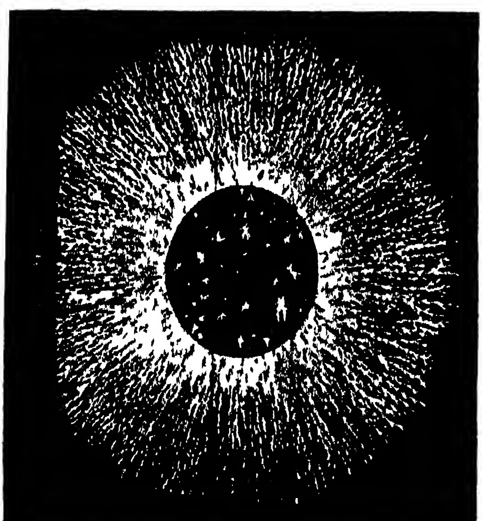

FIG. 1.—LICHTENBURG FIGURE.

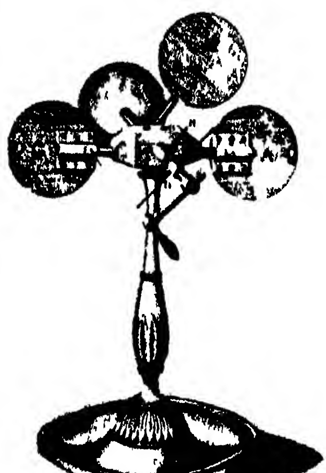

FIG. 3.—REVOLVING DOUBLER.

In the Introduction the author adopts Franklin's view that electricity is an extremely subtle elastic fluid. The topics discussed in the light of this theory are the distinction between conductors and non-conductors, insulation, attraction and repulsion, and the charge and discharge of the Leyden bottle. The electric field of a charged body is regarded as an electric "atmosphere" and we have the interesting statement that "if light conducting substances as *bits of gold leaf*[1] be brought within an electrical atmosphere, they become attracted towards the electrified body." Electrical phenomena are explained, though not very convincingly to a modern reader, in terms of these "atmospheres."

Finally the reader is warned that it is impossible to acquire an adequate knowledge of electrical science without an attentive performance of experiments, and the author suggests that the gold-leaf electrometer may, without partiality to his own contrivance, be recommended as the first instrument to be used, and that the use of the electrometer and the doubler will suffice for the instruction and amusement of the ladies as well as gentlemen who have honoured him with their patronage.

Section I contains the description of the gold-leaf electrometer and is quoted at length.

"DESCRIPTION OF A GOLD-LEAF ELECTROMETER

"This instrument principally consists of two narrow slips of gold leaf suspended in the middle of a hollow cylindrical glass.

"The foot A, plate 1, may be made of metal or wood, and about three inches high, that there may be convenient room to handle the instrument without touching the glass. The cylindrical glass B in which the gold leaf is suspended may be about five inches high, and two inches in diameter. The cap C is made of metal, and flat on the top, that the various substances whose electricity is to be examined may be conveniently placed upon it. The diameter of the cap is about an inch more than that of the glass, and its rim D is about an inch broad and hangs parallel to the glass to keep it clean and dry. Within this is another circular rim about half as broad as the other, made to go over or within the glass, and is therefore lined or covered with leather, or other soft substance, to make it fit close, and thus the cap may be easily taken off to repair any accident happening to the gold leaf. Within this rim and in the centre of the cap a tube is fixed, wherein the peg E is placed. To the peg, which is made round at one end and flat at the other, two slips of gold leaf F are fastened with paste, gum water or varnish.

[1] The italics are inserted.

"If gold leaf is used it may be shorter than silver leaf. The gold is much more sensible, but the silver is easier to cut and less liable to be accidentally torn. I have mostly used gold about two inches long, tapering to a fine point and fastened to the peg at the broad end. The breadth of the upper end of the gold is about one-fifth of an inch, which keeps the slips more exactly parallel, and the electrical repulsion is more sensible when the points are narrow, as I have observed when an accidental very narrow slip hung by the side of two parallel ones, the narrow slip always moving first.

"Without the glass the gold leaf would be so agitated by the least motion of the air that it would be entirely useless, and if electricity should be communicated to the sides, the gold would be attracted and torn, therefore two pieces of polished tinfoil G H are fastened with varnish on opposite sides of the internal surface where the gold leaf may be expected to strike, and are connected with the foot of the electrometer. The breadth of the tinfoil at the foot is one-fourth of the circumference of the glass, and it terminates in a point towards the cap, about as high as the peg to which the gold leaf is fastened. It is broad at the bottom, because there the points of the gold leaf are most liable to strike the glass, and being made narrow upwards does not prevent the repulsion from being easily observed.

"These slips of tinfoil, not only carry off superfluous electricity, but serve other important purposes, as will appear from these two experiments.

"*Experiment I*

"Upon a supposition that the gold leaf was attracted by the tinfoil, I suspended it in the open air, without a glass, and when electrified I brought two wires near it, and the gold leaves open'd wider, till they touched and collapsed, then upon bringing the wires still nearer they again diverged, which proved that the two pieces of tinfoil were useful to increase the sensibility of the instrument.

"*Experiment II*

"I fastened the gold leaf to the inside of an iron mortar hanging by a silk string with the mouth downwards, and though I communicated to it as much electricity as it would receive, the gold leaf did not diverge till a wire was introduced, which first caused the points to open, and then the higher part of the gold, as the wire came nearer to the place where the gold was fastened. This experiment shows that the tinfoil takes off the influence of the cap which would otherwise diminish the repulsion of the gold leaf.

"The upper end of the glass is covered and lined with sealing-wax, at least as low as the outermost rim, to render its insulation more perfect, for sealing-wax does not collect moisture from the air so soon as glass. In performing this operation, the glass should be gradually heated over a candle till it will cause the wax to flow uniformly over the surface, for if it be covered whilst the glass is cooler than the wax it will not insulate so perfectly. The foot may be about three inches and an half in diameter at the bottom, that the instrument may stand sufficiently firm.

"An electrometer of this kind has been carried from Birmingham to London; another from Wirksworth to York, and a third from Wirksworth to Etruria[1] in a portmanteau on horseback, yet without injury; it is therefore easy to make electrometers less in every dimension, especially in diameter, whereby their sensibility is increased, and inclose them in a proper case to carry in the pocket, to observe the atmospheric electricity whilst on a journey, or on the top of a mountain. For some purposes it may be also necessary to make them larger. In many experiments I have used one about five inches in diameter, which shewed the changes in the atmospheric electricity more distinctly than a smaller electrometer whose gold leaf would sooner strike the sides.

"The broad cap of this large electrometer was also very convenient for placing upon it red-hot crucibles or vessels of water in experiments on evaporation."

Section II contains an account of thirty-eight "experiments on clouds of powders and vapours artificially diffused in the air, and other experiments illustrating the principles of electricity, and shewing the great sensibility of the gold-leaf electrometer." The powders were for the most part blown from a bellows on to the electrometer. In several of the experiments a lighted candle is used in conjunction with the electrometer, and it is stated that "a lighted candle placed upon the electrometer very much increases its sensibility, and is peculiarly useful in collecting atmospheric electricity: but if the communicated electricity is not constantly supplied, it will also soon dissipate its charge in the air."

Section III is entitled "New Experiments with M. Lichtenburg's Large Electrophorus." Lichtenburg's figures were originally made on a resinous electrophorus by drawing over it the knob of a charged phial, and were rendered visible by sifting powdered resin over the plate. Bennet describes forty experiments in which chalk and other powders were employed with electric charges of varied strength. He also describes methods of fixing the figures on paper and other materials.

[1] Josiah Wedgwood lived at Etruria.

At Wedgwood's suggestion a successful attempt was made to fix the figures on porcelain by projecting fine powdered enamel and then baking the plate in the ordinary way. The frontispiece to the volume is a figure obtained by placing a circular brass plate with an insulating handle upon a resinous plate, communicating a spark from the charged bottle to the latter, removing it by the insulating handle and then projecting the powdered chalk.

Section IV is devoted to " experiments in which electricity is condensed or rarefied by the evaporation of water from various substances." Reference is made to Volta's discovery of electricity produced by evaporation of water from hot coals ; to experiments by Lavoisier and De la Place in which electricity is obtained when a gas is evolved by chemical means, e.g. by the action of dilute vitriolic acid on iron filings ; and to the Saussure experiments in which hot iron or other metal was plunged in water or water was poured into a heated crucible of iron or other material. Seventy experiments of this character in which the resulting charges were tested by the gold-leaf electrometer are then described.

Section V contains " a description of a doubler of electricity by which a very small quantity of electricity may be augmented till it becomes sensible by common electrometers, or visible in sparks."

The great importance of a machine for the purpose of detecting very small quantities of electricity is pointed out, as by its aid it was hoped that important discoveries might be made in atmospheric electricity, as well as in chemical experiments. The doubler, which is shown in the illustration above the electroscope, is thus described :

" This instrument in its first and simplest construction consists of two polished brass plates with insulating handles. The handle of one is fixed on the side of the plate, and the other on the middle, and standing perpendicularly.

" The plates are varnished on the underside, and the handles are made of mahogany, and fixed to the plates by insulating nuts of glass covered with sealing-wax.

" The method of collecting electricity from the atmosphere, and continually augmenting it till it became sensible, was thus performed.

" In dry weather I carried into the open air a lighted torch not liable to be easily blown out, or a small lantern with a lighted candle in it, to the bottom of which was fixed by means of a socket an insulating handle of glass covered with sealing wax. In the other hand was carried a coated phial. Then elevating the flame a little higher than my head I applied it to the knob of the phial, holding it in this situation about half

a minute. By this means I have found that more electricity may be collected than by an exploring wire insulated and fixed to the top of a church steeple, as practised by F. Beccaria. Having thus collected a sufficient charge I returned into the house and applied the knob of the phial to the cap of the gold leaf electrometer, upon which I placed the plate (b) touching it with the forefinger stretched over the insulating nut ; by this operation the electricity contained in the phial spreads upon the cap which serves as a condensing plate, and electrifies the plate (b) contrarily, because it is connected with the earth, and the varnish is interposed as a charged electric. The phial being now removed and the forefinger lifted up, the plate (b) is separated from the cap, and the plate (c) placed upon its upper side and touched by stretching a finger over the nut of its insulating handle, this last plate is then electrified contrary to (b) and the finger being removed, and the plate (c) separated from (b) it will be evident to electricians that the electricity of the cap and that of the plate (c) will be of the same kind, and nearly of equal quantity, so that the original charge is now doubled.

"I then apply the edge of the plate (c) to the side of the cap, and placing (b) as before, and touching it again, the electricity of (c) as well as that of the cap, both act upon the plate (b) and the intensity of its contrary electricity becomes equal to both ; then removing (c) which comes away unelectrified ; I take off my forefingers from (b) and lift it up, and placing (c) upon it I proceed as before, thus continuing to repeat this doubling process till the gold leaf diverges sufficiently to examine the quality of its electricity ; or if the gold leaf be first taken out, the process may be continued till sparks appear.

"In rainy weather the knob of the phial was applied to the insulating handle of an umbrella, or to a torch carried under it, and in this manner I continued to examine atmospheric electricity, till I constructed a more convenient apparatus."

"To prove that the electricity is doubled, it may be observed that the gold leaf opens to about twice the distance at each operation, and the application of the plate (c) to the side of the cap, or to a wire placed in it, does not diminish the divergency of the gold leaf though in this situation their electricity is diffused over double the quantity of surface, and admitting that the charge is doubled every time, which is not far from the truth whilst the intensity is weak, the twentieth operation will augment the first quantity of electricity above 500,000 times, and this process even with a doubler in its original and most imperfect state may be performed in less time than a minute."

Section VI describes "improvements of the electrical doubler, with experiments made to discover the causes and obviate the inconvenience of its adhering or spontaneous electricity."

The object of the doubler was to test the sign of small charges communicated to the instrument, by the aid of the gold-leaf electrometer. It was found, however, that the doubling process invariably produced electricity resulting either from a small residual charge or from accidental friction. Although the defect was not so great as to render the instrument useless, it was thought desirable to try to deprive the doubler of its "adherent or spontaneous" electricity. The attempts resulted in the "revolving doubler," in which the electrical operations of the simple doubler are performed mechanically.

"Dr. Darwin, at the desire of Lord G. A. Cavendish, made the first attempt with two plates moving between two others by a lever, so as to bring them exactly to the same position in each operation. This contrivance he soon improved by another instrument in which the plates stood vertically and moved by rack work in a direction exactly parallel to each other."

Bennet tried whether the plates would act without any resinous substance, found that the interposed air was a good substitute, and hoped that since it was not now necessary to varnish the plates, nor bring them into contact, the spontaneous electricity supposed to arise from the accidental friction of the plates would not be produced. He also made a doubler which consisted of three plates arranged vertically, the middle plate sliding backwards and forwards between the other two.

Dr. Darwin, in his *Phytologia*, has an illustration of Bennet's doubler in which the plates are arranged horizontally, the moving plate being attached to the pendulum of a clock. This was devised in order to keep a flower-pot constantly charged with positive or negative electricity and so to ascertain the effect of electrification on the growth of plants.

Finally Bennet contemplated constructing a revolving doubler, but before he had completed it he was presented with one made on the same principles by Mr. Wm. Nicholson which he considered the most satisfactory doubler yet made.

"This doubler consists of two insulated and immoveable plates about two inches in diameter, and a moveable plate also insulated which revolves in a vertical plane parallel to the two immoveable plates, passing them alternately."

"The plate A is constantly insulated and receives the communicated electricity. The plate B revolves, and when it is opposite the plate A, the connecting wires at the end of the cross piece D must touch the pins of A and C at E F, and a wire proceeding from the plate B must touch the middle piece G, which is supported by a brass, wooden, or other conducting pillar in connection with the earth. In this position if electricity be communicated to the plate A, the plate B will acquire a contrary state, and passing forwards, the wires also

moving with it by means of the same insulating axis, the plates are again insulated till the plate B is opposite to C, and then the wire at H touches the pin in C, connecting it with the earth, and communicating the contrary state of electricity to that of B, but of the same kind with that of A. By moving the handle still further B is again brought opposite to A, and the connecting wires joining A and C, they both act upon B, which is connected with the earth as before, and nearly double its intensity, whilst the electricity of C is absorbed into A; because of the increased capacity of A, whilst opposed to B, capable by its connexion with the earth of acquiring a contrary state sufficient to balance the influential atmospheres of both plates.

"Thus by continuing to revolve the plate B, the process is performed in a very expeditious and accurate manner.

"The ball (I) is made heavier on one side than the other, and screwed upon the axis opposite to the handle, to counterbalance the plate B, which may therefore be stopped in any part of its revolution."

Seven experiments illustrating the action of the instrument are then described.

Dr. Darwin thought Bennet's Doubler of Electricity "the greatest discovery made in that science since the coated jar, and the eduction of lightning from the skies." The instrument was thought highly of in scientific circles at the time, but was found unreliable and fell into disuse.

Section VII deals with "experiments on the adhesive electricity of metals and other conducting substances." In these experiments the doubler was deprived of electricity and the revolving plate brought opposite a fixed plate. The plates were then touched with different metallic substances, the doubling operation took place, the number of revolutions as well as the sign of the resulting charge being noted. The results obtained from lead-ore, lead, iron wire, tinfoil, and zinc are given in the form of tables. Experiments were also made with gold, silver, copper, and other substances, including various kinds of wood and stone.

Section VIII records "observations on atmospheric electricity collected with the flame of a candle." The observations, of which there are fifty, are prefaced by an account of atmospheric electricity. In these observations a cone of tinned iron was mounted at the end of a deal rod about ten feet long, from which it was insulated by a suitable cement, the rod being supported at an angle of about 50° to the horizontal. From the cone a small lantern containing a lighted candle was suspended by a short metal chain, and from the lower end of the cone a wire led to the cap of the gold-leaf electrometer. In some cases a kite was employed.

Bennet contributed four papers to the Royal Society, which were published in the *Philosophical Transactions* for 1787 and 1792. Two of these relate to the electrometer. The first is entitled, " Description of a New Electrometer," and is dated " Wirksworth, Sept. 14, 1786." The third paper bears the title, " An Account of a Doubler of Electricity, or a Machine by which the least conceivable Quantity of Positive or Negative Electricity may be Continually Doubled, till it becomes Perceptible by Common Electrometers or Visible in Sparks." The substance of these three papers is incorporated in the book, *New Experiments on Electricity*. In the fourth paper, published in 1792, which is entitled, " A New Suspension of the Magnetic Needle, intended for the Discovery of Minute Quantities of Magnetic Attraction ; also an Air Vane of great Sensibility ; with New Experiments on the Magnetism of Iron Filings and Brass," Bennet faces the problem of making a sensitive magnetometer. He suspended a small sewing needle, by means of a spider's thread, in the cylindrical glass of his gold-leaf electrometer, and found the arrangement very sensitive and remarkably free from torsion. The magnetism of brass is considered to be due to the iron it contains. Eighteen experiments are described in this interesting paper.

There is a quaint portrait of Bennet in the vestry of the church at Wirksworth. It is a half-length oil painting about 12 inches by 10 inches in size, and is rather cracked and discoloured. The portrait reveals him in profile and facing the left of the picture. He is wearing the bands distinctive of his profession, and the portrait reveals him as a sturdy, capable, clean-shaven divine with closely cropped grey hair, a broad forehead, straight nose, and firm chin. Before him in the lower part of the picture are his literary works. There is a roll of parchment which may represent his Royal Society papers ; beneath that is his book inscribed, *Experiments on Electricity*, and beneath that again what appears to be a book of sermons. This fixes the date of the portrait as between 1789 and 1799. It is to be regretted that the history of the painting is unknown. It is conceivable that it may be a study by Joseph Wright, with whom Bennet was certainly acquainted, or perhaps by one of Wright's pupils.

While we have full information as to Bennet's scientific work and know a good deal about his life at Wirksworth, we unfortunately know very little about his personal history. We know nothing of his parentage or education, or whether he married. It would be a happy circumstance if these pages should meet the eye of a reader who can supplement the information which they contain.

THOUGHTS ON MEDICAL DISCOVERY[1]

By Sir RONALD ROSS, K.C.B., K.C.M.G., F.R.S.

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When you did me the honour of inviting me to deliver the Inaugural Address this evening, I determined to risk your indulgence by taking as my subject not any particular line of medical work, but the wider theme of Medical Discovery. By the word " discovery " I mean any advance, however small, in general knowledge ; and by the words " medical knowledge " I mean all those branches of knowledge which are concerned in the prevention or cure of disease of any kind. Discovery is thus the increment, the differential coefficient of the curve of knowledge ; and I propose to submit for your consideration to-night some thoughts which I have long endeavoured to mature regarding what may be called " the natural history of discovery," especially how it came to be made in the past and how it can best be encouraged in the future.

On commencing a preliminary survey of the field, we shall see at once that discovery and the process of search (which sometimes leads to discovery) are very common phenomena in the whole of nature. Thus rivers and winds and even the solid masses of the mountains are compelled to seek and to find their most convenient courses and configurations. Plants and animals must not only discover their food but also those environments which suit them best ; and in the higher scales of animal life, in the nests of bees, ants, birds, and many mammalia, we observe more conscious efforts of research and discovery always proceeding in the wonderful world around us. When we come to man we witness the higher development of the process in modern science and civilisation.

What strike us most in the picture are first the amazing extent and yet the slowness of the advance ; and secondly, its intermittent or, rather, its undulant course. How many centuries have elapsed since our ancestral species first descended from the trees and took to hunting on the hard ground not

[1] Being the Inaugural Address of the 188th Session of the Royal Medical Society, delivered at Edinburgh, October 17, 1924. Published also in the *Edinburgh Medical Journal*.

even the palæontologists can tell us with certainty: evolution itself is a part of discovery, unconscious or conscious. After that, weapons, habitations, and clothing were, we may opine, the earliest discoveries; and then came crops and herds, fire, boats, and wheeled vehicles, the use of animals for draught or riding, fortifications, villages and cities, and social ordinances. We are not disposed to contemplate with pleasure or pride the earliest members of our own profession, the "medicine men" of savage tribes; but nevertheless they must have been not only the first physicians but also probably the first priests and philosophers. In the barbarous tribes of to-day—as for instance the Andamanese, which I know—we can still witness the position reached by men before the subsequent accumulation of discoveries and inventions created what we call our present civilisation, our own age of engines, electricity, radiations, and scientific inquiry into everything. The interval is immense. I have heard it argued, on the contrary, that after all discovery and invention have not given us much; but one wishes that those who say so would go and live, let us say, in an Andamanese jungle without even clothes and bows and arrows—which were only the first gifts of discovery!

But, as I said, the advance has been amazingly slow until recent times. We cannot estimate the ages which were required to evolve such simple implements as pottery and bows and arrows. The Stone Age and the Bronze Age occupied many millenniums before we came to the Iron Age. Seven thousand years have elapsed since the earliest known civilisations of Egypt, Mesopotamia, India, and China; and, until the last few centuries, life was not so very different in Europe from what it was in those countries so long ago. Personally, I always suspect that the archæologists underestimate the depths of the past; or that still older civilisations will be disclosed some day. What untold numbers of human beings have existed since those times! and yet progress has been so slow.

This is my first point. If I were to argue that on a general average not one man in a million ever adds anything to general knowledge, I should probably be greatly overestimating the proportion. This is an age of discovery, and yet the suggested proportion is conceivably too high even for to-day. What must it have been in many previous times? We know from history that whole ages have elapsed without adding any noteworthy discovery or invention to the stock of human knowledge. Civilisations have grown up in single spots and then dwindled away, leaving great periods of time during which not only have no advances been made but previously acquired knowledge has faded and been forgotten. Sir Flinders Petrie, in his remarkable *Revolutions of Civilisation* (Harper, 1912), recognises no less than

eight cycles of alternating advance and decay in Egypt during the last ten thousand years. In each of these, progress in discovery, invention, and art occurred only during a short period at the height of the cycle and was then followed by decay. One reads much the same thing in the histories of China, India, Mesopotamia, Greece, and Rome. In the slack periods, little or no additions to knowledge at all are made. The history of science is eloquent on the point. For example, ancient mathematics practically began with Thales and Pythagoras about 620 B.C. and almost ended with Diophantus about A.D. 280—a period of only nine hundred years. This was followed by almost utter darkness as regards mathematics for a period of twelve hundred years, until the renaissance of the science with Ferro, Cardan, and Copernicus in the fifteenth century. Now, it is remarkable that the same great period of Græco-Roman mathematics covers also the great periods of Græco-Roman philosophy, medicine, literature, art, and military predominance, all of which commenced and ended at about the same epochs. These are well-authenticated facts: what is the explanation? Is it that the intellectual efflorescence of nations is limited by some great law of nature which we perceive only dimly at present?—that there is some periodic law as regards the vigour of nations, which automatically ordains their advance and their decay?

If we can define science as the discovery of natural laws, invention as the discovery of processes, and art as the discovery of beauty, we shall obtain a single formula to include all those movements which make for what we call civilisation; and from the very brief survey which I have just attempted we can already extract two propositions with certainty, namely, that discovery is a rare phenomenon, and secondly, that it tends to be closely localised both in place and in time. After the Græco-Roman period of discovery lasting about nine centuries, there ensued a long negative period of about eleven or twelve centuries, during which almost no advance of any kind was made. One can not only read about the decay, as in Gibbon and in Sir J. G. Frazer's essay on Pausanias, but can almost witness it in sculpture as, for instance, in the upper carvings on Trajan's Arch in Rome. Then, suddenly, in the fourteenth and fifteenth centuries the re-birth began almost simultaneously in every line, in art, literature, science, exploration, and philosophy; and we are to-day in the fifth or sixth century of that astonishing renaissance.

Sir Flinders Petrie's hypothesis as to the cause of these periodic augmentations of intellectual vigour is that they follow the admixture of races by permeation or conquest—that is, that they follow what may be called a *racial* zygosis somewhat

as biological fertility follows ordinary sexual zygosis. Some centuries after permeation with a foreign race the vigour of a people begins to increase, its arts and sciences are born or reborn, and its wealth and prosperity rise to a maximum: then come regression and decay, to be followed by subsequent invasions and conquest; and so on, in cycle after cycle. I think that something very like this is shown in the histories of the older peoples of the world in Egypt, India, and China; and the present vigour of Europe would, according to this hypothesis, be due to the great mixture of races which followed the downfall of the Roman Empire. Of course it is not necessary —and is indeed unlikely—that all the world's nations should be simultaneously in the same stage of development; and we should say that India and China are to-day in the slack stage compared with Europe; and that Europe is approaching the slack stage as compared with America. To summarise: the hypothesis is that great intellectual achievements are not produced by any nation at a constant flat rate through time but only during short and successive periods, like the efflorescence of plants; that in the intervals the boughs are bare or covered only with leaves; and that the flowering does not necessarily occur in all nations simultaneously. The best examples are perhaps the great Periclean Age of ancient Greece and the later Augustan Age of Rome.

But my proper subject to-night is *medical* discovery. Here, so far as my own reading instructs me, we find exactly the same phenomena. Our knowledge of ancient Egyptian, Sumerian, Indian, and Chinese medicine is very small; but advances in all these countries were apparently periodic, though, I fancy, they were probably much greater than we can now judge from the very few inscriptions or books on medical subjects which survive. For example, the great Indian medical works were compiled within the second to the seventh century A.D., and showed considerable knowledge. It is not true that —as has been claimed—Susruta (fifth century) attributed malarial fever to mosquitoes; but these books indicate some discrimination of many diseases, including diabetes, and also of many drugs; and they describe a hundred and twenty-one surgical instruments, many of which "were properly handled and jointed, the blade instruments sharp enough to cut a hair and kept clean by wrapping in flannel in a box"—to quote from Dr. Fielding H. Garrison's admirable *History of Medicine* (Saunders, 1917).

This was, of course, after the great period of Greek and Alexandrian sciences, from which much of the Indian medical learning (like Indian mathematics) was quite possibly derived—though Indian physicians and surgeons were well recognised

as early as the time of Alexander. Both the Greeks and the Indians seemed to know that rats are connected with plague. The Greeks certainly connected malaria with marshy soil, and were able to distinguish the tertian, subtertian, and quartan varieties; and Empedocles of Agrigentum, in Sicily, was said to have cleared that town of malaria by removing its marshes as early as the fifth century B.C. (I suspect that the reason why he threw himself into the crater of Etna was because of his disgust at the indifference of his countrymen towards anti-malaria measures!) I need scarcely discuss Hippocrates (460–370 B.C.) here, but may repeat the old complaint that ancient medicine went very slowly after his time. Nevertheless there was some considerable advance after him and the Alexandrian anatomists to Galen (A.D. 131–201), whom also I need not particularise. The whole period lasted about seven hundred years, and then the sun of medical science set for nearly thirteen centuries, except for those faint reflections of its ancient glory which still haunted that long night. Now observe another fact. The entire epoch of Græco-Roman medicine produced only a few men who added materially to medical science. The excellent chapter of Dr. Garrison's work, for example, which deals with this epoch, contains only eighteen black-letter names, including Hippocrates and Galen, in the whole seven centuries. This is a smaller number than that of the poets or of the mathematicians of the same period, and, of course, a much smaller number than that of the orators, politicians, soldiers, and sovereigns. Yet during the same time there must have lived very many thousands of physicians, surgeons, and apothecaries, of whom a considerable proportion must have been able clinicians. Such numbers and such comparisons prove the great rarity of discovery even in a scientific profession such as the medical profession. But, of course, we possess only a minute fraction of the medical works of that epoch, some of which, now lost, would probably or almost certainly be recognised by us to-day with our more advanced knowledge as having been of first-rate importance, though they were perhaps not recognised in their own time as such.

The revival of learning, or rather of discovery, commenced in the fifteenth and sixteenth centuries, nearly thirteen centuries (as I have said) after the death of Galen; and was at first largely concerned with refutations of that somewhat imaginative writer. The original observations of the new era began with those of the great Italian anatomists, Vesalius, Eustachius, Fallopius, and Fabricius, who reconstructed the science in the sixteenth century. Harvey published his demonstration of the circulation of the blood in 1628; and the microscope, the telescope of biology and medicine, began to be used

by Kircher, Hooke, Swammerdam, van Leeuvenhoek, and Malpighi shortly afterwards. In the latter half of the same century (the seventeenth) Redi disproved "spontaneous generation." Towards the end of the eighteenth century Jenner's great discovery of vaccination opened the study of immunity against disease; and in the nineteenth century Lister, Pasteur, and Koch created bacteriology, Küchenmeister, Leuckart, and Laveran developed parasitology, and rapid advances were made in every branch of medicine.

I do not repeat these familiar facts for the mere pleasure of doing so—though I know of no greater pleasure than that given by the enumeration and description of the great triumphs of the human intellect: I wish to discuss the "natural history" of these discoveries. In Dr. Garrison's book the names of the men who made the leading advances in medicine are entered in black-letter, and on counting them we shall obtain the following results. During the century and a half from A.D. 1450 to 1600 there are 46 names; during the seventeenth century there are 79 names; during the eighteenth century 122 names; and during the nineteenth 446 names. There are altogether nearly 700 names in the four and a half centuries from 1450 to 1900. Now, this is an "outside figure," because Dr. Garrison has been careful to mention every person who has added materially to medical knowledge during the period, though some of the names, such as Linnæus and Darwin, are certainly famous for collateral rather than for medical work, and many others are the names of distinguished clinicians; so that a few of the names should perhaps have been excluded from a strict list of "discoverers" in medicine. On the other hand—especially for the nineteenth century, before we have reached the perspective of distance—some investigators whose altitude is only now becoming apparent have perhaps been omitted; but these exceptions count against each other, and Dr. Garrison's list is perhaps as accurate as possible under the circumstances. What conclusions can we draw from it? During the same period many thousands of millions of human beings have lived and died throughout the world; and yet only about seven hundred of them altogether have added materially to medical knowledge. Secondly, nearly all of these seven hundred lived in the comparatively small proportion of the world's surface included within Italy, France, Britain, Holland, Belgium, Scandinavia, and Germany. Thirdly, these men were only a small proportion even of all the medical practitioners who had lived during the same period—there are more than thirty thousand medical practitioners in Great Britain to-day; but fourthly, many of the most important advances (such as those of Jenner, Lister, Koch, and Laveran)

were made by men who were "private practitioners" at the time.

These facts are doubtless already familiar to you all; but they provide much food for thought. The total number of medical "discoverers" is probably less than the numbers, let us say, of poets, artists, or musicians of the same period, or of mathematicians or physicists; and much less than the numbers of politicians, generals, or kings living in all countries at the same time—not to mention the philosophers, who are always very numerous. The proportion of medical discoverers in Dr. Garrison's list to the total population of the world is probably nearer to one in ten millions than to one in a million as previously conjectured in this paper; and the proportion of them to all medical men is probably something like one in a thousand.

But this gives rather a false estimate of the real work which is being done in medicine—to-day, at least. If we count only the generals of an army that army will be but a small one. Actually an immense amount of work is being turned out daily by the rank and file—though medical history can take cognisance only of the leaders; and we have but to read our copious contemporary medical press to convince ourselves of this fact. A very large proportion of the medical profession of to-day add something, however small, to the store—contribute their mite to the mass of knowledge, lay a brick or two to build the magnificent palace of science. We are apt to overlook this constant but almost inappreciable accretion. There are no strikes in the building trade of science! Day after day, year after year, the structure grows before our eyes. The men whose names appear in the medical anthologies are those who dug the foundations, or perhaps those who have completed the towers and the pinnacles; but it is the thousands engaged in treating the sick who have actually raised the walls of that august temple.

I remember that before the war several of our colonies laid it down among the duties of their medical officers that they should "engage in research." What an easy rule to make! The popular notion is that a man has only to glance through a microscope in order to find something new. A friend of mine once said that the world looks upon any doctor as a person who, on returning home after a hard day's work, pulls out his watch and exclaims, "Ah! I have half an hour to spare before dinner. I will just step down to my laboratory and make a discovery." As a matter of fact it is impossible for most busy practitioners to indulge in such a time-wasting and often fruitless or nerve-wracking pursuit as deliberate, meticulous, and exigent investigation. What many of them really do,

however, is to collect an enormous mass of observations on the symptomatology, the pathology, and the treatment of diseases; and it is on this basis that the most important advances are made by subsequent diligent laboratory-work or brain-work. After all, the brain is the best and last laboratory. Observation is essential but, by itself, is not enough: we have to co-ordinate observations. The ultimate object of science is not merely to make and to record observations, but to classify them in groups, and then to show that one group of them depends upon another group—that is, to solve problems. Thus many astronomers collected data about the distances and the movements of the heavenly bodies; but it was Kepler who showed that all the planets move in similar elliptic orbits; and it was Newton who explained this fact by his law of gravitation. For another example, many surgeons had studied, described, and despaired over septic wounds; but it was your great fellow-townsman Lister who discovered how septic wounds were caused and could be prevented. Long before Jenner, milkmaids and farm-hands believed that if they acquired cowpox they would not acquire smallpox; but it was Edward Jenner who co-ordinated such statements, tested them experimentally, and created vaccination.[1] Science requires not only observation but proof, and not only proof but explanation. Now, it is one thing to make observations, but quite another thing—and usually a much more laborious process—to verify them and to explain them.

It is a very common fault, especially with medical writers, to confuse conjecture with proof. Thus, even Oliver Wendell Holmes wrote in 1883 that—

"It (medicine) learned from a monk how to use antimony, from a Jesuit how to cure agues, from a friar how to cut for stone, from a soldier how to treat gout, from a sailor how to keep off scurvy, from a postmaster how to sound the Eustachian tube, from a dairymaid how to prevent smallpox, and from an old market-woman how to catch the itch insect."

Alas! most of these were merely conjectures. The proofs were obtained afterwards by the long-continued labours of medical men. Without such proofs—well, the conjectures would have remained only conjectures. Proof is as necessary in science as it is in the criminal courts. I have recently been studying cancer and have been told by half a dozen distinguished medical men exactly how it is caused. Unfortunately all their explanations were different. The man who *proves* how it is

[1] It has been left to Italian publishers, R. Lier & Co., 7 Via Brera, Milan, to bring out a facsimile (12*s*. 6*d*. post free) of Jenner's great *Inquiry into the Causes and Effects of the Variolæ Vaccinæ*, London, 1798, in honour of Jenner's centenary last year.

caused will be the discoverer—and a great one. Conjectures are useful only as clues for future trial. Unfortunately they are apt to be many and to lead in different directions. Sometimes indeed they guide the research; but more often they confuse and betray it.

I said just now that discoverers in medicine are certainly not more numerous than those in the other sciences or arts. This again is a subject for surprise. After all, medical science concerns every living person somewhat closely; it concerns the health and even the existence of the thousand and more million people now living. Prosperity, happiness, and even virtue are largely matters of personal health. Perhaps I might argue that medical matters are really more important to men than astronomy, physics, art, and even politics and philosophy. After all, what happens to the star Sirius is not so vital to me personally as what happens in my own digestive organs. You may call me a gross materialist or even a grosser utilitarian: well, I confess that I am. And if all men were the same they would suffer from less indigestion and other ills. To put it briefly, medical discovery is to us human beings even the most important of all kinds of discovery, as defined in this paper. Yet to judge from the public attention given to it, it is the least important. Men seem to pass it over as if it were of no consequence; they hand it over in mass to the "doctors"; and they are good enough to subscribe almost no money to the prosecution of it. The total fund now allotted for medical research in Britain consists of about £130,000 a year given to the Medical Research Council; and something like (I cannot ascertain exactly) the sum of another £50,000 a year for pure medical research, as distinct from teaching, provides for the numerous research laboratories and institutions. The total would amount to about £180,000 a year, if I am not far wrong. What a gigantic sum this is—what a generous contribution from this wealthy state to the science which is concerned with the life and health of all the people! Think of it, £180,000 a year! That is to say, one penny per annum given by each living person in Scotland, England, and Wales. What generosity! For this gigantic penny every one hopes to have his life prolonged to at least the maximum of the three score years and ten laid down by the Psalmist. Think of it again; during seventy years each person will have given seventy pennies, that is, a total of 5*s.* 10*d.* for this cause. In the meantime every such person will have given many pounds to pay for his politicians, his army and navy, his local municipal affairs, and his education.

The truth is that the mass of the people do not in the least even yet recognise the effect of medical discovery. I see from

the Annual Report of the Medical Officer of Health for the County of London for 1923 that in 1841 the average expectation of life for every individual in London was 34·6 years among males and 38·3 years among females; and, that this period was prolonged last year to 53·8 years among males and 59·1 years among females. This means that the average length of life has now been prolonged by twenty years, that is by more than half of what the average length of life used to be in 1840. Of course many factors have been concerned in this wonderful addition to life, but I think that one of the most potent factors has been the general increase of medical knowledge during that period of eighty-three years. Now perhaps if this fact can be violently rammed into the thick head of the general public, it might produce some effect upon the tightly drawn purse-strings of the same. If the public some day acquires sufficient sense, it will largely increase its contribution for medical science. I think that we doctors could easily arrange to spend on medical research a sum of not only £200,000 a year as at present, but of quite £1,000,000 a year; and, what is more, the general public would really benefit by the expenditure—which is probably not the case if public expenditure were to be correspondingly increased in many other lines, such as politics or even education. I am glad to say that many more rich persons are now beginning to bequeath considerable sums for medical research; they will be wise to do so more frequently still. I have no doubt that homes for paupers and cats, and that scholarships for young men and women who hope to make discoveries some day, if they have the brains for it, are laudable objects of bequests; but I think that bequests and donations on other lines for research in medical and other sciences are likely to produce much greater results as regards the welfare of the whole population. I say so perhaps with some ulterior motive, because a number of friends of mine are now organising a medical research institute for myself to work in: they are receiving a very gratifying response from numbers of private persons, from business houses connected with the tropics, and even from British possessions abroad; and we can deal with every penny that we can get. But I am not ashamed of saying this. Let all of us doctors invariably follow the example of Oliver Twist and *ask for more* for this great cause. After all, the donors of the money are likely to receive most of the benefit. I see no reason why the average expectation of life should not be prolonged from 59·1 years to the 70 years of the Psalmist. More than that, I am by no means convinced that it cannot be prolonged still further. Metchnikoff suggested 150 years of life. Unfortunately, he himself did not attain to it. But we

may live in hopes. I have often wondered what would be the length of life of an individual kept entirely free from pathogenic organisms and certain deleterious foods and drinks (some of which have already been debarred in the United States, though not in Scotland). An American wrote to me recently arguing that under such circumstances human life in the world might be eternal. But I am not sure ; perhaps the process of somatic cell-proliferation must end at some time. We do not yet know ; nor do we know why elephants, parrots, and tortoises live for more than a century, while horses and dogs die before twenty. This, too, is a very great subject for research.

It is true, I think, that medical discovery does not require the supreme intellectual ability which is needed for astronomy, physics, and mathematics. Archimedes, Descartes, Newton, Laplace, Clerk-Maxwell, Kelvin, and the modern Relativists possessed or possess, I believe, a more intense intellectual fire than the moderate heat needed for medical, and indeed for any biological studies. The reason for this is that, at least up to the present, biology requires observation rather than calculation—it has more to do with the collection of facts than with exact measurements and induced predictions. But it is a mistake to suppose, as is often done, that anyone and everyone can equally easily achieve biological and medical discovery. True, many important medical discoveries seem to have been made simply by good luck ; but we are apt to forget the earlier years of toil and disappointment which generally preceded, and were only finally crowned by, that good luck. Now, anyone can have good luck or bad luck ; but not everyone is willing to face the previous years of effort. The huge nuggets and the great diamonds of all discovery generally lie deep down in the earth : and those who find them must generally dig deep, far, and long for them. That is just the point. We have to-day numerous scholarships which provide numerous young people with their first opportunities ; but one by one the venturers tend to drop out of the competition. Medical discovery, like all discovery, requires two rather rare qualities—an acute instinct for the right direction, and a burning perseverance in following it up. Now, in my opinion these qualities are possessed only by a few individuals ; and—what is more—it is impossible to know whether any individual really possesses them until he has actually succeeded in his quest. We cannot isolate and detect the future discoverer by any known qualities—by his success in examinations, by the height of his brow, or by the cut of his collars. For these reasons I have always argued that the best way to encourage discoveries in the future is to keep at work, by whatever means you can, the few men

who have actually achieved discoveries in the past; and this is by no means always done, at least in Britain, at present.

Though medicine has advanced very far since the Renaissance, yet it must go still further before the millennium is reached. Many great problems remain to be solved. For example, it is amazing that, though 130 years have elapsed since Jenner proved that the smallpox virus is contained in a minute drop of vaccine, the organism of smallpox has not yet been certainly discovered. Dr. David Thomson, a distinguished alumnus of Edinburgh University, now Director of the Pickett-Thomson Laboratory in London, has recently published in the new annals of that laboratory (St. Paul's Hospital, Endell Street, London, W.C.2) a very careful analysis of the past researches on the viruses of smallpox, measles, and scarlet fever. Causative agents have been often described, nevertheless the matter has scarcely emerged from the conjectural stage; and it is astonishing that a subject of such importance as the causes of zymotic diseases has not yet been completely cleared up. Instead of attacking the subject because of its difficulty, investigators seem to have shunned it for that very reason; and yet the country continues to pay vast sums for the prevention or treatment of these maladies. Just similarly, the country has to pay millions of pounds on account of the foot-and-mouth disease in cattle, chiefly because, I think, it refuses to pay small sums for the proper investigation of the disease. Those who rule us never seem to gather wisdom in this respect. They like to leave all such investigations to be carried out by doctors at their own expense. For example, the solution of the malaria problem cost the Indian Government a sum of no less than £240 in the years 1898–99; and all the recent advances on tropical medicine—advances which were probably greater than those in any other branch of medicine in the same period—cost the world's governments somewhat similar sums. We doctors and other men of science may congratulate ourselves on one point, namely, that almost the whole of medical discovery has been made by ourselves, without help from anyone and at our own expense. Let us boast about it and blow our own trumpets—because no one seems willing to do so for us, and much less to pay us for our work! The cancer problem is being cleared of many of its surrounding difficulties, but it still remains impregnable. Advance is not as quick in many lines where we expected triumphant issues years ago. For example, hook-worm disease can be easily cured in the patient, but by no means so easily in the community. Though Leishman and Donovan found the cause of kala-azar twenty years ago we have failed to ascertain how it is carried; and I could spend an hour in discussing the victories which we have failed to

make, just as I have already spent an hour in discussing some of those which we have made.

It is time now to finish; but before doing so I must reiterate my principal conclusion: that the world will be wise to spend much more money than it is now doing in order to fight diseases of all kinds by investigating how they may be fought. This is the first desideratum. Medical organisation, medical education, medical literature, are secondary; but we as a nation have not even yet quite understood this. Scotland in general, and the Royal Medical Society of Edinburgh in particular, have done "their bit," and perhaps more, in this old war for humanity; and I can only hope that as the eyes of the public become more and more opened to the importance of medical discovery, you will be given larger and larger opportunities to continue the great work.

POPULAR SCIENCE

THE CAUSE OF OUR "ICE AGE"

I. BY ROBERT CLUCAS

Liverpool

ALTHOUGH it is well known that a primitive warm congenial period of vast duration has prevailed upon our Globe, caused by a former moderate warmth of its crust, surface waters, and surrounding atmosphere, counteracting any baneful effect of our axial inclination by delaying the advent of frost and snow, as numerous remains of extinct but former dominant warmth-loving life-forms fully establish, yet numerous fossils of warmth-loving life-forms found intermingled with glacial remains, and also many alien surface boulders still resting as formerly deposited, in precarious positions on previously worn glacial slides (as if mutely striving to preserve from oblivion the history of their past), furnish confirmatory evidence of many successive warm and glacial periods, the cause of which, notwithstanding all previous research, has remained a " Great Enigma."

Why should the cause of our " Ice Age " remain a great enigma? The laws governing gravity and motion have long been discovered, and applied to explain satisfactorily the cause and effect of many previously considered mysterious planetary motions, and this knowledge is still placed freely in our hands, to fulfil its lawful purpose in unmasking the cause and effect of any real or apparent mysterious motion of our globe; therefore, it is to these laws only that we will appeal.

The theoretical unvarying orbital inclination of our axis, demanding several unaccountable long alternating glacial and tropical periods, has too long held sway, and remained practically unchallenged, although frequently questioned. Many eminent scientists have been influenced by these intermingled glacial and tropical remains to consider the probable occurrence of former concurrent arctic winters and tropical summers, although fully realising that this would render the theoretical unvarying orbital inclination of our axis incorrect. Therefore,

whilst a former warm congenial period of vast duration, succeeded by recurring glacial and warm periods of uncertain number and duration are well established, also the strange comparatively rapid extinction of former dominant warmth-loving life-forms, and mysterious ice-conveyance of our huge alien surface boulders from distant places to alien soil fully proved, the mystery of how natural forces have formerly operated still exists. As the laws of gravity and motion during the infancy of our Cosmos have caused, and still maintain, the mechanical motions of our globe governing our present seasons, it is obvious that the operation of these laws has also played a leading part in governing our former seasons. It will therefore be undisputed if it can be shown these laws are creating a continually more vertical orbital inclination of our axis, that our present seasons are becoming less variable, also that our former seasons have been correspondingly more varied.

It is the purpose of this article not only to show that during past ages the laws of gravity and motion have not maintained an unvarying orbital inclination of our axis, but also how, on the contrary, they have directly operated upon it, and are so operating at present, in actually forcing it in a slow swirl retrograde to its orbital motion towards a more vertical inclination to its orbital plane ; exactly as all former records fully confirm, and present observations plainly indicate, in the similar retrograde swirl and gradual approach of the "pole of the heavens" around and towards the "pole of the ecliptic." Therefore, before describing how the laws of gravity and motion cause this peculiar motion of our globe, it will be necessary to examine the premises of a few generally accepted very important theories. We will first, by casting aside all mental bias from preconceived ideas, impartially reconsider the remarkable theory that the slow, angular, swirling motion of our axis, expressed in "the precession of the equinoxes," is due to the same principle governing the peculiar slow, angular swirl of a peg-top commonly observed when it is slowing down in rotation. This principle is said to operate in some obscure manner, through "the gravitational attraction of the sun and moon on the protuberance of matter around the Earth's equator"; it is evident that if our globe were a perfect sphere this motion could not occur.

But how these forces operate is by no means made clear. That the slow axial swirl of our globe is dominated by the same principle governing the slow axial swirl of the peg-top does not bear close inspection, and quite fails in two vital particulars, namely, that whereas both the slow axial swirl and rapid spin of the top are each in the same direction, both the slow axial swirl and rapid spin of the earth are each in

contrary directions. We know why the decreasing rotation of the top is necessarily accompanied by a slow axial swirl in the same direction as its rotation, yet, although our rotation is surely slightly decreasing, there is no slow axial swirl in the same direction as our orbital motion, but exactly the reverse.

As the forces operating around the circumference of the top, if so operating around our equator, could not make an exception with the Earth and produce an entirely opposite effect, the slow retrograde swirl of our axis must be due to other forces; which forces we will presently endeavour to show are quite uninfluenced by "the protuberance of matter around the Earth's equator," but are powerfully operating directly upon our axis throughout its entire length, consequently even if our globe was a perfect sphere, the retrograde swirl of our axis would not cease, but, on the contrary, the accompanying increased axial length would necessarily cause its retrograde swirl to be proportionately accelerated. The theoretical unvarying orbital inclination of our axis is responsible for a further theoretical variation in the "plane of the ecliptic" (caused by mutual planetary attractions) equal to the relative decreasing distance of the "pole of the heavens" to the "pole of the ecliptic." If a model sphere, representing our globe, is made to rotate rapidly in gimbals suitably mounted on the margin of a flat plane—representing the "ecliptic"—the axis will maintain its original inclination to a line projected vertically from that plane to represent the "pole of the ecliptic," quite uninfluenced by any subsequent varying inclination of the plane. So that, even if any variation in the "plane of the ecliptic" is occurring, that it could have any influence against the powerful gyroscopic force of our rotation to alter our relative axial direction from any fixed point in the heavens is contrary to the most elementary laws of motion. Again, if planetary attractions are causing, as suggested by Laplace (who was largely responsible for the theory of the unvarying inclination of our axis), a variation in the "plane of the ecliptic," why has not the "preponderant moment of Jupiter" long ago overcome weaker planetary attractions by forcing us to revolve in an orbital plane inclined similarly to his own?

The inclination of Mercury's orbital plane to the "ecliptic" is given as 7°; Venus, 3° 23′; Mars, 1° 51′; Jupiter, 1° 19′; Saturn, 2° 9′; Uranus, very slight; and Neptune, 1° 47′. Is it not strange, if any real variation of inclination in the plane of the "ecliptic" is occurring, that these relative planetary orbital inclinations have not been observed to change? Does not this prove that any varying inclination of our own orbital plane must also be quite negligible? thereby confirming the only alternative, namely, the actual varying inclination of our

equator, as so plainly indicated for thousands of years both in the sun's decreasing angular range north and south of the equator and equivalent decreasing inclination of the "pole of the heavens" to (any fixed point in the heavens there or thereabouts) the "pole of the ecliptic."

It is, of course, well known that our fellow-planets slightly vary the ellipticity of their orbits during each opposition, but it must be remembered this is entirely due to the fact of their common revolution around the same centre of gravity in nearly similar orbital planes; thereby causing their mutual gravitational attractions during oppositions to easily either assist or counteract the central attraction of the sun, without affecting the powerful centrifugal motion of each planet which continually maintains them in their original orbital planes. So that whilst Jupiter, Mars, the Earth, Venus, and other planets during opposition slightly vary the ellipticity of their orbits, they still maintain their relative orbital inclinations.

The decreasing inclination of the "pole of the heavens" to the "pole of the ecliptic" is proved, and admitted to have been continual from the earliest records, which extend well over the past 3,000 years, whilst the rate of this decreasing inclination is known to have varied from a former 48 seconds per century to its present rate of 46½ seconds per century, and it is confirmed that this rate is still decreasing. Laplace has stated "the greatest extent to which the 'plane of the ecliptic' could vary is 1° 40′, on either side of the 'invariable plane.'" Flammarion states "the variation of this movement cannot exceed 2° 37′." Herschel states "the 'invariable plane' is constant at 23° 28′." It is, therefore, clear that since Laplace, through sharing the general deep-rooted belief in the unvarying orbital inclination of our axis, was forced to accept the only alternative (when asked to explain the sun's decreasing angular range north and south of the equator), namely, a decreasing inclination in the "plane of the ecliptic," that a theoretical "diminution in the obliquity of the ecliptic" has, with all its imperfections, even up to the present held sway. But even should it be contended that a variation in the "plane of the ecliptic" may be occurring, this could not, as previously explained, cause any variation in the direction of our axis, much less make that direction vary equal to its own.

We have now endeavoured to show by the evidence these theories themselves contain, quite apart from and before explaining the cause of our axial motion, that the gradual decreasing inclination of our equator to the ecliptic and correspondingly decreasing axial inclination to the "pole of the ecliptic" can only be explained by a definite motion of our globe, which accounts perfectly not only for the sun's angular

decreasing range north and south of the equator, but also for the gradual approach of the "pole of the heavens" towards the pole of the ecliptic. This indicates clearly a former increased axial inclination, which, in proportion to a former decreasing warmth of our crust, has gradually caused colder winters, responsible for the extinction of many former dominant warmth-loving life-forms, and has finally created severe winters with their vast accumulations of snow and ice, causing, on the approach of correspondingly hot summers, not only severe glaciation, but stupendous spring floods bearing their boulder cargoes over temporarily submerged land to alien soil in the melting equatorwards flowing ice-packs of the past. It now remains to explain how the laws of gravity and motion have operated, and are at present operating, in causing this to all living creatures vital motion of our axis.

Newton has shown that the laws of gravitation act and react upon planetary bodies from their centres of gravity. The laws of motion impart to all travelling bodies a momentum of one pound per foot per second, which momentary force is, of course, only fully realised when the motion is completely resisted. Now it must not be forgotten, but unfailingly remembered, that when a motion is being partially resisted a corresponding momentum is thereby imparted, also that when a motion is being accelerated an equivalent resistance is encountered. If our Globe, whilst revolving annually round the central attraction (the sun) had its axis perpendicular to its orbital plane (the "ecliptic"), the axis would have nothing to interfere with the gyroscopic power maintaining its vertical position even if that power could be greatly decreased by a slow rotation, because our axis throughout its entire length would, of course, be revolving at the same distance and velocity as its centre of gravity round the sun. But if at our present axial inclination our daily rotation were very slow, the gyroscopic power of our rotation, which alone maintains our axial inclination, would be proportionately weaker, and the physical effort of our axis, throughout its entire length, to revolve at a constant velocity relative to its centre of gravity around the sun, would rapidly force our axis to a vertical inclination to its orbital plane, and it is the natural effort of our axis to attain this vertical equilibrium that is expressed in the slow-retrograde swirl and very much slower approach of the "pole of the heavens" around and towards the "pole of the ecliptic." The powerful gyroscopic effect imparted by our rotation is thus combated by the superior forces of momentum and resistance actually born from and in proportion to our axial inclination. If the gyroscopic influence of our rotation was entirely successful, any retrograde swirl or varied orbital inclination of our

axis could not occur, but by the Poles, both north and south, being forced to travel in proportion to our axial inclination at continually varying distances relative to the axial centre either towards or from the sun, forces of momentum and resistance equal to their continually varying relative orbital velocities are respectively exerted in opposing directions to each other tangent to our orbital course at either end of our axis.

A fair estimate of these powerful natural forces continually exerted upon our axis may be obtained in the following manner. Either extremity of our axis, north or south, in travelling from midsummer to midwinter is forced to describe an increasing sweep relative to its axial centre (from the sun), so that, on reaching midwinter, it has been forced outwards from its previous inner relative midsummer position to the axial centre more than 3,000 miles, which gives a polar velocity relative to the axial centre (from the sun) of over one foot per second, which meets with a resistance of over one "foot-pound"; the opposite axial extremity meanwhile travelling from midwinter to midsummer, necessarily advances (towards the sun) relative to the axial centre more than 3,000 miles, which gives a polar velocity relative to the axial centre (towards the sun) of over one foot per second, thereby creating a momentum of over one "foot-pound." The powerful gyroscopic influence imparted by our rotation is thus combated by the superior forces of momentum and resistance which are, of course, exerted at their maximum power at each extreme end of our axis, tangent to its orbital course and in opposite directions to each other during the entire year. As either Pole departs from midsummer and our axis becomes more tangent to its orbital course, so the force of resistance is exerted more against its inclination, gradually reaching a maximum strength directly against its inclination when tangent to its orbital course in midautumn, and then gradually decreasing as it becomes less tangent to its orbital course towards midwinter. The opposite Pole, meanwhile advancing from midwinter, causes the force of momentum, which is exerted in the same direction as our orbital motion, to reach gradually its maximum strength directly against our axial inclination when the axis is tangent to its orbital course in midspring, and then to decrease gradually as it becomes less tangent to its orbital course towards midsummer.

It is evident that, by reason of our axis being tangent to its orbital course in midspring and midautumn, these powerful natural forces of resistance and momentum are then exerted at their maximum strength in forcing it towards a vertical inclination to its orbital plane. But these forces of resistance

and momentum also cause the swirling retrograde motion of our axis. As either Pole, north or south, departing from midautumn to midwinter is subject to resistance exerted tangent to, and against its orbital motion, so the opposite Pole meanwhile advancing from midspring to midsummer is subject to momentum exerted tangent to, and in the same direction as, its orbital motion.

During this journey, our axis, which gradually becomes more diametrical to its orbital course, is therefore subject to these gradually increasing powerful side-thrusts, exerted in opposite directions to each other and tangent to its orbital course (attaining their maximum power in midwinter and midsummer), thereby causing our axis to swirl in a retrograde direction to its orbital motion. But, as either Pole passes midwinter and advances to midspring, the previous resistance against its orbital motion is suddenly replaced by a powerful momentum exerted in the same direction as its orbital motion, whilst the opposite Pole meanwhile passing midsummer and departing to midautumn, the previous momentum in the same direction as its orbital motion is suddenly replaced by a powerful resistance exerted against its orbital motion. During this journey the extremities of our axis are thus subject to these reactionary side-thrusts (exerted in opposite direction to each other tangent to its orbital course) gradually decreasing in power as our axis becomes less diametrical to its orbital course towards midspring and midautumn. As our axis during this portion of its journey is subject to these reactionary forces, why do they not cause a reactionary right swirl from midwinter to midspring and midsummer to midautumn, equal to its retrograde swirl from autumn to winter and spring to summer, and so prevent any accumulated swirl either in a right or retrograde direction?

That the retrograde forces have the mastery is plainly revealed in the "precession of the equinoxes," and the reason why these forces predominate is clear, if we again, but more in detail, consider the conditions to which our axis is subject during one revolution of its orbit. As our axis is tangent to its orbital course in midautumn and midspring, the forces of momentum and resistance do not at these periods exert any side-thrust on our axis one way or the other, but are then obviously, as previously stated, at their maximum strength in forcing it towards an upright position. But as either Pole respectively journeys from midspring to midsummer and midautumn to midwinter (by reason of our axis becoming more diametrical to its orbital course) proportionately powerful side-thrusts causing an increasing retrograde swirl comes gradually into play, attaining their maximum power in mid-

summer and midwinter, and although, after our axis has passed this diametrical position, the reactionary forces from midwinter to midspring and midsummer to midautumn are immediately brought into play, yet the momentum of the retrograde swirl (attaining its maximum power in midwinter and midsummer) requires time to be arrested before any reactionary right swirl can commence, and it is in consequence of these periods of arrest, after passing every midwinter and midsummer, that the supremacy of the forces causing the approximate slow retrograde swirling motion of our axis is obtained. The gyroscopic power imparted by the radial velocity at our equator is, of course, enormous, yet it must be remembered as we approach either Pole this power gradually decreases equal to the decreasing radial velocity, until at last a central mass surrounding our axis, forming as it were a huge axial core, has a slow radial velocity imparting very little or no gyroscopic power whatever, and it is by reason of this "huge axial core" throughout its entire length, being forced to travel in proportion to its orbital inclination at varying orbital velocities relative to its centre of gravity, that correspondingly powerful forces of momentum and resistance have respectively from the infancy of our globe been exerted in opposing directions tangent to our orbital course at opposite ends of our axis, causing a slow swirl-retrograde to its orbital motion and gradual more vertical inclination to its orbital plane, exactly as plainly indicated not only in the long-observed gradually decreasing inclinations of the "pole of the heavens" to the "pole of the ecliptic," and the equivalent decreasing angular range of the sun north and south of the equator, but also in the various glacial and tropical remains (found so plentifully in our present temperate and arctic zones), many of which by their remarkable preservation are at present true natural witnesses, actually revealing the history of their environment when formidable climatic and terrestrial forces were deciding their destiny during the highly variable seasons composing our "Ice Age" of the past.

We have now shown as briefly as possible that, just as an excessive axial inclination was during and subsequent to the departing warmth of our crust the natural cause of our "Ice Age," therefore vast alternating glacial and tropical periods have not occurred, but that our present temperate and arctic zones, whilst still enjoying tropical summers, have also endured a long period of increasingly colder winters, finally developing into a vast period of concurrent arctic winters and tropical summers, which with their accompanying severe annual glacial conditions have subsequently, in proportion to our decreasing axial inclination, gradually decreased in range. Also as the

laws of gravity and motion are still forcing our axis, and must continue to force it at an ever-increasingly slower rate, towards an upright position to its orbital plane, so the present range of our "Ice Belt" is continuing to decrease and, therefore, proportionately less variable seasons must in future prevail upon our globe.

II. By W. M. H. Greaves, M.A.

Royal Observatory, Greenwich

Mr. Clucas attempts to attribute the "Ice Age" to a larger obliquity of the ecliptic than exists at present. He considers that the obliquity of the ecliptic is steadily diminishing, and that such diminution is progressive—that is to say, it is always in the same direction. There is, of course, a well-known change in the obliquity owing to the planetary precession, but it is perfectly well established that the "secular" change arising from this cause is made up of a number of periodic terms of long period, and, as a matter of fact, it has been shown that the total range in obliquity thus produced amounts to some two or three degrees. Mr. Clucas, however, rejects the accepted theory of planetary perturbation in dealing with the observed variation of obliquity. He is evidently ignorant of the mathematical theory of perturbations, and his argument, such as it is, is almost impossible to follow. He, however, mentions, among other things, the obliquities of the orbits of the major planets and asks: "Is it not strange, if any real variation of inclination in the plane of the ecliptic is occurring, that these relative planetary orbital inclinations have not been observed to change? Does not this prove that any varying inclination of our own orbital plane must also be quite negligible?" I am afraid that Mr. Clucas's statements are in error. As a matter of fact, it is well known that the obliquities of the orbits of the planets do change, and the observed rate of change is perfectly accordant with the values deduced from the theory of their perturbations.

Mr. Clucas attempts to give in general terms a mechanical explanation of his alleged steady diminution of the obliquity of the ecliptic. He confines his attention to the earth's axis and ignores the equatorial protuberance. He rejects the accepted theory of precession, remarking: "That the slow axial swirl of our globe is dominated by the same principle governing the slow axial swirl of the peg-top does not bear inspection, and quite fails in two vital particulars, namely, that whereas both the slow axial swirl and rapid spin of the top are each in the same direction, both the slow axial swirl

and rapid spin of the earth are each in contrary directions, etc." Mr. Clucas apparently does not realise that in the case of a top the external forces exert a couple tending to drive the axis away from its vertical position, whereas in the case of the earth the external couple (which is due to the equatorial protuberance) tends to draw the axis towards the pole of the ecliptic and, consequently, there is retrograde precession. Confining his attention to the axis, Mr. Clucas enters into an incomprehensible argument by means of which he thinks he has proved his case. I take it that Mr. Clucas for his purposes regards the earth as a material rod coincident with its axis. Such a procedure is absurd, in any case; but, even if it were so, elementary dynamics show that there would be direct precession and not retrograde precession, as is actually the case. Evidently Mr. Clucas has gone astray again somewhere in his argument. The actual retrograde precession is of course due, as has been said, to the equatorial protuberance. Accepted dynamical theory also shows that in no case can gravitational forces alone produce an effect such as that alleged by Mr. Clucas, who, as has been stated, thinks that the obliquity of the ecliptic is diminishing, and will always continue to diminish.

NOTES

Sir Malcolm Watson (Sir R. Ross)

In our previous issue we noted the recent honours given to Sir Malcolm Watson for his great anti-malaria work in the Federated Malay States and elsewhere. On October 2, the Ross Institute for Research in Tropical Medicine (now being formed) gave him a congratulatory luncheon at the Hotel Belgravia, at which the following gentlemen were present: Lord Henry Cavendish-Bentinck, M.P. (Ross Institute), Sir West Ridgeway, General Sir William Leishman (D.G. Army Medical Department), General Sir David Bruce, Surgeon-Rear-Admiral Sir Percy Bassett-Smith (President Royal Society of Tropical Medicine), Sir William Simpson (Ross Institute), Sir James Cantlie, Sir Charles McLeod (Ross Institute), Eric Macfadyen, Esq., M.P., Dr. Andrew Balfour (Director, London School of Hygiene and Tropical Medicine), Dr. Morley Wenyon (Director, Wellcome Bureau), Dr. Aldo Castellani (Ross Institute), Dr. David Thomson (Director, Pickett-Thomson Laboratory), Dr. John Gordon Thomson (London School of Tropical Medicine), and ten other gentlemen connected with tropical studies. Subsequently, Sir Malcolm has delivered a series of lectures on malaria at the Glasgow University; and finally, on October 23, the eve of his return to the Federated Malay States, he read a paper before the Royal Society of Tropical Medicine, entitled, "Observations on Malaria Control with Special Reference to the Assam Tea Gardens, and some Remarks on Mian Mir, Lahore Cantonment."

This was really a very important meeting, which amounted to one of the great "decisive battles of the world"—though perhaps few of the crowded audience quite realised the fact! Peace has its victories as well as war, and generally much more important ones. For more than a quarter of a century anti-malaria work by mosquito-reduction has been held up throughout India and largely throughout the world by a wholly gratuitous assumption, which was advanced long ago by a few good but mistaken observers, that mosquitoes could not be reduced. The importance of the question may be gauged from the fact that malaria causes a mortality which I estimate to be 2,000,000 annually in the world for a *minimum*—that is, a greater annual mortality than that caused by the war while it lasted. But the world finds it very difficult to understand these little matters, and this country in particular has been about as stupid over the business as can be conceived. The French at Ismailia, the Americans at Havana, Dr. Balfour at Khartoum, Dr. Clarke at Hong-Kong, gave instances of successful mosquito-reduction on large or on small scales; but our authorities remained quite above such proofs. The facts are that governments do not like to spend money on sanitation, and that we as a nation are much more given to politics, sport, and novels than to any really useful work that does not put money into the pockets of the workers. At the meeting referred to Sir Malcolm finally demolished this fantastic hypothesis that the mosquito is too powerful for the human race. Of course, in these days when agriculture undertakes great campaigns against insect pests of crops and trees, farmers rid their cattle of vermin, and civilised men are themselves ceasing to be verminous, campaigns against such pests as mosquitoes are not at all out of the usual

course. But it has been difficult to drive this into the heads of our rulers, whether these be governors or municipalities. Years ago an alleged attempt was made at Mian Mir in India, with the support of the Government, to investigate the possibility of reducing mosquitoes. Sir Malcolm has recently visited the place, and he made some remarks on the subject. On the whole this address of his has been the Waterloo of the anti-antimosquito-ists. We can hardly venture to hope that they will never be heard of again, but do not think that we need trouble ourselves any longer about their opinions. Surely it is now time that all malarious tropical countries should undertake antimosquito work upon a proper scale. The cost of malaria is always far greater than the cost of malaria prevention, at least in towns; and, after a quarter of a century, we should really begin to understand the vast practical bearing of the subject.

August as a Holiday Month (The Rev. E. F. Robson, F.R.Met.Soc.)

From the Weather Point of View

Why is it that August is the general holiday month? Why is it that the seaside resorts everywhere are crowded out in August, so that prices rule very high and those who have only very modest banking accounts are compelled either to curtail their holiday or to take it at another time of the year? The reason obviously is this: that the schools are closed, children are no longer at work, and therefore August is the best month in the year for members of a family to be free to enjoy together the pleasures of a holiday either at the sea or in the country. This explains, without a doubt, the popularity of August as a holiday month—but, from a weather point of view, can it be regarded as a success? Now this article is written after a careful study of each August since this century began, and also by one who, like many another, has invariably taken his holidays in the so-called holiday month, and whose experience has in consequence taught him what others might have been tempted to doubt, namely, that August is the most disappointing month from a holiday-maker's point of view, and the most invariably unsettled month, not only of the summer months, but of the whole twelve. In fact, in view of the Augusts of this present century, we can go as far as to say it has only once proved to be otherwise as far as the northern half of the United Kingdom is concerned; in fact, this statement will be found to be endorsed by those who have kept a daily record of the weather this century as far south as the Midlands of England. In Scotland—North Wales—Northern England—the seaside resorts of Lancashire and Cheshire—in Shropshire and Warwickshire and adjoining counties, August has maintained with remarkable consistency a character for wet, unsettled weather which has not been attained by any other of the twelve months. In many districts and at many meteorological stations it is actually the wettest month in the amount of rainfall for the whole twelve months, and in all parts of the country, including the southern section of England, it is the wettest of all the summer months—a few isolated stations alone having a slightly heavier rainfall in July. Since 1900 the only really fine August was that of 1911, and even in that year the opening week in the greater part of England produced remarkably heavy rainfalls; but the general character of that month, as of the whole summer, was fine, warm, and very pleasant. In 1921, of more recent memory, when great heat and drought were generally experienced, August, true to its character, was the most unsatisfactory and disappointing of all the summer months from the holiday-maker's point of view; in fact, in many parts of the northern half of England, including the Central and Western Midlands, the rainfall was very excessive, and the general character of the weather unreliable, stormy, and at times very wet; and even in the extreme south of England it was the least fine and least sunny month since March of

that year, and less fine than either September or October. In 1914, the memorable August of the opening of the Great War, there were some spells of fine weather, but in looking at the past records it could hardly be considered as a month of settled fine summer weather. Indeed, as has been suggested throughout this short article, it is rather the unvariable character of this month for unsettled weather, making it perhaps the one month in the year which one can rely upon as being unsettled and changeable, which is probably the most notable feature of it. One cannot find the similar consistent regularity for unsettled weather in any other of the twelve calendar months which one finds in August. Only one really fine August and one moderately fine one out of twenty-five years is not a bad record for reliability where the weather of the British Isles is concerned. October, which takes second place in some districts, and even first in others, in the matter of heaviest total rainfall is more variable than August, and as a matter of fact during the last five years three of the Octobers have been remarkably fine and dry, and even this year we have experienced a greater number of fine and pleasant days than we did in August. The same mark of variableness can be applied to all the other months of the year, August alone excepted. Of course, we are all acquainted with the St. Swithun story; but, putting aside all superstition and bias on the one hand or the other, it does seem to point to this, that some great wave of atmospheric disturbance does take place somewhere between July 10–20, and that it does actually rule the weather for about six weeks. Even in the memorable year of 1921, when it seemed as if the great drought might remain unbroken, it was during this period that the weather began to assume a generally unsettled nature, particularly over the northern half of the British Isles, and even farther south it was felt to a certain degree, and from which it did not recover until the close of the month, to be followed by the brilliant September, when anticyclonic conditions reasserted themselves. Without labouring the point much further, one has only to remind the reader of the Lammas Floods of Scotland which are so recognised a feature of that country. Lammas Day—meaning Loaf Mass Day—the day of the Thanksgiving for the early wheat harvest, was originally observed on August 1. The Prayer Book Calendar of the English Church still calls August 1 Lammas Day—though in the Church of Scotland it is now kept on August 12, probably the latter date being in accordance with the earlier calendar. This, of course, strengthens the argument that August has for many years been regarded in Scotland as a period of much rain. In conclusion, one might add that, until the time comes when the date of the general school summer holidays is altered, which is much to be wished, it is idle to depend upon a spell of fine, warm, dry weather in August—generally the most disappointing and unsettled of all the months of the summer. It is only a matter of mere chance, such as has only occurred once in the past twenty-five years, that the holiday-maker enjoys a spell of really fine, warm, settled conditions in this month. "This is very bad weather for August," said many a man to me in the depressing August which prevailed this year. "No," I replied, "it is August being true to its character!"

Cancer

Perhaps the best abstract of our present knowledge of cancer which we have recently read is one by Mr. H. W. S. Wright, M.S., F.R.C.S., contained in the *Cheeloo Papers* (1924), which are a "Selection of Addresses delivered at Shantung Christian University, Tsinan, China." It maintains the seeming paradox that cancer is easily curable by surgery—if taken in time. There are four stages: (1) The pre-cancerous stage, in which there is generally (? always) some chronic local irritation; (2) the early cancer stage, before the neighbouring lymphatic glands become involved, during which stage,

the author thinks, 90 per cent. of the cases can be cured by surgery; (3) the stage in which the neighbouring lymph glands are involved—in which about 30 per cent. of the cases are still curable by surgery; and (4) the last and inoperable stage—in which 40 to 50 per cent. of all cases are found to be when they first seek medical advice. The author rightly urges that every-one over forty years of age should be regularly examined twice a year for pre-cancerous or early cancerous conditions, just as they now go to the dentist for their teeth. X-rays and radium are valuable adjuvants at certain stages. Few people in this country quite appreciate the fact that more than one in every ten of them is destined to die of this dreadful disease. Yet the British people contribute only about one penny *per* head *per annum* for medical research in all subjects, and only a fraction of this is devoted to cancer research.

In favour of the medical as against the surgical treatment of cancer, Dr. John Harger has recently published a small book on *Cancer: Its Causation, Prevention and Cure* (C. Timling, Liverpool, 1924), in which he supports the views of Drs. Robert Bell and L. Duncan Bulkley regarding treatment by diet and intestinal antisepsis.

The Indian Medical Gazette for August 1924 has consulted several experienced practitioners in India regarding the frequency of cancer among the natives. Lieutenant-Colonel H. Halliday, M.B., I.M.S., says: "My experiences lead me to conclude that intestinal stasis is practically non-existent in the unsophisticated Indian; that such surgical diseases as appendicitis, cholecystitis, gall-stones with their complications, gastric and duodenal ulcer are all but unknown; and that finally amongst the class that I have described intestinal cancer is of even greater rarity." But he found appendicitis among Indians who have taken to European diets and customs; and Major W. L. Harnett, M.B., F.R.C.S., I.M.S., states that cancers of the uterus and cervix are quite as common among Indian women as amongst European women, though cancer of the breast may not be so common. Captain Mukerji, F.R.C.S., I.M.S., however, does not quite agree. The question as to whether cancer occurs as frequently among vegetarian races, such as the Indians, Chinese, and Japanese, as it does among races who eat more meat should be examined much more frequently and rigorously than it has been up to the present.[1]

The high experts on Cancer have not been at all kind to Mr. J. Ellis Barker's book called *Cancer: How it is Caused and how it can be Prevented* (John Murray, 1924); but we are very glad to welcome dissertations on medical subjects by men who are not medical men; at least they bring fresh air into discussions. Mr. Barker's book shows the best of all qualities for scientific work, namely enthusiasm. Unfortunately enthusiasts must stand the racket; they are always opposed; they are sometimes annihilated; but they generally live through it, and occasionally win the ultimate prize. Mr. Barker's hypothesis regarding cancer was that it is due principally to toxins generated in the intestinal tract. Many years ago Mr. H. C. Ross, of the Mcfadden Researches, not only enunciated the same hypothesis, but also showed that certain chemical substances which are commonly produced by septic processes in the intestinal tract have the singular power of forcing leucocytes in a specimen of blood to divide under one's eyes. This suggested that cancer is caused by a process of overhealing. According to him, cancer starts in the healing of some local injury or lesion; then, owing to the presence of too many of these chemical substances (which he called "auxetics"), the process of healing is overdone, and a tumour is gradually built up. The bigger the tumour becomes the more auxetics are produced, and the more the cells are forced to divide. This theory also led to considerable opposition,

[1] See also the October number of SCIENCE PROGRESS, p. 325.

but apparently to no actual disproof. Mr. Barker's hypothesis seems to be a part of it. Unfortunately, hypotheses are only the beginnings of things, and science always looks for the ends of things, namely proofs, before she is satisfied.

A Gorilla Sanctuary

In *The Times* of September 5, 1924, Mr. J. G. Whiteley states that the Belgian Government is establishing a large tract of territory comprising an area of 250 square miles of high and healthy territory in the Kivu district of the Congo State, which is especially the gorilla country, for a sanctuary for all kinds of wild animals, including gorillas, and of plants. Provision has been made for a sufficient number of wardens to prevent the intrusion of hunters and the destruction of plants or trees. The area embraces the three volcanoes of Mounts Mikeno, Karissimbi, and Vissoke. Apparently scientists are to be allowed into the area, but they must be disarmed; and one wonders what they will do when they meet the first buck gorilla. The area is to be called Parc National Albert in honour of His Majesty King Albert, who has taken an active and personal interest in the project.

In *The Times* for October 30 T. Alexander Barns writes that: "As the result of our researches, we have quite established the fact that what we may call the Central African gorilla can no longer be considered as a rare animal. They are exceedingly numerous over a huge extent of country that takes in not only the mountain forests north-west of Tanganyika and west of the Rusisi Valley, but the greater part of the lowa district west of Kivu as well, and probably extends right away to the east bank of the Upper Congo River, where a gorilla was shot as long ago as 1883. Then, again, they are to be found on the high mountains close to the west shore of Lake Kivu, besides those on the Birunga Volcanoes to the north-east of the lake. I am quite in agreement with the Belgian Government in making a sanctuary for these animals in the latter area, but entirely to forbid shooting them throughout the Eastern Congo is quite an unnecessary restriction. I will even go so far as to suggest that one or two gorillas be allowed to be shot on the 2,000 fr. (now advanced to 3,000 fr.) licence already in force, which would induce many sportsmen to visit the country."

In *The Times* of two days later, however, Dr. Drake Brockman objects altogether to the shooting of gorillas for sport, and most sportsmen of to-day will probably support him in this opinion. See page 539.

Vaccination (Sir R. Ross)

There was an interesting discussion of smallpox, with special reference to diagnosis, at the last meeting of the British Medical Association (*British Medical Journal*, August 23, 1924). Dr. W. McConnel Wanklyn of the London County Council opened it with a paper which dealt not only with the diagnosis of smallpox, but also with the prevalence of the disease and public vaccination. He gave a table of deaths and cases in England and Wales, and also in London from 1841–1924. Smallpox cases in London during the nineteen years before 1904 totalled 15,804, while those during the nineteen years subsequently totalled only 351, which improvement has occurred in spite of the fact that vaccination has considerably diminished during the same period. He attributes the improvement to what he calls team work in connection with smallpox—that is, the early detection of cases by medical officers of health and their speedy removal to hospitals. This may be the case, but years ago I gave another possible explanation based upon my mathematical consideration of epidemics (*Prevention of Malaria*, 2nd edition, Murray), namely, that epidemic diseases are apt to remain very much at a constant level, depending upon the number of susceptible persons, until

this number increases greatly within a given area. If the increase is sufficient the epidemic flares up to large numbers. I called this phenomenon the Flare Phenomenon in epidemics. Though I wrote a good deal on the matter (see also a paper in the *Proceedings of the Royal Society A*, vol. xcii, 1916; and vol. xciii, 1917), no notice whatever has been taken in this country of my equations, although some of them have been discussed and elaborated in America (but not in connection with smallpox).

In the discussion referred to, however, there was an admirable contribution by Dr. R. P. Garrow, who showed pretty clearly that nearly all the recent smallpox in Derbyshire was " Alastrim." We now appear to recognise three varieties of this disease, namely (1) true virulent smallpox, (2) alastrim, (3) chickenpox. The first is generally known as Asiatic or European smallpox, the second as American smallpox, and the third is, of course, familiar everywhere and is also a very mild similar disease. I have always thought that these three forms are possibly due to three species of some organism. For example, the three species of malaria vary similarly as regards malignancy—most of the fatal cases being due to malignant tertian, while the mild tertian and the quartan are not so dangerous—yet these three species of malaria are caused by parasites which are closely allied. Though more than a century has passed since Jenner proved that the causative organism of smallpox must be contained in a minute drop of vaccine, no one apparently has as yet with certainty isolated it under the microscope. Yet smallpox and vaccination are estimated by Dr. Wanklyn to cost the country three million pounds a year. Our rulers go on spending this money, but have not got the intelligence to make any attempt to get rid of the expenditure by investigating the disease. Dr. Wanklyn thinks it might be possible to drive smallpox out of this country altogether, and I quite agree with him; but it will not be done until the people and their rulers begin to show some intelligence in connection with the matter. Anyway, our public health authorities are greatly to be congratulated upon the improvement which they have been constantly making, in spite of the absurd opposition of the more foolish elements of the population.

Science in Soviet Russia

Many articles have appeared in the press regarding the present state of Soviet Russia; but one by Dr. W. Horsley Gantt, B.Sc., M.D. (Virginia), formerly Chief of the Medical Division of the Leningrad Unit of the American Relief Administration, on the hospitals and health conditions in Russia to-day (*British Medical Journal*, August 23, 1924) draws a terrible picture. The question which is the best form of government for the human race to adopt is really a strictly scientific question, requiring scientific modes of thought by men who have had experience in scientific methods. The popular idea to-day that anybody from a manual worker to a fox-hunting nobleman is capable of forming just opinions on this subject is as ridiculous as the supposition that such men would be able without special study to form just opinions on modern astronomy, physics, or medicine. As long as men imagine that they can be properly ruled by amateurs in the science and art of government, so long will they be badly ruled—they may as well have their engineering or medical treatment done by similar people. It has been proved over and over again in scientific works on political economy that the creed commonly called Socialism is as impossible as the creed that two sides of a triangle are together less than the third side. Obviously if no one is personally interested in maximum production, no maximum production will exist, and the people will not have their work done properly in any line. Thus, where no one is interested in producing food, little food will be produced. Where all men are equal there will be no stimulus to work, and the results will be that nothing will be done and that the whole

nation which adopts such theories will sink into starvation. Dr. Gantt shows that this result is being achieved in Russia—just as the economists knew it would be. Men, like other things, have to submit to the laws of nature, and nature has ordained that men must live by the sweat of their brows.

In a later article, however (*British Medical Journal*, September 20, 1924), Dr. Gantt commends the Soviet Government for its attitude towards science, and remarks: "Although laboratories and schools are much poorer than formerly, and although scientists as well as others have undergone severe privations, science in Soviet Russia has kept up to its former high standards and, paradoxically, has even prospered." The reason for this is that science and art are really created almost entirely by exceptional men who, like the germ-cells of the body, work largely outside the economical system of the masses.

Joseph Aspdin

There are many men who have conferred benefits upon the world but whose names are unknown to the public and cannot always be found even in biographical dictionaries. Joseph Aspdin was a mason of Leeds who in 1824 invented "Portland cement," the name he gave to a mixture of finely powdered lime and of clay in certain proportions. This is now employed almost everywhere in constructing houses, in lining drains and sewers, and in facing work of many kinds, either by itself or in the form of "concrete." On Saturday, September 8, 1924, the public of Leeds honoured his memory. Mr. P. W. Kelly, President of the American Portland Cement Association, presented a bronze tablet to his memory, in the presence of a large company. Aspdin was not a chemist, but he was a stone-mason in a very large sense of the word. Mr. P. M. Stewart, Chairman of the British Cement Makers' Federation, added that we were probably on the eve of a *concrete* age, and that the age had already come in the United States and was a *concrete* fact. Sir Edwin Airey claimed that Aspdin's invention had a more revolutionary effect upon civilisation than any other invention; though we can scarcely admit this. The Lord Mayor of Leeds hoped that a chair for the cement industry might be established at Leeds University.

Transplantation of Eyes

A number of surprising experiments have recently been emanating from Vienna, and biologists seem to be considerably exercised regarding them. There has been much discussion about Dr. Kammerer's work on the inheritance of acquired characters, and we have published considerable correspondence from Prof. E. W. MacBride and others on the subject. Then Walter Finkler recently claimed to have successfully exchanged the heads of various insects; and Dr. Koppanyi, who has carried out investigations in the Biological Research Institute at Vienna, thinks that he can transplant eyes. Recently, however (we have obtained our information from *The Times* of September 29, 1924), Dr. Blatt, writing in von Graefe's *Archiv für Ophthalmologie*, finds, on investigating the whole subject, that an eye may be transplanted, but that it remains sightless in its new position. He has made eye transplantation experiments on 400 fresh-water fish, 40 chickens, and 80 rabbits, always using anæsthetics. He obtained anatomical healing in 26 cases of fish and in two rabbits, but all the fowls failed. *The Times* remarks: "He took special precautions to secure that the sockets of the respective animals in each operation were similar in size, and that the eye muscles and optic nerves were severed so as to bring the cut ends as closely as possible into apposition. The wounds healed, and it was clear the transplanting eye was receiving nutrition. Short, irregular movements could be observed,

showing that the muscles had made new connexions; but these were totally unlike the purposive rotation of normal eyes. The eyes, moreover, were totally blind, as was shown by direct and indirect tests. They made no response even to sudden flashes of light, and they did not guide the movements of the animal. Fishes are able to change the colour of their skin, the colour changes being controlled through the sense of sight. Those with transplanted eyes became darker, precisely as happens with blinded fish. It may be taken as proved, therefore, that within limits it is possible to transplant eyeballs, but that there is no evidence for recovery of sight. Dr. Koppanyi has added nothing to what was already known. It is to be hoped that such futile, and, even with the use of anæsthetics, somewhat cruel experiments may now cease."

Dental Caries

The effect of diet on the development of this disease has been well studied by three investigators in the *British Medical Journal* for August 30, 1924, page 354. They took three groups of children numbering nine, ten, and thirteen respectively, and gave each group a different diet, upon which they were kept for seven to eight months. The average age of the patients at the beginning of the experiments was a little over seven years. It was found that caries developed considerably more frequently in those who were kept upon one of these diets, namely diet B, which contained much carbohydrates, including oatmeal. The best diet contained no oatmeal, but the intermediate diet, C, contained some occasionally. This work verifies with some statistical exactness the well-known hypothesis that caries in teeth is due to carbohydrates. The paper should be studied for details, and we hope that the research will be continued. Is there any chemical constituent of oatmeal which dissolves dentine more easily than bread does ? In the same *Journal* for September 13, page 485, Dr. A. H. Habgood makes a vigorous and witty plea for more fruit in diets.

The Extermination of Rats and Rabbits by the " Rodier Method "

We have received some vigorous and amusing pamphlets on this subject, from W. Rodier, 327 Collins Street, Melbourne, Australia. The method consists in killing out the female rats and rabbits, leaving the males alive in excess to destroy each other and to prevent fertilisation of the remaining females by over-mating. The author traverses criticisms made in *Nature* of November 4, 1922, complains of its refusal to publish his replies, and criticises remarks made by Mr. Alfred Moore and others. Those who attempt to persuade the world to wage war against its vermin have often been obliged to protest against the world's indifference ; and Mr. Rodier is evidently one of them. We think that there is much likelihood in his hypothesis, and that his method should be fully tried under experimental conditions. The cost of a single battleship or " dirigible " would probably more than suffice to settle a matter which is really of first-rate importance both for agriculture and for public health.

An Ido-English Dictionary

We have just received a copy of this (Pitman & Sons, price 10*s*., 1924). It is very nicely arranged and printed and we should use it regularly if we were not too lazy to learn the language. That, we fear, is the predicament of most people. We suffer from occasional correspondence with foreigners, but always find the easiest way out of the difficulty to consist in replying in our own unfortunate language and leaving the interpretation thereof to our correspondent. We might take the trouble to learn Ido if we were quite

certain that a sufficient proportion of the world's population were to do the same. When we travel abroad we meet many ignorant persons, such as ticket-collectors, porters, guards, hotel-keepers, and voluble persons who wish to deprive us of our seat in the railway carriage; but unfortunately, up to the present, few of them have been more energetic in studying Ido than we ourselves have been. We once heard a witty Frenchman remark that Esperanto was an excellent language, because all one had to do was to use it for preliminary salutations and then to continue the conversation in French. Perhaps some day the world will become more rational and also more hard-working, especially after the League of Nations succeeds in dominating us all—even the Turks. By that time most of the writers and readers of SCIENCE PROGRESS will probably be dead. However, we wish the cause every success, and sing the praises of those men of superabundant energy who write these books.

Public Gratitude for Scientific Achievement

Almost every day we find in the papers very considerable lists of legacies and bequests. The fortunes bequeathed vary from millions of pounds down to a very few pounds. Sometimes those who leave large sums are well known; but it is a curious fact that the men who are best known leave the smallest sums. For example, we saw in *The Sunday Times*, of November 2, that Mr. James Boomer Charters, J.P., had left £225,817, that Miss J. A. Hankey had left £45,883, that Mr. J. S. Wyatt had left £34,477; and then, at the very tail of the list, comes the announcement that Lieut.-Col. H. H. Godwin-Austen, F.R.S., the pioneer explorer and geologist who surveyed large tracts of the Himalayas and various parts of Central Asia, left £90! We are not Socialists, but think that if the State could interfere in any way in the distribution of wealth it ought to interfere in one respect, namely, that the men who render the greatest services to the world should receive at least some of the good things which the world still appears to provide for those who have done it little service, or indeed, sometimes, much dis-service, or who have spent their lives chiefly in helping themselves. One would think that human beings have now developed sufficient intelligence to understand that it is to their own interest to help those who help them.

Flood of New Books

From several articles in the press we learn that the number of new books published this year is likely to exceed 9,500. Most of them, we presume, are novels, while the volumes of verse are said to reach sixteen a week. We have tried recently to read some of the more popular new novels, but the attacks almost always failed after the end of the first chapter. Perhaps our taste is a bad one; but we see so many dull people about that we do not care to spend the rest of the day in reading about others of the same type. Some persons, however, appear to enjoy the process. The truth is that modern novel-writing is merely a branch of the periodical press. The novels are brought out in numbers every day, and are meant to be thrown away a few days later. We wonder how many of them will last over the year, not to mention over the century. Art must tend to choke itself by excess of growth, like flowers in a garden. We are even very doubtful of the alleged classics of last century and we fear that the word "classic" as applied to the *Iliad* or the *Æneid*, will scarcely apply to any of our modern literature.

The Fifth Estate (R. R.)

The lords temporal, the lords spiritual, and the commoners were the *Three Estates* at the time when " Edmund Burke, pointing to the Reporters' Gallery, said: ' There sits a Fourth Estate, more important far than they

all.'" Dr. Arthur D. Little has found a fifth estate, in his paper read at the centenary celebration of the founding of the Franklin Institution, on September 19, 1904 (*Science*, October 3). The fifth estate is "the company of thinkers, workers, expounders, and practitioners upon which the world is absolutely dependent for the preservation and advancement of that organised knowledge which we call science. . . . It is they who bring the power and the fruits of knowledge to the multitude who are content to go through life without thinking and without questioning, who accept fire and the hatching of an egg, the attraction of a feather by a bit of amber, and the stars in their courses as a fish accepts the ocean." Leavened with wisdom and enlivened with wit, Dr. Little's magnificent address elaborates the true position of this fifth estate to-day. It constitutes a new priesthood of a new religion of a new world. Not the old world of craft and cunning, personal force and selfish intrigue, but one of massed progress through the tangled forest of opposing nature by means of inquiry and invention. With this new philosophy, men for the first time begin to approach the promised land. But the guides are few, the waverers many; and as yet we do but see the verge of our inheritance.

Notes and News

H.M. the King has approved the award, by the President and Council of the Royal Society, of Royal medals to Sir Dugald Clerk, for his work on internal combustion engines, and Dr. H. H. Dale for his researches in pharmacology and physiology.

The President and Council of the Royal Society have also made the following awards: Copley medal to Sir E. Sharpey-Schafer for his valuable work in physiology and histology: Rumford medal to Mr. C. V. Boys for his invention of the gas calorimeter; Davy medal to Prof. A. G. Perkin for his researches on the structure of natural colouring matters; Darwin medal to Prof. T. H. Morgan for his valuable work in zoology.

The Earl of Crawford and Balcarres and Sir Otto Beit have been elected Fellows of the Royal Society under the statute which permits of elections on grounds of conspicuous services rendered to science.

The chief officers of the Society for the session 1924–5 are: *President*, Sir Charles Sherrington; *Treasurer*, Sir David Prain; *Secretaries*, Mr. W. B. Hardy and Mr. J. H. Jeans; *Foreign Secretary*, Sir Richard Glazebrook.

Dr. J. W. Scott, of University College, Cardiff, has long been advocating the so-called Maxwell plan for the relief of unemployment. It consists in giving workmen attached to factories a small plot of land of about one-third of an acre to each cottage, so that when the said workmen are not employed at the factory they can cultivate their own gardens. Thus, workmen and factories would tend to migrate into the country and general prosperity would be increased. "Even if the cottage and third of an acre cost £800 to £1,000 (and why need it be anything like that figure ?), it could be repaid for around £1 a week; that is to say, a few shillings over what the workman very likely pays in rent already." Probably this idea is somewhat too simple for easy comprehension by our political logomachists.

The James Tait Black Memorial Book Prizes, instituted by the late Mrs. Janet Coats, or Black, of Millearn, Ayr, in memory of her husband, the distinguished Edinburgh publisher, have now been awarded for 1923 as follows: for the best biography or literary work of that nature, to Sir Ronald Ross for his *Memoirs* (John Murray); and, for the best novel, to Mr. Arnold Bennett for *Riceyman Steps* (Cassell & Co). Each prize amounts to over £141, and the awards were decided by Dr. J. B. C. Grierson, Professor of Rhetoric and English Literature, Edinburgh University.

Messrs. Williams & Norgate have recently published (price 1*s*.) the speech

of Maître Aubert of the Geneva Bar, before the criminal court of Lausanne, in defence of A. Polounine, who was tried there in November 1923 for shooting a Bolshevist emissary in the previous May. It will be remembered that the jury condemned the crime but acquitted the accused on the grounds of "irresistible impulse," a finding strictly in accordance with Vaudois law. The publication is called *Bolshevism's Terrible Record: An Indictment*, and is very well worth reading.

The Nobel prize for Medicine for 1924 has been awarded to Dr. William Einthoven, Professor of Physiology in the University of Leyden, chiefly for his notable invention of the string-galvanometer for recording the action of the heart. This adds one more to the long list of these prizes given since 1901 for physiology and hygiene, as against only four or five awarded for pathology. No doubt all of them have been more than well deserved; but one cannot help remarking that recent discoveries as to how diseases are caused and may be cured or prevented are really far more important than most of the studies of natural processes in the body which seem to be the chief preoccupation of the academies and which win most of the honours.

The *British Medical Journal* for May 24, May 31, and June 7, 1924, contained the Milroy Lectures on *Plague*, by Lt.-Col. W. Glen Liston, I.M.S., describing the researches which proved how that most terrible of epidemic diseases is carried by rat-fleas. It is characteristic of the barbaric attitude adopted in this country towards medical research that not a single honour or reward worth mention seems to have been given to any of the men who achieved that important and brilliant discovery—twenty years ago! Mr. Haffkine, whose vaccine saved innumerable lives in India when the Government was powerless, has been treated no better. We wonder how many honours have been given during this period to politicians, generals, nobodies, and quacks. Truly, though science advances with giant strides, yet the mind of the public remains to-day what it was in the dark ages.

Just as we are going to press we have received the twenty-first anniversary number of *School Nature Study* (George Philip & Son, 32 Fleet Street, E.C.4, price 1*s*. net). It is full of interesting articles on science and education by many men of distinction, including a number of the leading men of science.

The Council of the Institution of Civil Engineers has awarded a Telford gold medal to Mr. E. H. Lamb and a Telford Premium to Prof. L. N. G. Filon, F.R.S.

Prof. Eddington has been elected an honorary member of the American Astronomical Society and Prof. Soddy a foreign member of the Reale Accademia Nazionale dei Lincei of Rome.

Dr. H. Knox-Shaw has been appointed to succeed the late Dr. A. Rambaut as Radcliffe Observer by the trustees of the Radcliffe Observatory, Oxford.

Prof. F. Paschen has been appointed Director of the Reichsanstalt.

Sir William Bragg opened the new College of Pure Science in the University of Durham on October 1, and after the ceremony received the honorary degree of D.C.L. in the University.

Mr. S. A. Courtauld has given a sum of £20,000 to the University of London for the foundation of a chair of Anatomy at Middlesex Hospital Medical School.

Prof. H. Kamerlingh Onnes, of the University of Leyden, has resigned and Prof. W. J. de Haas of Groningen has been appointed to succeed him.

Mr. Ernest Gates, of Norfolk, has offered to complete the sum of £33,000 required to enable the University of Cambridge to accept the offer of the Rockefeller Trustees to build and partly endow a School of Pathology, provided the sum necessary to complete the endowment could be obtained from other sources.

We have to record with regret the announcement of the death of the following scientific men during the past quarter: Lord Abercromby, archæo-

logist; Mr. F. H. Bradley, O.M., philosopher; Mr. J. Britten, editor of the *Journal of Botany, British and Foreign*; Dr. J. E. Campbell, F.R.S., mathematician; Dr. E. G. Deville, Director-General of Canadian Surveys; Sir Archibald Geikie, O.M.; Dr. J. E. Gilpin, professor of chemistry at Johns Hopkins University; Mr. O. Hehner, analytical chemist; Dr. W. B. Hemsley, F.R.S., botanist; Mr. H. Lupton, physiologist; Dr. S. H. C. Martin, F.R.S., pathologist; Mr. H. G. Smith, Australian chemist; Prof. T. Wertheimer, chemist.

The Viceroy of India has sent a donation of Rs. 1,000 and the Maharajah of Nepal of Rs. 1,500 towards the fund for the foundation of the Ross Institute and Hospital for Tropical Diseases.

As a result of the Corn Production (Repeal) Act a further sum of £500,000 has been allotted for agricultural education and research. This is an addition to the £850,000 which was granted in 1921 for this purpose, and also to the annual grant which provides payment for the statutory activities of the Development Commissioners. The Ministry of Agriculture announces that the new money will be applied in part to research on foot-and-mouth disease, veterinary research, soil surveys and the National Institute of Agricultural Botany.

Two thousand five hundred quarters of a new wheat, Yeoman II, bred by Professor Biffen, has been sold this autumn by the Council of the National Institute of Agricultural Botany. The new wheat will replace the original Yeoman variety, which is now difficult to obtain as a pure seed free from admixture with other varieties. Both wheats are products of the cross Browick × Red Fife, but the new variety is decidedly superior. Its yield has been thoroughly tested on different soils in various parts of the country, and the flour is of exceptionally good quality.

The committee appointed by the President of the Board of Education "to indicate what are the principal changes now most needed in the existing constitution of the University of London, and on what basis a Statutory Commission should be set up to frame new Statutes for the University" is constituted as follows: Lord Ernle (chairman), Sir Robert Blair, Dr. H. L. Eason, Mr. H. B. Lees-Smith, Sir Henry Miers, Prof. A. F. Pollard, Sir Amherst Selby-Bigge, Miss K. Wallas, and Mr. S. H. Wood (secretary). A Royal Commission on University Education in London (generally known as the Haldane Commission) sat before the war, and its final report was dated March 1913. No effect has hitherto been given to the recommendations of this body, and the Departmental Committee now appointed will inquire whether some modifications in the scheme may not be desirable. The Senate is of opinion that any changes which may be necessary in the organisation of the University would be best effected by amendments initiated by the University itself. That the statutes and organisation of the University require very considerable modifications can hardly be a matter of dispute; the danger attached to any change is that it may result in those who really constitute the University (its teachers, research workers, and students) having even a smaller share in the control than they have to-day.

A meeting, attended by many foreign delegates, was arranged by the Netherlands Chemical Society in Amsterdam to celebrate the jubilee of the publication by Van't Hoff of the first paper on Stereochemistry. In his paper Van't Hoff introduced, for the first time, a mechanical theory of valency and connected the optical activity exhibited by many carbon compounds with their chemical constitution. In this connection, however, his name is generally joined with that of Le Bel, who, two months later, published, in France, a paper dealing with asymmetric carbon compounds. They jointly received the Davy medal from the Royal Society in 1893 on account of this work. Le Bel, who is now in his seventy-eighth year, was invited to the jubilee celebration of the work, but unfortunately was unable to be present.

Some years ago De Sitter, the Dutch astronomer, expressed the view that

the universe must be regarded as a "sphere" in a four-dimensional manifold, and, at the British Association meeting at Toronto last August, Dr. L. Silberstein showed how it is possible to estimate the radius of curvature of this "sphere." His calculations were based on the observed shifts of the lines in the spectra of certain very distant star-clusters and clouds. These shifts are, of course, in the main due to the Doppler Effect; but, according to Einstein, a small part may be due to an apparent decrease in the period of the light-waves from very massive sources. Starting from this hypothesis, Dr. Silberstein estimates the radius of curvature of the universe as 67×10^{19} miles (*i.e.* about 114 million light-years).

Dr. Harlow Shapley states that, as a result of photometric investigations of a series of photographs, taken at the Arequipa station in Peru, of the *Small Magellanic Cloud*, the magnitude of the stars in it has been determined, and thus the magnitude and distance of the system have been estimated. This distance comes out as 32 kiloparsecs (or, in rational units, about 100,000 light-years), while the diameter of the cloud is 6,500 light-years. More than half a million stars in the system are at least a hundred times as luminous as the sun. Some of the brightest are shown by their spectra to be of the red type, so that the intensity of light emission is low, and their size must be enormous to have so great a total brightness. The largest have, in all probability, a diameter of the order of 1,000 million miles, about four times that of Betelgeuse. The Small Magellanic Cloud is moving away from our galaxy with a velocity of about 100 miles a second, and Dr. Shapley is of opinion that both it and the Large Cloud were in the Milky Way at a time more recent than the Palæozoic era.

In the *Comptes Rendus* (July 21 and Aug. 8) E. Mathias proposes a new explanation of the origin of thunder and of ball lightning. His suggestion is that at the very high temperature of the flash heavy complex molecules of oxygen and nitrogen may be formed (such as O_{18}, N_{18}) which, on cooling, break down with the explosive violence. Ball lightning is then regarded as a flattened cylinder of the heavy molecules formed by an intense flash near the ground. Mathias quotes one instance in which ball lightning behaved in the manner his theory would lead one to expect, but has no other experimental evidence to support it.

The *Handbook to the Exhibition of Pure Science*, arranged by the Royal Society at Wembley, is deserving of all the praise which it has received. It is divided into two parts, of which the first contains a simplified account of those branches of science which are exciting most interest at the present time. Each subject is treated by an expert, sometimes by an article specially written for the Handbook, sometimes by a suitably edited extract from an address to a scientific body, and, in a few cases, by the reprinting of an article written for the British Science Guild Publicity Service. Whatever the original purpose of the articles, as presented here they are admirably suited for their purpose, *i.e.* to give the intelligent layman an interesting, true, and unexaggerated account of the present position of science. We are glad to note that the Handbook may be obtained from Messrs. A. F. Denny, Ltd., 163A, Strand, London, W.C.2. (Price 1*s.* + 3*d.* for postage), but regret that it is not brought more prominently before the public by sale on the bookstalls. Would it not be worth while to reprint the Handbook without the description of the exhibits which forms the second part of the work, and then sell it through a large distributing agency at the lowest possible price?

The Colonial Office has issued a Report of the work of the *Imperial Mycological Conference* held at the Imperial College of Science last July (H.M. Stationery Office, Adastral House, Kingsway, W.C.2. Price 9*d.* net). The proceedings were opened by Earl Buxton, who pointed out that, for the progress of the tropical Colonies, the fight against diseases of plants is scarcely less important than that against human diseases; a fact much better appreciated in the Dominions themselves than in the home country. The mycological work of the Empire is centralised in the Imperial Bureau of Mycology,

in charge of Dr. E. J. Butler, C.I.E., which has at present an income of £5,075 per annum from Government grants. The staff comprises just eleven persons, including the charwoman and an indexer (both part time), and only a relatively small amount of work can be done. A Committee appointed to consider the future work of the Bureau turned down many important and useful suggestions on account of the expense involved by the increase of staff which would thereby be made necessary, and only ventured a modest request for one additional mycologist and increases of salary which would raise the cost of the Bureau to £6,500 per annum. A large part of the work carried out would appear to consist in the preparation of the *Review of Applied Mycology*, whose publication involves an annual loss of some £600 on printing and distribution alone.

The *Empire Forestry Journal* for July last contains an account of the afforestation work which is being carried on in this country, and emphasizes the need of a more settled policy relating to this matter. Shortly after the conclusion of the first Imperial Forestry Conference in 1920, the Geddes Committee recommended that the Forestry Commission called into being by the Forestry Act of 1919 should be disbanded, and its work abandoned. The recommendation was not accepted in its entirety, but the sum allotted for the work was halved until, later on in the year 1921, urged by increasing unemployment, the Government requested the Commissioners to proceed with a planting programme much larger than that originally proposed!

In 1923 about 25,000 acres of plantable land were secured and 10,000 acres actually planted, 18,000,000 trees being used for the purpose. Mr. Ramsay Macdonald, during his tenure of office as Premier, expressed the view that the annual planting should extend to 30,000 acres instead of 10,000, and it is to be hoped that the new Government will support his proposal. In any case, a settled policy is required, for the trees themselves take from two to four years before they are fit to place in a plantation, and of course many years longer to mature. The supply of timber from North America is being exhausted very rapidly, and before trees planted to-day can mature it is expected that the United States will become a competitor for foreign supplies instead of an exporter, and the situation which will then arise may be a serious one. It ensures, however, that money spent on timber production will turn out in the end a good investment.

The Thirteenth Annual Conference of Educational Associations will be held at University College, Gower Street, W.C.1, from the 1st to the 8th January, 1925. The Honorary Organising Secretary and Treasurer is Miss Busk, 14 Taviton Street, W.C.1. Among much other matter, there will be a discussion on Simplified Spelling on Saturday, January 3, at 10.30 a.m.

CORRESPONDENCE

To the Editor of Science Progress

THE CAUSATION OF CANCER

I—From J. Ellis Barker

Sir,—In your October number Dr. Archibald Leitch, in reviewing my book, *Cancer—How it is Caused, how it can be Prevented*, makes several statements which I cannot allow to pass. He writes, "The thesis that cancer is a disease of civilisation is easily disposed of," and in vague generalities he denies that cancer is a disease of civilisation. There are, of course, a few cases of cancer—sarcoma is a different matter—among primitive races, and as a rule they occur among primitive people who have adopted the food and manners of civilisation. However, according to more than a hundred medical men, living among Red Indians, African negroes, etc., whom I have quoted or referred to, cancer is practically unknown among primitive tribes. In the September issue of *The Fortnightly Review* I have quoted a statement of Colonel Halliday, of the Indian Medical Service, who during more than twenty years has been working among the Hill-men. He has performed thousands of operations upon them, but has not had a single case of internal cancer. My thesis that cancer is a disease of civilisation seems to me unchallengeable, and if Dr. Leitch had read the mass of evidence I have quoted he would probably share my opinion. The fact that, in Dr. Leitch's words, "cancer occurs in all sorts of animals, mammals, birds, fishes, etc.," does not disprove my statement. It seems to me not permissible to confuse human disease and animal disease. Some learned investigators are even studying cancer in trees. There is, after all, some difference between humans, animals, fowls, fishes, and plants.

Continuing, Dr. Leitch proclaims: "The second contention, that cancer is increasing at an alarming rate, is not borne out by mortality statistics carefully interpreted." I must also challenge that statement. Civil servants are the most cautious of people. The 1923 Report of the Ministry of Health gives on page 13 a lengthy table, showing that between 1838 and 1921 deaths per million from cancer in England and Wales have increased from 173 to 1,215. Commenting upon the table the report states: "From these figures it will be seen that the death-rate from cancer is now more than seven times what it was in 1838." Under the heading, "Is Cancer Increasing?" the Report considers in nearly nine pages whether this apparent increase is real or fictitious. On page 21 the result of a careful critical examination regarding the English figures is summarised in the words: "The result of an application to the extensive data available in England and Wales would appear to confirm the popular impression that *the recorded increase of mortality from cancer is not a statistical fiction, but an established fact*," while with regard to the cancer mortality elsewhere the Report states on page 22: "As in this country, *the recorded mortality from cancer is increasing throughout the world.*" The italicised portions are italicised in the original. I am afraid Dr. Leitch is mistaken. The fact that cancer is increasing alarmingly cannot be questioned.

The teaching of my book is that cancer is due to chronic poisoning and vitamin starvation. Among the chronic poisons leading to cancer there are the bowel poisons regarding which Sir Arbuthnot Lane has done pioneer work of inestimable value. Dr. Leitch cannot disprove Sir Arbuthnot's teaching by telling you of his having experimentally " injected into the lower bowel of a large number of animals for periods of nine months several of the products of intestinal putrefaction without inducing any lesion whatever." These experiments seem very artificial, and are to me very unconvincing. McCarrison has shown by his experiments, which I have quoted very fully in my book, that vitamin starvation in animals leads to the ballooning of the stomach and to serious lesions throughout the alimentary canal. Through these lesions bowel poisons are readily absorbed into the system. Possibly, but not necessarily, Dr. Leitch might have produced cancer in the alimentary tract of animals by producing in them bowel lesions through vitamin starvation and chronic constipation without injecting " products of intestinal putrefaction." But that result need not necessarily have followed. Men and animals react differently. Certain animal diseases attack man, certain human diseases may be caught by animals, while other diseases are not transmissible from men to animals or from animals to men. These are fundamental facts which animal experimenters overlook only too often.

In his concluding sentence Dr. Leitch seems to suggest that I recommend my readers to " dine on the unappetising dishes of savages " to avoid cancer. That is surely a very unnecessary distortion of my views. I have recommended a diet rich in vitamins and roughage, containing an abundance of raw fruit, salads, and wholemeal bread. I really fail to see that wholemeal bread and fruit are an unappetising diet fit only for savages, and Dr. Leitch need not have made this ridiculous mis-statement, which is really not very creditable to him.

Yours faithfully
J. Ellis Barker.

October 14, 1924.

II—From Archibald Leitch, M.D.

This letter adds nothing new to what the writer is pleased to call his " teaching," nor does it meet any of the criticisms passed upon it. I am prepared to believe the statement that more than a hundred medical men who have lived for various periods in more or less intimate contact with less civilised races may not have recognised a single case of cancer, just as I am prepared to believe that hundreds of people may have crossed the Atlantic Ocean without seeing another ship on the waters. We have, however, definite knowledge that the mercantile marine exists, and we have definite information from medical men that cancer occurs in primitive races. We have no means, and in the nature of things cannot have means, of determining the relative incidence in civilised and primitive countries. Mr. Barker seems to be incapable of distinguishing between an opinion and a fact: to him a nebulous notion is " pioneer work of inestimable value." A statement that the statistical study of admittedly unsound materials " would appear to confirm the popular impression that the recorded increase of mortality from cancer " is true, becomes in his mind overwhelming proof that the actual increase is alarming and cannot be questioned. At the risk of incurring Mr. Ellis Barker's displeasure, I shall continue to believe that man is an animal, that sarcoma is cancer, that certain tissue reactions are the same in all animals, that ignorant speculations are not so important as ascertained facts, and that pathologists know more about disease than Mr. Barker knows about logic.

Archibald Leitch.

ESSAYS

HISTORY AND AGRICULTURAL SCIENCE (G. E. Fussell, Research Branch, Ministry of Agriculture)

The value of historical research in connection with any particular industry has been constantly questioned. This is partly the result of looking at history as a fiction having little more real relation to our lives than a story or novel, partly the result of imagining that the methods we now adopt are the only possible methods. Why should any interest be taken in methods that have been superseded when it is incredible that we should ever dream of returning to them? How can it benefit us to know such methods, or their productive results? The present, however, has its roots in the past, and the relation between the past and the present in agricultural science is as clearly marked as it is in any other. Actually, of course, history is merely a statement of what has happened in the past, recent or remote. It may be the history of yesterday or it may be the history of old times.

The scientific value of the classical experiments at Rothamsted is universally known and admired, and, in point of fact, these experiments are little more than the story of the system of cropping continuously carried out for a period of years and recorded. Some of the most interesting and valuable of the results are admitted to be due to the long continuance of these experiments, although at their inception no such plan was formulated. Science in this case is entirely historical. It is a record of what was done in each year, and the effect upon the crop. In the grassland experiments, which have been carried out and recorded at Cockle Park for the past nineteen years, and which have resulted in the discovery of the best manures for grass to be used in characteristic soils in the county of Northumberland, the scientific knowledge was obtained from the story of the work and its results.

Agricultural development in England, as is also the case in continental countries, is inextricably bound up with the system of land tenure. Until the enclosures had taken place it was not possible to apply to agriculture the slight but increasing measure of scientific knowledge that was obtained in the seventeenth and eighteenth centuries. It was only in those districts where large farms held in severalty had become commonplace that people were able to introduce the new crop rotations, to use artificial grass, and to cultivate roots. Upon the smaller and mixed holdings in the common fields traditional implements were used, and the traditional rotation was followed because the pace of development was necessarily that of the slowest and most backward member of the community.

The enthusiasm for agriculture which marked the eighteenth century in this country culminated in the enclosure movement at its end. The movement was instigated by men who were anxious to utilise the new knowledge, and who could not adopt it on farms split up into small sections distributed in the common fields. Their efforts would have been negatived by the bad cultivation of their neighbours. The work of enclosure, which had been going on slowly, was greatly accelerated and the possession of the new holdings in severalty necessarily permitted experiments to be carried out

which the Continent was incapable of attempting, merely because their own system of land tenure did not undergo any such complete, concentrated, and speedy amendment.

In this connection the distinction between the agriculture of northern and southern France is interesting. In the south of France, which, in their day, was occupied by the Romans, the open field method of cultivation was not used in the same way as it was in the more Teutonic north, where the holdings were the characteristic, distributed acre strips of the common field system. In the south holdings were set out on the more classical rectangular lines, and artificial grasses, such as lucerne, which were known to the classical agriculturists, continued to be cultivated. In the north, where the open field system was, till very recent times, practically universal, artificial grasses were not cultivated, the ordinary three-field or two-field systems of rotation being used. The characteristic implements of the two districts also differed very much. But Young had found, in 1794, to his astonishment, that the enclosures of the south had not inspired improvement. The rotations employed were traditional, and the improved rotations, that were the consequence of the introduction of the turnip and the artificial grasses from Flanders, combined with the enclosures in England, were only passed on to France in the middle of the nineteenth century. The new system might not have become serviceable even then had it not been for the modifications in tenure introduced by the Revolution, not the least important of which was the permission granted to every proprietor to cultivate as he pleased.

In the polders, reclaimed from the sea, the most important consequence of enclosure, in Young's eyes, was the fact that the "complete control of the individual cultivator over the course of cultivation had existed for centuries." These conditions were to be found about the beginning of last century along the Rhine through Holland, and in some provinces of Germany, and it is worthy of remark that it was from this district of unrestricted cultivation that many of the improvements were introduced into England during the late seventeenth and eighteenth centuries.

Again, in Eastern Prussia, the Junkers had, since the sixteenth century, held a large proportion of the land in their own occupation and had been consistently adding to the extent of their possessions as opportunity provided, so that by the early years of the nineteenth century the Junkers were capitalist farmers of their own land of a type quite different from the landowners of England or the great nobles of France. During the first thirty years of the century the habit of farming their own land became consolidated, and, although the practice of leasing farms was not unknown in the eighteenth century, large farms, worked by their proprietors, were, and still are, a consequence of this movement in the early nineteenth century. The Junkers were enthusiastic agriculturists, and they learned and adopted the scientific methods, which had been used with such success in England. Their enthusiasm also led them to conduct experiments of their own, resulting in the development of German agricultural science which is so well known.

The very evident necessity for consideration of history in relation to agricultural science is thus clearly established, but, if any further evidence was wanted, it would only be necessary to cite the work of Arthur Young. The history he wrote was contemporary, but it cannot be described as anything else. In the course of his travels he examined and recorded divergent practices all over this country, in France, in Ireland, and in Italy, and what he recorded was the inspiration of many of our most valuable and interesting experiments.

Again, the application of the chemical discoveries of the eighteenth century to agriculture was made possible by the work and lectures of John Humphries, and it was only when the intensive period of the enclosure movement had come to an end that it was found practicable for a sufficient number of large

farmers to adopt the system of artificial manuring and careful cultivation he advocated.

On the face of it, perhaps the least important type of historical investigation is that which deals with the statistics of production, population, and so on. Statistics are notoriously difficult to understand, and uninteresting, but they can be made of human interest, and they can indicate what progress has been made and afford some kind of indication why it has been made, if they are properly applied. Common observation is responsible for a large number of statements, which can only be checked by the progress of historical investigation, and very frequently the results of common observation are found to be erroneous. Ultimately science depends almost entirely upon historical method.

REVIEWS

MATHEMATICS

Linear Integral Equations. By WILLIAM VERNON LOVITT, Ph.D. [Pp. xiii + 253.] (New York: McGraw-Hill Publishing Co., Inc., 1924. Price 15*s.*)

THE standard books on the theory of integral equations, such as those of Bôcher and Volterra, make extremely heavy reading by reason of the fewness of the assumptions made with regard to the kernel. By considering only linear equations with a continuous kernel and with a single integration, Mr. Lovitt has been able to produce an introduction to the subject which is simple and readable, though not lacking in rigour. It is based on the lectures of Prof. Oscar Bolza at the University of Chicago. An account is given of the three methods of obtaining solutions; the method of successive substitutions, due to Liouville, Neumann, and Volterra, which gives the solution as a power series in the parameter, converging within a certain range; the method of Fredholm, which regards the integral equation as the limiting form of a system of n linear algebraic equations when n tends to infinity and which gives the solution as the quotient of two infinite determinants; and the method of Hilbert and Schmidt, based on the fundamental functions, the solutions of the corresponding homogeneous equation when the parameter is a characteristic number. Applications are given to various problems of mathematical physics, differential equations with boundary conditions being shown with the use of Green's function to be equivalent to integral equations. At times the treatment seems rather long-winded, and spread over too many pages. For instance, is it necessary to take three lines to prove that if m is the greatest integer in $\frac{1}{2}n$ then $m < n$ (p. 125)? And there is a misprint in the last line, at that! In fact, misprints are far too frequent, especially in the determinants on pp. 37 ff. There is an occasional slovenliness in phrasing, too, such as " let u and v be any two reals constant with respect to x," and we confess that we cannot make very much of the passage (p. 85): " A curve C, $x = \phi(t)$, $y = \psi(t)$ is said to be *continuous* if ϕ and ψ are continuous. We write this

$$C^c \cdot \sim \cdot \psi \cdot \phi^c \cdot$$

The symbol $\cdot \sim \cdot$ is read *is equivalent to* or *implies and is implied by*. The symbol $\cdot$ is read *and*."

But these are slight blemishes on a good book, and can easily be removed in a new edition, which the publishers apparently expect with confidence, as they print " First Edition " upon the title-page.

F. P. W.

On the Direct Numerical Calculation of Elliptic Functions and Integrals. By LOUIS V. KING, F.R.S. [Pp. viii + 42.] (Cambridge: At the University Press, 1924. Price 3*s.* 6*d.* net.)

IF we take any two positive numbers a_1 and b_1 and form their arithmetic mean a_2 and their geometric mean b_2, and then do the same with the numbers a_2 and b_2, and so on, we obtain two monotonic sequences (a_n) and (b_n) which converge to a common limit l with extraordinary rapidity, even when the

numbers with which we began are of very different magnitudes. For example, if $a = 1$, $b = 0{\cdot}2$, then a_5 and b_5 are represented by the same number to fifteen significant figures. The limit l was called by Gauss the arithmetico-geometric mean of a_1, b_1, and was applied by him to calculate certain elliptic integrals. We have for instance

$$\frac{\pi}{2l} = \int_0^{\frac{1}{2}\pi} \frac{d\theta}{(a_1^2 \cos^2\theta + b_1^2 \sin^2\theta)^{\frac{1}{2}}} = \int_0^{\frac{1}{2}\pi} \frac{d\theta}{(a_n^2 \cos^2\theta + b_n^2 \sin^2\theta)^{\frac{1}{2}}}.$$

Prof. L. V. King has developed and extended this method of calculation and presents his results in this little book. He gives an outline of the main analytical steps and provides a compendium of formulæ for the use of a computer who requires to reduce to numbers expressions involving elliptic functions and integrals which arise in physical and astronomical problems. It is interesting to note that this matter was to have received special attention in the third volume of Halphen's *Fonctions Elliptiques*, which was left incomplete at the author's death. An appendix contains a large number of interesting and elegant formulæ concerning recurrence relations of the sequences (a_n) and (b_n), and will form a mine of problems for the student beginning the study of the Jacobian Theta-functions.

F. P. W.

Advanced Vector Analysis with Application to Mathematical Physics. By C. E. Weatherburn, M.A., D.Sc. [Pp. xvi + 222.] (London: G. Bell & Sons, Ltd., 1924. Price 15*s*.)

This book is a sequel to the author's Elementary Vector Analysis, which dealt with vector algebra and differentiation with respect to one scalar variable. Here we begin with partial differentiation and are introduced at once to the gradient of a scalar and the divergence and curl of a vector function. Then come the theorems connecting line, surface, and volume integrals, and, later, an introductory account of linear vector functions and dyadics. This general theory occupies four chapters; the rest of the book is concerned with applications to potential theory, the conduction of heat, hydrodynamics, rigid dynamics, elasticity and electricity, not forgetting some account of the restricted principle of relativity.

The treatment is admirably clear and interesting, and exhibits the advantages of the use of vector methods in mathematical physics, provided that they are kept in their proper place.

F. P. W.

Practical Calculus for Home Study. By Claude Irwin Palmer. [Pp. xx + 443.] (London: McGraw-Hill Publishing Co., 1924. Price 15*s*. net.)

This book is intended to give the essential ideas and applications of the calculus in as clear a form as possible. It is a book for the practical scientist, not for the mathematician, and as such is neither much better nor much worse than many others which have appeared on the subject. The more difficult points in the proofs are frankly stated, but are not presented. There is a long table of Integral and other formulæ given at the end of the text.

In some sense the book is a continuation of the Author's *Practical Mathematics*, to which frequent references are made; and in any case a good knowledge of elementary mathematics is presupposed. One of the most difficult parts to present to the practical scientist, the differentiation of the exponential and logarithmic functions, is overcome by a reference to the author's more elementary work. There is, however, a chapter on Analytical Geometry, but it is placed at the end.

The reader should perhaps be warned against a few American terms and practices which differ from the English customs, though there are none of any very great importance.

L'Espace, la Relation et la Position : Essais sur le fondement de la Géométrie. Par Le Vicomte de Guëll. [Pp. ix + 139.] (Paris: Gauthier-Villars et Cie, 1924. Price 10 frs.)

This is one of the immense crowd of books which are a result of the relativity boom. It is true that it purports to give a new basis for the development of geometry, but the author admits in his preface that he has been encouraged to write the book by the remarkable attention and intelligence with which "les lecteurs non spécialistes" have received recent works on relativity !

The author's main thesis is that it is possible to construct the whole of geometry without any geometrical axioms, and naturally he doesn't get very far and has to take refuge in vague generalities about direction and so on. For instance, here is his definition of parallels : "Deux droites parallèles sont celles qui ont la même relation de position par rapport à une troisième qui les coupe."

But the style is pleasant and the whole may very comfortably be read through in a sitting in an arm-chair. It contains a slight but on the whole accurate historical account of the rise of non-Euclidean geometry, which may be of service to those who are not acquainted with Bonola.

F. P. W.

PHYSICS

The Specific Heats of Gases. By J. R. Partington, M.B.E., D.Sc., and W. G. Shilling, M.C., M.Sc. [Pp. 252, with 46 figures.] (London: Benn & Co., 1924. Price 30*s*. net.)

An accurate knowledge of the thermal properties of many gases and vapours is of great importance in various technical enterprises. Tables of such data for steam are readily available and are in constant use, but the authors indicate that, up to the present time, the choice of similar data for gases and gas mixtures has been left to the individual requiring the information, and that such choice, if incorrectly made from the host of experimental results published, may give rise to a large error in drawing up, for example, the heat balance sheet of a furnace.

The authors must especially be thanked for having carried out so successfully the immense amount of historical research which has been necessary in order that results could be quoted from so many different sources, and also for their concise descriptions of the various experimental arrangements and methods which have been employed.

These include the work done on the explosion of gaseous mixtures, and, necessarily, the effects of pressure and of temperature on the values of the specific heats are fully discussed.

Other sections deal with the applications of a knowledge of these values in practical problems relating to furnaces, gas producers, etc. The last chapter deals with the theory of specific heats, and includes accounts of the kinetic theory of gases and of the quantum theory. References are given to over three hundred and fifty original papers. Tables of the values of the specific heats, "accurate and inaccurate," as determined by the various investigators are included. Wherever possible, these results have been recalculated by the authors from the original experimental figures and certain corrections have been applied in order that the results should be more strictly comparable. Messrs. Partington and Shilling have drawn up a further table containing a series of values, at 15° Cent. and atmospheric pressure, for the molecular heats of about twenty gases. These

values, they consider, may be taken as best representing the existing experimental data. This table would have been of greater value if some indication had been given as to the order of accuracy of the figures quoted. It seems that, with very few exceptions, the values of the specific heats of gases are not known to an accuracy greater than about one-half of one per cent., even when determined under the above conditions, which are those most favourable for accurate experimental work. The values given for acetylene appear to require revision. Further tables and large-scale diagrams give values for the molecular heats of some gases up to 2000° Cent.

One or two points require special mention. Besides a few printer's errors, there is a mistake in the description of the apparatus used by Brinkworth in the determination of the specific heat of steam. This is called a "throttling calorimeter"; as a matter of fact, in this work a "constant-flow method" was utilised. The method of experiment is described correctly, but the equation given as allowing for a heat loss depending on the flow is wrong. It would have been helpful if the actual magnitudes of the flows which were used in the various constant-flow experiments described had been given. Again, in a list of over fifty particular equations of state it is probable that Callendar's characteristic gas equation would always be included. The authors have omitted it, although it expresses so exactly the properties of steam within the usual limits of temperature and pressure reached in steam-engine work. Perhaps especially it should have been included, since one of the tables of the molecular heats of steam given is based on this equation.

Possibly some readers may feel a sense of disappointment that the authors, who have personally made such valuable experimental contributions to our knowledge of specific heat values, have not criticised, favourably or otherwise, the many experimental methods and results quoted as fully as seems desirable in a work of this character. In conclusion, the authors very rightly emphasise the necessity for more accurate experimental results in order that various theoretical deductions may be thoroughly tested.

J. H. B.

Matter and Change: An Introduction to Physical and Chemical Science. By W. C. D. Whetham, M.A., F.R.S. [Pp. vi + 280, with 103 diagrams.] (Cambridge: at the University Press, 1924. Price 7*s*. 6*d*. net.)

It is doubtful whether there is another scientific writer who could give in less than 300 octavo pages so clear and complete a survey of the modern aspects of physical and chemical science as is provided in Mr. Whetham's book, *Matter and Change*. It is to be feared, however, that the author scarcely realises how stiff a proposition modern scientific ideas are to the unscientific reader in spite of the wonderful lucidity of his explanations and of his apposite illustrative metaphors.

W. C. B.

X-rays and Crystal Structure. By Sir W. H. Bragg, K.B.E., M.A., D.Sc., F.R.S., and W. L. Bragg, M.A., F.R.S. [Pp. viii + 322, with 106 figures and 8 plates.] (London: G. Bell & Sons, 1924. Price 21*s*. net.)

This book, more than any other recently published, bears witness to the ever-increasing pace of physical research. The authors state in their preface that they have recoiled before the task of writing a book treating of all the work that has been done in the new fields opened up by the X-ray methods. This volume, therefore, follows the original edition in method, uniting physics, crystallography, and chemistry, and intended to give students a general idea of the methods which have been so fruitful. The textbook idea prevails throughout, with its very necessary summing-up at intervals, and timely mental jogs to make sure that the processes are understood.

After the first three introductory chapters the book falls naturally into the two parts indicated in the title. Fig. 9, on p. 25, which has often been reproduced, apparently shows the production of a real image by specular reflection. Figs. 65 and 70 contradict this. The so-called focusing action of a crystal is a doubtful process. The reference to black and red phosphorus on p. 42 is evidently to a new and hitherto unknown blend. In the heading to Table I, on p. 46, λ is misprinted for μ.

Chapter v deals with the absorption of X-rays. Further work on this subject has been published lately, but little improvement has been made on the empirical laws given in the book. The book would be handier for purpose of reference if the wave-length tables were placed at the end of the book. The chapter on X-ray spectra might then be enlarged. At present it is very difficult to read.

The second part of the book, on crystal structure, is written far more in detail than the first part. There is a short introductory chapter on the rudiments of crystallography, and the treatment then follows the lines of the first edition. In a foot-note, on p. 99, the conclusion is written, " the carbon atom must be slightly tetrahedral." This is a very vague statement. The meaning is probably that the electrons in the outer shells, which give the atom tetrahedral symmetry noted by the chemists, have a small but appreciable effect on the scattering produced by the atom as a whole. This effect illustrates the difficulty of the calculation of intensities in corundum and quartz. The statement, on p. 111, that the metals and acid radicles, in the isomorphous series discussed, probably exist as ions, is apt to lead the student to the conclusion that isomorphism has no real chemical meaning. Chapter xii, on crystal analysis and atomic forces, is probably the most interesting of the book to the general reader. Much of it is debatable, but all is suggestive.

Chapter xiii gives an account of the work which has been done on the intensity of X-ray reflection. The question is a very difficult one, and few will agree with the conclusion that the intensities of spectra give a most delicate indication of the relative positions of the atoms. The doubt about the structure of graphite makes the student sceptical about these calculations.

The chapter on organic crystals illustrates the great service that these methods have rendered us in the chemical field since the publication of the first edition of this book.

The statement, on p. 258, that the reflection maxima from certain planes are more intense because of the concentration of atoms in them is only a half-truth. The number of atoms contributing to the maxima are the same for all planes, even those of the most complicated indices. But the planes of greatest reticular density will probably have the simplest " grating " constitution, and thus give maxima of the greater intensity. The conclusion is the same, but the reasoning in the book seems to obscure the fact that reflection is not a surface effect, which is so necessarily insisted upon on p. 14.

The method of von Laue is explained in chapter xv, and will be a great help to students. The final chapter gives a short account of more recent work and a further discussion of some of the methods described earlier. There is a very useful appendix on the various crystals which have been analysed by X-ray methods.

Any recommendation of this book is superfluous. It is one of those books which, even at a guinea, the student must possess. J. H. S.

Elementary Crystallography. By J. W. Evans, D.Sc., F.R.S., F.R.G.S., and G. M. Davies, M.Sc., F.G.S. [Pp. vi + 134, with 181 figures.] (London: Thomas Murby & Co., 1924. Price 9*s*. 6*d*. net.)

As its title indicates, this volume is a textbook for beginners. Only very short references are made to the various methods of projection; but simple

calculations of Millerian indices, together with suggestions for practical work, are given at the ends of appropriate chapters. Of the thirty-two classes, only eleven of the more important ones are discussed in detail. The figures are very clear, and as simple as possible. A great deal of the penultimate chapter, on "Twin-crystals," will be very difficult reading for the elementary student; and the final chapter, on "The Thirty-two Classes of Crystal Symmetry," will be far above the heads of students to whom the book, as a whole, is suited. The new notation introduced by Dr. Evans is, no doubt, very convenient; but there are already several systems in use, so it would be far better to decide upon one of the established systems than to introduce yet another.

J. H. S.

The Natural History of Crystals. By A. E. H. Tutton, D.Sc., M.A., F.R.S. [Pp. xii + 287, with 166 figures, including 32 full-page plates.] (London: Kegan Paul, Trench, Trübner & Co., 1924. Price 15*s.* net.)

This book is a new edition of Dr. Tutton's popular book, *Crystals*, in *The International Scientific Series*, published in 1911. It has been considerably enlarged and, to a great extent, rewritten, and it is now impossible to call it a popular book. The style is informal enough, but, for the reader who does not possess a working knowledge of physics and chemistry, it will be far too difficult.

The first three chapters are introductory and contain an interesting account of the early history of crystallography, ending with Haüy's contributions to the science. Chapters iv and v constitute an elementary introduction to the study of the seven systems and their nomenclature. The next chapter, on the "Nature of Crystallisation," is short, but very clear, and, with the references given, complete. Chapter vii reverts to the elementary textbook with a description of the goniometer. The next two chapters give a good account of the work of Mitscherlich on isomorphism, and of the later work which distinguishes between morphotrophy and isomorphism. The author then shows how the modern atomic theory helps in the explanation of the significance of crystal structure, and leads up to an account of his own work on the variation of angles in isomorphous series. A chapter on the X-ray method of analysis follows, with examples of typical cases which have been elucidated by this method.

Polymorphism and enantiomorphism are next described. Chapters xv to xvii deal with the optical properties of crystals, and some account of liquid crystals is given in chapter xviii. The author then refers briefly to his own very extensive work on measurements of heat expansion and elastic constants. The final chapter discusses the various theories of crystal formation, and ends with a well-deserved tribute to Federov.

A glossary of technical terms and a list of the thirty-two classes are added as appendices.

The book is very well turned out, the plates in particular being very fine, even if a few are irrelevant. Dr. Tutton loves crystals for their beauty, and he wishes to bring others to that greater love of Nature which comes with increased knowledge of her works.

J. H. S.

Valence and the Structure of Atoms and Molecules. By G. N. Lewis, Professor of Chemistry in the University of California. [Pp. 172 and 27 figures.] (New York: The Chemical Catalog Co., 1923.)

In this monograph Prof. G. N. Lewis has written an account of the electronic theory of valency of which he was one of the pioneers. The earlier chapters are devoted to a description of the historical development of the atomic theory and the theories of the structure of the atom. Contrasting the static view of the structure of the atom with the dynamic model of Bohr, he puts forward

suggestions as to how these may be reconciled and attempts to weld them into a single theory (p. 56). Accepting the view that the electrons are in motion, he puts forward the hypothesis that the atomic nuclei are not necessarily the centre or focus of the orbits of the electrons, and that the orbits occupy fixed positions with respect to one another and the nuclei. He then lays stress on the part played by the pairing of the electrons in forming the chemical linking and on the grouping of eight electrons which so largely determines the valency of the atoms in a chemical compound. In the remainder of the book his theory is applied in discussions of the dualistic theory, the chemical bond, double and triple bonds, co-ordination number, the structure of molecules composed of atoms with small kernels, electromers, and electrochemical and magneto-chemical theories of chemical structure. In the last chapter he puts forward some interesting views on the discontinuity of the physical chemical process.

On p. 29 is reproduced a very interesting memorandum, made by Prof. Lewis in 1902, on the electronic structure of a number of atoms, before the paper by Abegg in 1904 on normal and contra-valencies; this diagram is a brief synopsis of the whole of the octet theory of valency. This brilliant speculation, not published until 1916, has already exercised a great influence on modern views of valency and will no doubt be of permanent value in chemistry.

The book is an expression of the author's own views, based partly on logical reasoning and partly on that mystic reasoning called intuition, and it not unnaturally forms fruitful ground for criticism. The test will, however, lie in the future.

W. E. G.

CHEMISTRY

Clouds and Smokes. The Properties of Disperse Systems in Gases and their Practical Applications. By William E. Gibbs, D.Sc. [Pp. xiii + 240, with 31 figures.] (London: J. & A. Churchill, 1924. Price 10*s*. 6*d*. net.)

The study of disperse systems in gases has had a twofold aspect; on the one hand, it has led to important discoveries in physics and chemistry, and, on the other, to a considerable saving of life and money in industry. The work of Millikan on the determination of *N* and *e* and that of Lodge and Cottrell on the electrical removal of the fogs and fumes produced in technical processes may be cited as examples of the two types of discoveries.

The author gives an interesting and comprehensive survey of the whole subject. On the theoretical side, he discusses the methods of formation, optical properties, stability, and methods of examination of water, metallic, and oxide aerosols, copious references being made to the work of Millikan, Whytlaw-Gray, and Wells and Gerke. On the industrial side, "this information is considered in its direct relation to the phenomena of meteorology, the problem of dust explosions, the industrial treatment of fumes and dusty gases, the manufacture of substances in a finely divided condition, and, finally, the use of smoke in warfare." The author is to be congratulated on the manner in which he has produced a very readable book from the widely scattered information which lay at his disposal.

W. E. G.

Instrumental Methods of Chemical Analysis. By W. N. Lacey. [Pp. vi + 95, with 18 figures.] (New York: The Macmillan Company, 1924. Price 7*s*. net.)

This is an outline of a brief course of laboratory instruction given in the California Institute of Technology. It consists of twenty exercises each requiring three hours for their completion. Each laboratory period is devoted to the use of a particular instrument, and all of the analytical methods have

considerable technical importance. In the appendix, a scheme is given for the allocation of experiments to students under which the time allowed a student to finish an experiment is strictly limited. The inflexible type of organisation proposed is hardly such as will develop the initiative and resource of a student.

It is difficult to visualise the conditions which call for such a course of instruction, for the experiments, with few exceptions, are usually included in courses on quantitative analysis or practical physical chemistry. This method of teaching is preferable to that of grouping analytical methods in one laboratory course. Thus electrometric analysis is best studied immediately after measurements on electrometric force, extraction analysis under organic chemistry, and microscopic analysis, fire assay, etc., in the inorganic laboratory.

The book has been carefully prepared. but is not very suitable for use in the laboratories in this country, W. E. G.

Applied Chemistry. By IRA D. GARARD, Ph.D., Professor of Chemistry in New Jersey College for Women. [Pp. viii + 496.] (New York: Macmillan Co., 1924. Price 15*s*. net.)

THE purpose of the book is to provide a textbook for college students who have taken a course in general chemistry, and who are further interested in the applications of chemistry and the manner in which it functions in modern life. Sufficient organic chemistry is included to make clear those portions which deal primarily with organic substances, whilst the chapter upon natural and synthetic organic substances constituted a short review of the subject.

The earlier chapters deal chiefly with the laws and principles of the science, the middle chapters (viii to xiii) are chiefly concerned with the question of foods and nutrition, the last chapters (xiv to xxii) discuss such varied subjects as cleansing agents, leather, rubber, paints, metals, ceramics and fuels, whilst at the end is included a series of exercises and laboratory experiments to illustrate the course.

The volume is well written and full of interest, and should prove of great value to teachers engaged in lecturing on general applied chemistry, and to students desiring to widen their knowledge of the subject. Most of the statistics and examples are, of course, taken from American sources, and would require to be supplemented by home-grown information to attain their maximum value in this country. F. A. M.

Aniline and its Derivatives. By P. H. GROGGINS. [Pp. viii + 256, illustrated. (London: Chapman & Hall, 1924. Price 18*s*. net.)

IT is just on a century ago that Unverdorben first prepared aniline by distilling indigo, but to-day aniline is far from being the chemical curiosity of 1826. Whole industries have been built up based upon aniline and its derivatives, such as synthetic dyes, drugs, explosives, flavouring matters, and the thousand-and-one products of modern organic chemistry; the Great War could neither have been fought nor won without its aid, and aniline and the products of the coal-tar industry have become of sufficient importance to merit a section of the Peace Treaty to themselves. It is only meet, therefore, that so important a substance should have a special literature of its own amongst which Mr. Groggins' work should fill an honourable position.

Although intended primarily for the technical man it includes much information of value to other chemists; to the student may be commended the chapter on "Cost Factors in Manufacture" and the very useful section upon "the action of acids and alkalies on metals used in chemical operations," which summarises much valuable information collected from various sources, whilst for the physical chemist various tables of physico-chemical data should be of value. F. A. M.

The Chemistry of Rubber. By B. D. W. Luff, F.I.C., Research Chemist, The North British Rubber Company. [Pp. ix + 232.] (London: Ernest Benn, Ltd., 1923. Price 25*s*. net.)

The very great technical significance of rubber is responsible for the existence of a large number of books dealing with the industry; most of these, however, suffer from a common defect, namely, that of tending to over-emphasise one or other aspect of the subject, being written either from the planter's, the rubber chemist's, or the manufacturer's point of view; the result is that, as a rule, they present far too much detail in one direction and omit or pass lightly over essentials in another. The present author has, however, with very excellent discretion, selected just those points which the average reader will want to know, and has, moreover, placed them where the reader is likely to look for them. The book opens with an historical chapter in which is recalled how the original comparatively insignificant use of caoutchouc as an eraser of pencil-marks has given this commodity its present name. The second chapter is devoted to latex and its composition and properties; the next two chapters deal with the sources of rubber and give an account of various grades of wild and plantation rubber met with in commerce. Successive chapters then describe the composition of crude rubber, the physical properties of raw rubber, the chemical properties of rubber, and its constitution and "synthesis," vulcanisation, the properties of vulcanised rubber, the factors affecting vulcanisation, compounding ingredients, and accelerators. Having thus described all the important stages through which the rubber passes from the moment at which it issues from the tree, as latex, until it is ready to be converted into the various forms of the manufactured article, the author has added a chapter devoted to the description of the processes of milling, calendering, and the production of rubber articles, proofed cloth, sponge rubber and dipped goods, together with the methods employed for vulcanising these. The last chapter gives a useful account of the methods of analysis of rubber. The book, which is remarkably free from misprints, is very clearly printed and well illustrated by twenty odd full-page plates which, owing to their size, and method of production, really convey an excellent impression of what they are intended to represent. Altogether the book may be strongly recommended as providing a very interesting, readable, and informative account of one of the biggest industries of the present day.

P. H.

Complex Salts. By W. Thomas, B.A., M.Sc., Ph.D. [Pp. xi + 122.] (London: Blackie & Son, 1924. Price 10*s*. net.)

Werner may be said to have accomplished for inorganic chemistry that which Kekulé and Van't Hoff accomplished for organic chemistry. Just as Kekulé, by his simple valency theory and benzene theory, enabled chemists to represent simply the structure, behaviour, and relationships of carbon compounds, so Werner, with his co-ordination theory, introduced an idea which has proved equally helpful in dealing with a large number of apparently complicated inorganic compounds which the ordinary valency laws failed to explain.

Kekulé's notions inspired the labours of Petermann, Hübner, Körner, Ladenburg, and others who, by their researches on orientation and isomerism in the benzene compounds, put his theories on a firm experimental basis. Similarly, Werner created a school which has been enabled to deal with questions of orientation, substitution, and isomerism in metallic compounds by much the same methods that have been of so much service in organic chemistry. He further carried into inorganic chemistry the theories of the spatial distribution of atoms elaborated by Van't Hoff and Wislicenus, by demonstrating the existence of optical and geometrical isomerism amongst

inorganic compounds. One more result of the theory was the recognition, in inner complex salts, of rings which include a metallic atom as a member, and which are governed as to stability by the considerations which were put forward by Baeyer in his famous strain theory. Indeed, these complex anions and kations of Werner contain large groupings of atoms held together by forces sufficiently strong for the chemist to study their structure with comparative ease.

What is the nature of these forces? What is the essential difference between auxiliary and principal valencies? What is the meaning of the co-ordination number? To give a satisfactory answer to these questions has been the chief difficulty of the co-ordination theory. We have been compelled, until recently, to have two sets of ideas, in the main unconnected with each other. One set we have kept applies to organic and simple inorganic molecules—the ordinary valency idea; the other set has been the co-ordination idea, which has been applied to complicated inorganic molecules. The connection between the valency idea and the co-ordination idea was not apparent. Recently, however, the electronic theories of Lewis, Langmuir and Bohr have been applied to co-ordination compounds, and we have been able to see a relation between these two, seemingly different, manifestations of chemical affinity.

Werner's theories are very inadequately treated in the average chemistry textbook, and until the recent appearance, in America, of a translation of a monograph by Schwarz, the only comprehensive English account of the subject was contained in Hedley's translation of the 1908 edition of Werner's classical work. Besides being out of date, this is a very difficult book to read on account of a bewildering number of headings and subheadings and the absence of an index, and it requires a very keen student to extract the main ideas from the huge mass of facts which tends to hide them. This is greatly to be deplored, since the co-ordination theory is a divining-rod which can be used to open up a very rich territory of important research—a territory which provides a meeting-ground for inorganic, organic, and physical chemists.

Dr. Thomas, therefore, undoubtedly performs useful service in providing the student with an account of some of Werner's ideas. Since it is impossible in a small monograph to survey all the ground covered by Werner and Weinland in their books, the author has described briefly two groups of complex salts—those with co-ordination numbers four and six. This has enabled him to bring out many of the most important experimental results which the theory explains, namely, the various types of isomerism, the properties of complex salts, and the factors determining their stability. Dr. Thomas has himself carried out important researches in this field, and this is reflected in the praiseworthy prominence that he gives to the experimental side of the subject. The treatment of the subject is somewhat unbalanced, and there are notable omissions; for example, the system of nomenclature, the theory of acids and bases, the oxonium salts and the electronic theories of Kossel, Langmuir, Lowry and Sidgwick have not been dealt with. It is important to discuss the last topic, since these theories throw much light on the vexed question of the difference between principal and auxiliary valencies. According to the author (p. 110), Werner concluded that these are identical. He certainly abandoned hope of demonstrating certain theoretically possible types of valency isomerism, but he still considered it desirable to distinguish between the two types, and in the latest edition of his work, edited by Pfeiffer, and in Weinland's book the distinction is maintained.

Dr. Thomas collects together a number of lose ends in chapter xi, which is somewhat unsatisfactory. One gets the impression from p. 102 that the four groups co-ordinated by nitrogen are coplanar. Contrary to the statement on p. 106, that the higher the valency of a metal of varying

valency, the more stable the complex, we find that the ferrocyanide complex is more stable than the ferricyanide complex, and that, whereas there are complex cupra cyanides, there are no cuprocyanides. Again, the tri-dipyridyl ferrosalts are more stable than the corresponding ferrisalts. It may also be pointed out that Duff has recently prepared a cobalt complex salt containing a succinato ring.

Students taking a chemistry degree will find this a fascinating and useful book which they cannot afford to neglect.

C. H. S.

A First Chemistry for Schools. By W. H. Hewitt, B.A., B.Sc., A.R.C.S., and S. T. E. Dark, B.Sc. [Pp. viii + 316, with 87 diagrams.] (London: Methuen & Co. Price 5s.)

The writers of this compendious work have perhaps attempted to satisfy too many claims at once. In the first instance, the book is intended as a practical course in chemistry for schools; at the same time, the authors have had the interests of the private student in view. This is not by any means an undesirable combination of aims. On the contrary, it should have produced an excellent text for developing initiative, an aim that the authors certainly have had in mind, for they rightly urge in their preface that the boys should be led to think out the details for themselves. Unfortunately, however, to quote again from the preface, the full instructions that are given are intended to save time. Thus, for instance, a whole page is devoted to hints as to how to use a pipette. It is true that occasionally the pupil is asked to find a reason for the hint, but in many cases it would be far better to lead the pupil to discover the need for the hint. Nevertheless, if only for its thoroughness of treatment, the book is worthy of consideration by science teachers, seeking to give a training in scientific method.

W. C. B.

Chemical Synthesis. Studies in the Investigation of Natural Organic Products. By Harry Hepworth, D.Sc., F.I.C. [Pp. xx + 243.] (London: Blackie & Son, Ltd., 1924. Price 20s. net.)

At a time when so much attention is being paid either to the purely academic or to the technical aspects of organic chemistry, one welcomes a book devoted to a description of " the more important investigations which have been made by the organic chemist in modern times in the domain of natural organic products." These same products do not always receive due consideration by the general reader, and it will therefore come as a surprise to many to see how great have been the advances made during the last few years in unravelling some of nature's secrets; the various stages in the historical development of the subject have been skilfully set forth in the short introduction which serves to excite curiosity for the contents of the subsequent chapters. Opening with a chapter on the photosynthesis of plant products, the author proceeds in successive chapters to give an account of chlorophyll and other natural pigments, the carbohydrates, lichen products and tannins, oils, fats, and waxes, and the terpenes and their derivatives; next follow chapters on amino-acids of polypeptides, some simple natural bases, pyrimidine and purine bases, and finally the alkaloids. Owing to the exigencies of space parts of the subject have, of necessity, received somewhat inadequate treatment, and others have been little more than hinted at; but in most cases deficiencies have been made up for by the quoting of references. The author has an easy, flowing style, and it must be acknowledged that he has produced a very interesting and useful book which can be taken up and read with profit at any point.

P. H.

The Simple Carbohydrates and the Glucosides. By E. FRANKLAND ARMSTRONG, D.Sc., Ph.D., F.R.S. Monographs on Biochemistry. Fourth Edition. [Pp. xi + 293.] (London: Longmans, Green & Co., 1924. Price 16*s.* net.)

IN the interval which has elapsed since the appearance of the last edition in 1919 the painstaking and fundamentally important work of the St. Andrew's School has considerably advanced our knowledge of the constitution of the carbohydrates. The preparation and the characterisation of all those possible methoxyl derivatives of glucose—the so-called reference sugars—has provided a reliable basis for the further elucidation of the constitution of both the glucosides and the polysaccharides. In these circumstances the author has very wisely included in this volume a new chapter, in which he summarises the present state of our knowledge of the three polysaccharides cellulose, starch, and inulin. The discovery of the existence of the unstable modification of glucose known as γ-glucose, and also of a γ-form of fructose, has already borne fruit, for it is suspected that the γ-form of glucose is the one normally occurring in blood, while γ-fructose is the form in which fructose occurs in both cane sugar and inulin. The present edition also contains an account of some of the less-known derivatives of glucose, such as deoxyglucose and glucosan, whose study has only been developed in recent years. For the rest, it is only necessary to say that the new edition has maintained the high standard of comprehensiveness and accuracy which the earlier editions have accustomed us to.

P. H.

Some Studies in Biochemistry. By some students of DR. GILBERT J. FOWLER, D.Sc., F.I.C., Indian Institute of Science. [Pp. iv + 197.] (Bangalore: The Phœnix Printing House, 1924.)

As stated in the foreword, the publication of this brochure represents an attempt by some of Dr. Fowler's past and present students to mark their appreciation of his qualities of head and heart, on the eve of his retirement from the chair of Biochemistry at the Indian Institute of Science. The compilation consists of a number of short contributions by various authors on different subjects. While the complete work forms a graceful tribute to a retiring professor, it does not contain much that would appeal to the ordinary scientific reader not directly interested in the Institute.

P. H.

Organic Syntheses, Vol. III. By HANS THACKER CLARKE, Editor-in-Chief. [Pp. 104.] (New York: John Wiley & Sons; London: Chapman & Hall, 1923. Price 7*s.* 6*d.* net.)

THE third annual volume of this valuable series is very welcome and organic chemists must hope that the publication has met with sufficient success to justify its continuation for many years. Carefully tested methods are described for the preparation in fair quantities of a further thirty organic compounds; a wide field is covered, and one may indicate the scope of the book by mention of eight out of the thirty—acetal, arsanilic acid, catechol, diphenylacetic acid, ethyl cyanoacetate, *p*-nitrobenzoyl chloride, nitromethane, and mercury di-*p*-tolyl. One inorganic preparation included which calls for special mention is that of hydroxylamine hydrochloride, as the method given is a great improvement on those usually employed. Advantage is taken of the fact that sodium hydroxylamine-disulphonate reacts with acetone to give acetoxime; the distillation of this compound in steam and its subsequent hydrolysis eliminate the large amount of sodium salts so troublesome in the older process. A collective index of the three volumes is included.

O. L. B.

BOTANY

Outlines of Fungi and Plant Diseases. By F. T. Bennett, B.Sc. [Pp. xi + 254, with frontispiece and 25 plates.] (London: Macmillan & Co., 1924. Price 7s. 6d. net.)

There are comparatively few elementary accounts of fungi which treat of just those common types which are either familiar objects or are of economic importance. The present small volume is intended to meet this need so far as concerns the species causing disease in plants. Its aim is to serve as a handbook for the agricultural or horticultural student, and begins with an account of the fungal groups on the type system. The types selected are Mucor, Pythium, Eurotium, Nectria, Peziza, Saccharomyces, Agaricus, and two lichens; each of these is described in some detail, and the description is followed by useful suggestions for practical work.

Part II treats of about forty of the common plant diseases, such as the club-root of cabbages, the blight, scab, and wart diseases of the potato, the silver-leaf of the plum, and rusts and smuts of wheat, etc. Three brief appendices follow, dealing respectively with fungicides and soil sterilisation, fungi injurious to live stock, and an excerpt from the Pests Act.

The 25 plates and frontispiece contain a large number of figures, which, if rather too diagrammatic, should nevertheless prove helpful to the novice.

E. J. S.

The Biology of Flowering Plants. By Macgregor Skene, D.Sc. [Pp. xi + 523, with 8 plates and 68 text figures.] (London: Sidgwick and Jackson, 1924. Price 16s. net.)

Some idea of the scope covered by this volume is derived from reference to the bibliography, where complete citations are given to more than six hundred papers referred to in the text. From this it will be gathered that many of the results of recent research are embodied in these pages, which are mainly concerned with the relation of the individual plant to its environment. In the presentation of this aspect of biology Dr. Skene has succeeded in writing an account which is at once interesting and instructive. As a book for the student it can be thoroughly recommended, as, though the writing is at times rather laboured, the meaning is never in doubt. For the lay reader it is less suited, since he will find, for example, that a knowledge of what is meant by colloidal properties is assumed in the very first chapter, which deals with the soil as the source of supply of water and mineral salts to the plant.

From consideration of the soil solution the author leads us on to the nature and distribution of root systems, to osmotic pressure and absorbtion.

The second, and longest chapter, occupying 140 pages, deals with the important processes of assimilation and transpiration concerning which so much work has been done during recent years. Here we have, passed in review, the researches of Blackman on limiting factors, of Willstatter and Stoll on the nature of chlorophyll, Lloyd's investigation on the action of stomata, of Lotfield on stomatal regulation, and many others. The third chapter is concerned with special modes of nutrition as exemplified by parasitic plants, saprophytes, plants with mycorhiza, and insectivorous plants. Here reference is made to the work of Kostychew, who showed that assimilation of semi-parasites may be as great as in autotrophic plants, thus lending support to the view that their parasitic habit is a means of augmenting the water absorbtion from their meagre root system. Also to the extensive literature on the subject of mycorhizal relationships.

The next chapter is concerned with the mechanical aspects of the plant as exemplified in their morphological modifications and grosser anatomical

features. The last two chapters deal respectively with Reproduction and Dispersal, and Development. Here floral biology is very briefly treated, and the same is true of seed dispersal.

It is essentially the physiological aspects of biology which are here chiefly exploited, and the student will in these pages find much to stimulate his interest and thought in this direction.

E. J. S.

Soil Management. By FIRMAN E. BEAR. The Wiley Agricultural Series. [Pp. vi + 268, with 33 figures.] (New York: John Wiley & Sons; London: Chapman & Hall, 1924. Price 10*s.* net.)

THE subject of soil management—by which are meant the cultivation and manuring of the soil as distinct from the other major operations of farming—is a very popular one in America. In addition to numerous independent books on the subject, nearly every series of agricultural monographs contain several, under different titles, of course, but very similar in contents.

One reason for this is the comparative youth of a permanent system of agriculture in America—the days of continuous cropping of virgin soil are well within living memory, and the development of irrigation farming with its host of new and urgent problems in soil management is even more recent. Another reason is inherent in the system of organisation of the State and Federal Agricultural Service—practical results from investigations are expected quickly. Both these causes are apt to lead to a tendency to generalise on insufficient grounds and to draw a definite conclusion from thin evidence.

It is refreshing to find in the book under review that this danger is recognised. As it is intended for use in agricultural colleges, and not for the advanced student, it is naturally a collection of general statements. But the statements are frankly admitted to be of this general nature. The tables in the text are given merely as illustrative data, and, although they represent only a small part of the experimental work on which the author's conclusions are based, it is clear from the context that nothing more than a reasonable degree of probability is claimed for these conclusions, in so far as they follow from the data. This point perhaps needs further comment; advanced students of soils—especially those concerned with soil cultivation—know that many practices asserted by the practical man to be of obvious value are as yet without adequate scientific explanation or confirmation. In such cases, where the "conclusion" is already stated in terms of practice, there is an unconscious temptation to confirm it on the basis of inadequate scientific data. The conclusion may be quite correct, but it certainly does not follow from the experiment. And soil cultivation problems are not alone in this respect; many—one could almost say most—of the fertiliser experiments on which definite recommendations of quality and time of application are based do not stand the test of statistical examination. To be sure of his ground the conscientious research worker in agriculture has frequently to carry his problem back to the fundamentals—to eliminate for the time any idea of producing a practical recommendation until he has cleared up some obscure and complex point in soil science. At the same time the farmer and the college student need advice and knowledge, not only in the general principles of the farming industry, but in specific details. This information has to be given with a due sense of the limitations imposed by the present state of scientific investigation in agriculture. The task is not an easy one, and any author attempting it must command a subtle quality of cautious intuition. Prof. Bear emerges with considerable success from this somewhat unnerving ordeal.

B. A. KEEN.

ZOOLOGY

The Evolution of Man. Essays by G. ELLIOTT SMITH, F.R.S. [Pp. vi + 159, with 19 figures.] (Oxford: at the University Press, 1924. Price 8*s*. 6*d*. net.)

IN the little work under review Prof. Elliott Smith reprints, with certain additions, three lectures delivered in 1912, 1916, and 1924, and adds to them a foreword in which he states his views of the mutual relationships of the various groups included in the order Primates.

The book does not contain detailed descriptions of the fossil human bones which are known, but provides an interpretation of their characters and an evaluation of the factors which have led to man's evolution. It thus forms a most valuable companion to such books as Boule's *Fossil Men* and Keith's *Antiquity of Man*.

Elliott Smith emphasises the fact that it is in his brain and mind that man differs essentially from the great apes and from his own ancestors. He thus ignores, as a purely secondary problem, those changes in proportion which have been brought about during the assumption of an upright posture and bipedal gait. The shortening of the arms and lengthening of the great toe, features on which much stress has been laid by certain anatomists, are after all adaptive changes of a type whose history is familiar to palæontologists: whilst that enormous enlargement of the brain which distinguishes man, with all the mental changes which are associated with it, is unique, and must form the real subject of any serious study of man's origin.

From its initiation the group of Primates has undergone a series of improvement in brain structure, each in turn associated with some special habit of life, and of these the development of man's brain is the latest. The retention of the arboreal habit of early mammals in general by the ancestors of the lemurs led to an increased cultivation of those senses which are concerned with balance and an enlargement of those regions of the brain which serve them. The Tarsioids exhibit a further advance in the introduction of binocular viscord. The establishment of a mechanism for carrying out conjugate movements of the eyes, and of a manila lutea in the retina, by the monkeys made possible a more accurate judgment of distance and an appreciation of form.

That use of the fore-limbs for handling food, which is made possible by the habit of sitting erect, by encouraging the development of all those senses, both tactile and muscular, which are necessary for the carrying out of skilled movements forms an essential part of the story: for the information so conveyed to the brain forms the basis on which, by a long process of education, man builds up his power of judging the shape of an object by its visual appearance.

Finally, the establishment of that auditory symbolism, which is speech, enables man to take advantage of the experience of his ancestors and has made possible the whole of civilisation.

Such, in brief, are the main pivots of Elliott Smith's argument, brought together here for the first time and supported by a wealth of evidence drawn from the most varied sources. As so presented, the story of man's origin becomes intelligible, it is brought into line with the evolution of other mammals, and shown to represent the natural end of primate evolution, first one set of senses undergoing elaboration, another being then added, until finally all combine to provide that wealth of sensory information which stored in the brain forms that background of experience which enables us to appreciate the outcome of our actions and to make judgments of the most profitable course of behaviour.

Prof. Elliott Smith's exposition of this difficult subject is admirably lucid, and his delightful style and the excellent illustrations should render the book attractive to all those who are interested in the problems of their own origin.

D. M. WATSON.

Biology and Human Welfare. By J. E. PEABODY and A. E. HUNT. [Pp. x + 585, with 354 illustrations and diagrams.] (New York: The Macmillan Company. Price 8*s.* net.)

FROM some points of view this work may be regarded as an American companion to Sir Arthur Shipley's *Life*, recently reviewed in this journal. The economic point of view and a rather materialistic valuation of the life in and around us are its most prominent features; the title of the closing chapter, "How success in life is won," gives the key to the spirit and purpose of the book. The frequent references to the deleterious effects of alcohol, as well as of other drugs, not excluding tobacco, are characteristic of the American's habit of immediately translating scientific knowledge into terms of daily application. If only for this element of practicality the book is worthy of perusal by English readers, especially teachers.

W. C. B.

The Pigmentary Effector System. By L. T. HOGBEN, M.A., D.Sc., F.R.S.E. [Pp. xii + 152, with 17 illustrations.] (London and Edinburgh: Messrs. Oliver and Boyd. Price 10*s.* 6*d.* net.)

THIS volume is the first of a series of biological monographs and manuals now in course of publication by Messrs. Oliver and Boyd on those aspects of biology which have received most attention in recent years. A number of such series have appeared recently in America, and the present one, dealing with the work of English investigators, will be very acceptable.

These short memoirs are an outward and visible sign of the modern tendencies of biology, and point clearly to the direction in which the science is moving. It would seem that the old descriptive morphology has for the moment worked itself out, and that the newer generation of workers are becoming dissatisfied with the limitations of its methods, for much that is now appearing is being won by the weapon of experiment. Some of the volumes in the present series are purely physiological, but others deal with problems which lie near to the point towards which physiology and morphology converge.

Dr. Hogben's book deals with an aspect of biology which has hitherto chiefly been the concern of the systematist and speculative evolutionist, but is here approached from the point of view of physiology. It deals with the physiology of colour response in animals.

Most space is devoted to Reptiles, Amphibia, and Fishes. There is also a chapter on Crustacea, and a brief appendix on Molluscs. Great diversity appears to exist in the mechanism of colour response in these different groups. In Reptiles the evidence points to the adrenals as the organs involved. In Amphibians, chiefly through the work of Dr. Hogben, the hypophysis cerebri has been shown to be essential to normal colour response. Only in fishes does there appear to be satisfactory evidence that the nervous system takes an essential part. In Crustacea the effectors themselves, and the influences governing them, are much more complex than in the other groups, and our knowledge is chiefly confined to that derived from a study of the responses normally called forth by natural stimulation, an aspect in which Dr. Hogben seems to be less interested than in the internal mechanism by which they are effected.

The pigmentary effectors of Molluscs differ totally from those of the other groups discussed in this book in possessing true muscular elements, but our knowledge of their physiology is still very scanty.

Dr. Hogben writes in an admirably terse and lucid style, and his book contains some valuable critical discussions on the supposed sympathetic innervation of the receptors in Amphibia and on the supposed analogy between the action of certain drugs on the pigmentary receptors and their

possible innervation. The book also contains some interesting suggestions on the light thrown on the nature of receptors in general by these studies on those of the pigmentary system. There appears to be a word omitted after "anterior" on the fifth line from the bottom of p. 122.

J. H. W.

Growth. By G. R. DE BEER, B.A., B.Sc., F.L.S. [Pp. viii + 120, with frontispiece, 7 plates, and 17 illustrations.] (London: Edward Arnold & Co., 1924. Price 7*s.* 6*d.* net.)

ROUND the special problem of growth Mr. de Beer has succeeded in writing a very readable introduction to elementary biology. The book is intended for the general reader, and technical terms are avoided as far as possible. A great many have inevitably crept in, and some might with advantage have received more extended elucidation. The first seven chapters are, none the less, lucidly written from the point of view of the general reader, and will be read with profit even by regular students of biology, for they contain much that is too often scantily treated in student textbooks. These chapters deal with concrete examples of growth and development in animals and plants, and with the phenomena of regeneration, asexual reproduction, and abnormal growth.

The remaining chapters are less satisfactory. They deal with the results of modern methods of approach to the problems of growth and, consequently, with results that must be accepted critically and tentatively. This aspect does not seem to us to have received sufficient emphasis. In chapter viii, "Causes and Nature of Growth," we pass from the hypothetical "genes" of modern Mendelian investigation to parthenogenesis, enzymes, and modes of nutrition in animals and plants in a rather disconnected fashion. In chapter ix the conclusions of Sand and Steinach are stated as established truth, with no mention of the critical work of Moore. The eleventh chapter, on "Size," and the environmental conditions which limit it, is good, and calls attention to a great number of interesting facts and correlations. Chapter xiv is valuable in calling attention to the work of Child on "axial gradients." The inorganic analogies with growth are presented in chapter xv with more caution. In the concluding chapter the author defines his position as one "steering between 'materialism' and 'vitalism,'" and calls it "mechanism"; but, as these terms are not defined by the author, it is difficult to determine exactly what is meant by them, for, like "socialism," they have been applied to such diverse conceptions as to mean almost nothing. It would seem, from what he says on p. 112, that Mr. de Beer's views differ from those of mechanists, as that term is commonly understood; but "mechanism" and "materialism" are frequently employed as though they were synonymous. A new term is wanted to define the position midway between the overweening confidence of "mechanism" and the impotence of "vitalism," or of much to which that term has been applied. The term "materialism" is so vague and has been used in realms of thought so far removed from biology that it is best avoided altogether in biological literature.

Mr. de Beer has done a valuable service in bringing together in a small compass a great many important new facts bearing on the fundamental problems of biology. His book will fill a place on our shelves too long left vacant. It will bring to the notice of students of biology some of the most vital parts of their subject, and it will provide a starting-point for those who wish to pursue the problem of growth further, since it contains in an appendix an extended bibliography of references to recent papers, in addition to more general references at the end of each chapter.

J. H. W.

Birds in Legend, Fable, and Folklore. By ERNEST INGERSOLL. [Pp. v + 292.] (New York and London: Longmans, Green & Co., 1923. Price 12*s.* 6*d.* net.)

THE author of *Birds in Legend, Fable, and Folklore* has successfully brought together a most astounding assortment of fables concerning birds. It would be quite impossible to attempt even an inadequate outline of so great a variety, but the book is one of continual surprises. Perhaps the greatest of all concerns the crow, that arch-thief and vagabond, reviled and hunted down by farmers and sportsmen the world over. For he was, once upon a time, white! And he was, also once upon a time, deemed sacred!

Mr. Ingersoll avoids the general field of mythology, trespassing only here and there. Whilst he evidently tells his ancient stories in the spirit of an unbeliever—from his tone one knows, for instance, that he does *not* believe that the crow was ever white, either in body or in soul—yet he seems to come dangerously near accepting some more modern fables. Thus, he is quite sure that an early autumn migration portends a severe winter.

References to ornithological facts are, on the whole, infrequent, but by no means free of error. Whimbrel and Curlew are not one and the same thing; the barred plumage of the Snowy Owl is not the summer plumage but the perennial female attire; *Branta bernicla* is not *supposed* to breed in Spitsbergen: it *does* breed there.

But these are negligible flaws in a most entertaining volume, filled with information that has been assiduously and laboriously unearthed from the archives of all nations. It is a monument to human credulity as well as to a universal regard for the feathered hosts of the earth. It can be heartily commended to any and all who take an interest in birds.

W. R.

The Peregrine's Saga and other Stories of the Country Green. By HENRY WILLIAMSON. [Pp. vii + 301, with illustrations by WARWICK REYNOLDS.] (London: W. Collins & Sons, 1923. Price 7*s.* 6*d.* net.)

THIS book of animal stories differs in some essential respects from other similar volumes. For one thing, most of the tales have an unfortunate ending. This is no doubt as it should be if the pictures are to be true to life, where the happy ending is a rare event. But there is something beyond that in many of them—the gruesome touch of *le grand guignol*. Perhaps this is also as it should be, but we hope not. The knighted squire who gleefully releases a deliberately blinded peregrine to avenge himself for raids on his pet pigeons, or the pauper who freezes to death after his last meal of rat and owl, can hardly be every-day personages.

The author has an easy and vivid manner of recounting his tales, and is evidently on intimate terms with the wild life of the countryside. Although his interpretation of animal actions is distinctly anthropomorphic, he takes but few liberties with facts. If his raven does live to become a centenarian and his peregrine stoops at pigeons at two hundred miles an hour, and he mates his carrion crow with a rook, these are perhaps legitimate liberties, for they concern the heroes of stories that are exceptionally well told.

W. R.

Protoplasmic Action and Nervous Action. By RALPH S. LILLIE. [Pp. xii + 417.] (University of Chicago Press, Chicago, Illinois, 1923.)

MR. LILLIE represents the American school of what he himself might perhaps describe as synthetic biology. It is his contention that, whilst the analytical method has contributed fundamentally to the rapid march of physiological knowledge, there is in the synthetic approach a method of great value which has rather languished in neglect. It is not sufficient to analyse the reactions

which unite to comprise life; it is necessary to investigate the very organisation, the integration of activities, by which life perpetuates its own vitality.

The larger half of the book discusses the intimate nature of cellular organisation. Having conceived protoplasm as a polyphasic film-partitioned system, the author devotes much consideration to the relation of surface phenomena to the stability, permeability, and irritability of the system as conditioned by interfacial changes. The cell becomes a convenient rather than a fundamental unit of protoplasm. The discussion follows that of Höber in his classical *Physikalische Chemie der Zelle und der Gewebe*—an authority which Mr. Lillie suitably acknowledges. Catalysis and the modification of response to stimuli receive consideration on the same lines; the last 150 pages are devoted to nerve transmission as the special case of vital intercommunication. It is in these chapters that the more original contributions of the author will be found.

Whilst the book is somewhat reiterative, its object a little obscured by a too generous embrace, it contains a mass of material not elsewhere collected in our language in one cover. It can hardly fail to stimulate the zealous biologist to some hard thinking.

R. K. C.

The Evolution and Distribution of Fishes. By J. M. Macfarlane, D.Sc., LL.D. [Pp. vi + 564.] (New York: The Macmillan Co., 1923. Price 25*s*. net.)

The writer of this most interesting book has already given us in *The Causes and Course of Organic Evolution* the foreshadowing of his theories on the evolution and distribution of fishes, which are elaborated in the present volume. The main contention that organisms originated in fresh water is supported by much detailed geological evidence with regard exclusively to fishes, this subject occupying the greater part of the work. Showing as they do the life and environment of the ancient fishes and their relationship with plants and other animals, these chapters are the work of a true naturalist who has for years obviously delighted in following up such a subject and who has himself collected and weighed much of the information on which his views are based.

The author believes in the theory of the origin of fishes by direct descent from the Nemertinea, adding a good deal that is new to the points previously made by other workers. As, however, admittedly no fossil worm belonging to this group has yet been found, the subject is highly speculative, although we have in the nemertine an animal with most of the essential organs from which a simple vertebrate might be derived without too much upsetting of its anatomy. The caution of Hubrecht, who is quoted in chapter iii, is perhaps the safer attitude.

The origin of petroleum, another extremely interesting subject, is accorded a special volume of its own (*Fishes, the Source of Petroleum*, by the same author), but a good deal is included here. The view is taken that fishes, killed suddenly by earthquakes, volcanic eruptions, or catastrophes of a similar nature, are almost wholly responsible for the enormous reservoirs of petroleum occurring in many parts of the world, nearly all of which, up to the Liassic period or later, he affirms to be of fresh-water origin, only those of later date being marine.

Finally, the Tanganyika problem is reviewed, and the opinion given that all the fishes found in the lake are derived, not from marine, but from fresh-water ancestors.

The illustrations given above show only a small part of the scope of this book, which is full of suggestive points tending to stimulate the worker to further investigations.

Marie V. Lebour.

Big Game and Pygmies. By CUTHBERT CHRISTY, M.B., C.M. [Pp. xxxi + 313, with 120 illustrations from photographs and sketches and 1 map.] (London: Macmillan & Co., 1924. Price 21*s*. net.)

THIS book by a scientific naturalist helps to supplement our meagre knowledge about the remarkable fauna of Central Equatorial Africa. Mr. Alexander Barns has already given us two books dealing with the same subject in this region, but the more light thrown on these forest tracts, remarkable as the haunt of wonderful beasts such as the okapi and the great anthropoid apes, the gorilla and chimpanzi, is always welcome, and doubly welcome when such a careful scientific observer as Dr. Christy gives us his varied experiences. The information in this book is chiefly based on his note-books and journals, and his name is sufficient guarantee for the accuracy of the details. All lovers of the grand fauna of Africa will be delighted to learn that the author is of the opinion that the number of elephants is far from diminishing; but, as one would expect, the big tuskers are rapidly being killed. The classification of the elephant is not an easy problem, and he is not in agreement with the late Mr. Lydekker's classification based on the external ears, but until some better one is made this will probably be accepted by most mammalogists. He himself prefers to divide the African elephant into the two types: (*a*) Forest, and (*b*) Bush animals. As these two types live under very different conditions and never intermingle, they remain distinct. The Forest type, according to the author, stands on an average 2 to 3 feet lower at the shoulder than his bigger brother, and has several minor points of difference, such as more and longer hair on the body, more divergent tusks, black and supple skin, and large and rounded ears. No less than five chapters are devoted to the elephant, and all of them are full of interesting details.

The white rhinoceros seems to be in a bad way, according to Dr. Christy, especially in the Sudan; but the reviewer is under the impression that steps have already been taken to preserve this species in future as far as the territories under British protection are concerned. It is, however, to the Belgians we must look for the preservation of the greater number of the more interesting forest fauna with which Dr. Christy's book deals, and from reliable sources one hears of greater restrictions being placed by the Congo authorities on the slaughter (it can be called nothing else) of such rare forest animals as the two species of gorilla and the chimpanzi.

The author devotes four chapters to the African buffaloes, and these, from a zoological standpoint, are probably the most valuable contribution he makes to our knowledge on the fauna of Africa. Taking Lydekker's two main groups, he shows how the first or the Eastern and Southern group is quite distinct and lives under very different conditions from the second, or Western group. Of the latter he describes six distinct local races. With the photographs of the heads and horns of the different types these chapters are a very valuable addition to our scrappy knowledge of the distribution and local variations of the different races of buffaloes.

It is pleasing to hear that the chimpanzi is extremely abundant in the Congo forests, but from the little information available the same cannot be said of the gorilla. It is earnestly hoped that the Belgians will finally put a stop to the senseless slaughter of these anthropoid apes, whether for museums or not. Book after book relating the exploits of travellers and hunters in these Central African forests record the destruction of numbers of these apes, and the restriction, if not the complete stoppage of this slaughter, is long overdue.

Interesting chapters on the Bongo, the greatest of the forest antelopes, the aardvark, the various duikers in which the forest abounds, all contribute largely to our knowledge of these animals. Dr. Christy, if not the first

white man to track and shoot the okapi, certainly was the first Englishman to do so. He gives us a good account of his arduous experiences, and one realises that the okapi, thanks to its protective coloration and the nature of its habitat, is probably safer from extinction than any other of the larger denizens of the forest. The Bambute pygmies, wonderful trackers as they are, armed only with spears, have done little harm during the past ages, and unless armed and taught the use of modern rifles are not likely to affect the numbers appreciably. Chapters on smaller mammals, forest birds and insects, fishes and fish collecting, skinning and preserving specimens with a very interesting account of the Ituri forest and the Bambute pygmies complete a very good book. The photographs and maps are excellent, while a learned introduction by Sir Harry Johnston, the discoverer of the okapi, whom the present reviewer first met in Uganda shortly before his return to England bearing with him his first pieces of okapi skin, enhances the interest of this valuable addition to our knowledge of the fauna of Africa.

R. E. Drake-Brockman.

ANTHROPOLOGY

Primeval Man in Central Europe. By Prof. Dr. P. Goessler. [Pp. 87, with 40 plates.] (Stuttgart: Franck'sche Verlagshandlung.)

This book purports to describe the history of mankind in Central Europe, from the days of the ape-man of Java to those of the Slavs who entered Eastern Germany in the Early Middle Ages. The work consists of a series of plates (with descriptions), each of which illustrates—so far as is possible—the arts and crafts, habitations, burial-places, and skeletal remains of the various races of antiquity. Prof. Goessler is not at his best when dealing with the earliest vestiges of man. In fact, a perusal of the volume under review demonstrates that it adds yet one more to the already long list of books on early prehistoric man, written by people who do not understand the subject with which they deal. After denying that the discovery of the remains of *Pithecanthropus* in Java furnishes any proof of the existence of man in the Tertiary epoch, Prof. Goessler states (p. 8): " No more is this proved by the eoliths, *i.e.* the stones of the dawn of human culture . . . which have been considered by many as purposely made. . . ." And again (p. 8): " But pieces, such as reproduced in Figs. 14 and 15 from the Miocene of Aurillac owe their retouches to natural causes, as, for instance, the transport in the water, the glacial ice, the breakers of the sea." It is difficult to write with becoming calmness of dogmatic and unsupported statements of this kind. It is evident that Prof. Goessler is simply unaware of the later researches carried out in regard to what he lightly terms " eoliths." The time is now past when these specimens can be dismissed, summarily, by the mere assertion that their forms and flaking are due to such nebulous agencies as " transport in the water, the glacial ice, the breakers of the sea." No one is justified in writing upon this important matter who is not familiar with flint fracture, and with the manner in which flint implements are made. And when, on p. 12, we read that the earliest palæoliths are produced " from flint of chalk . . . by knocking off the crust," it becomes clear that it is not possible to regard Prof. Goessler as an authority on early flint implements.

He is, indeed, still in the " dark ages " of prehistory. For sheer archæological nonsense the following statement (p. 20) is, I imagine, almost unique: " From England also comes *Eoanthropus dawsoni*, found in Piltdown (Sussex). The latest investigations make it appear probable that it belongs to the same human race as the man of Galley Hill, i.e. *to that of Aurignac* " (my italics). It would be of interest to know where the results of these " latest investigations " are published, and who is the person responsible

for relegating Piltdown man to Upper Palæolithic times. The description of each plate is given in indifferent English, as well as in French and German, and it is evident that much trouble has been taken over the compilation of this work. The illustrations themselves are moderately good, and the later periods of man's history in Central Europe are treated, fortunately, in a more adequate and reliable manner than are those of early prehistoric times.

J. Reid Moir.

Ancient Hunters and their Modern Representatives. By W. J. Sollas, F.R.S. Third edition. [Pp. xxxvi + 697, with 2 plates and 368 illustrations.] (London: Macmillan & Co. Price 25*s.* net.)

The third edition of *Ancient Hunters* is not—in its main features—greatly different from the two earlier impressions. During the past few years a number of important discoveries relating to prehistoric man have been made in various parts of the world, and Prof. Sollas has done well in gathering together in his popular book some account of this newly acquired knowledge. Thus, we find in this work, an abbreviated description of certain flaked flints—claimed by some investigators as being intentionally shaped—from the very ancient Upper Miocene deposits of Aurillac in France; of the evidence for the existence of Pliocene man in East Anglia, England; of the puzzling and remarkable human skull and bones from the Broken Hill Mine in Rhodesia, South Africa, and of the cavern of Montespan in Southern France, where clay models of animals of Upper Palæolithic age have been found recently by M. Casteret.

For many years past Prof. Sollas has been an active opponent of those who believe that man was present on this earth in the remote Tertiary period, and it is, therefore, of interest to note, in the latest edition of *Ancient Hunters*, that, while many of the old, and, it is necessary to state, fallacious, arguments against this belief are quoted, yet, by means of certain paragraphs in the text, and by footnotes, the impression is conveyed that his attitude to this question has, of late, undergone considerable modification. It is not possible, however, in reading this volume, to decide whether Prof. Sollas accepts the evidence in favour of the existence of Tertiary man,—or whether he does not. And it is doubtful if his uncertain position on the hedge will satisfy either those who believe in the value of this evidence, or those who regard it as unsatisfactory. But, perhaps, in the next edition of *Ancient Hunters*, a definite line will be taken upon this very important and fundamental question of the greater antiquity of man.

Prof. Sollas is a believer in the Early Chellean age of the flint implements found in the Cromer Forest Bed—a deposit he regards as of Pleistocene age—and he does not hesitate to relegate the famous jaw-bone of the Heidelberg man to the same geological horizon and cultural stage. The inter-glacial age of the Lower Palæolithic implements of England is also recognised.

The book contains one or two printer's errors, as, for example, in footnote 2, on p. 643, where "divided" fossil should read "derived" fossil. A graver mistake is, however, made in the photograph of the section in the pit at Foxhall Hall, near Ipswich (Fig. 41), where the uppermost implementiferous level is shown, erroneously, as occurring at the junction of the glacial gravel with the underlying Red Crag, while the lowermost, which is located a few inches above the floor of the pit—and clearly delineated by a line drawn with a trowel—is not indicated. The letter *a* in Fig. 41 should replace the letter *b*, while the latter should be placed at the level of the lower trowel-line in the photograph.

J. Reid Moir.

MEDICINE

An Introduction to the Study of Secretion. By SWALE VINCENT, LL.D., D.Sc., M.D., F.R.S.(Ed.), etc. [Pp. 168, illustrated.] (London: Edward Arnold, 1924. Price 10*s.* 6*d.*)

AMONGST writers and workers on the organs of secretion Prof. Swale Vincent has always been conspicuous by his condemnation of the extravagant claims put forward as to the powers of the "internal" secretions of the body. His attitude is well expressed by the two quotations he puts at the beginning of his book:

"Another error is an impatience of doubting and a blind hurry of asserting without a mature suspension of judgment . . . if we begin with certainties we shall end in doubts; but, if we begin with doubts, and are patient in them, we shall end in certainties."—BACON, *Advancement of Learning.*

"Es ist schwer, genau und fein zu beobachten, aber noch schwerer, aus dem Beobachten nicht mehr zu folgern als es enthält."—EHRENBERG.

Adopting this attitude, he has examined in this book the main facts of secretion in varying organs and organisms in an attempt to ascertain how far they may be regarded in the different cases as being capable of explanation along similar lines, and has produced a very interesting survey of the subject.

The author gives an account of the secretions more usually described, water, digestive juices, urine, milk, tears, secretions of the skin, and of the "internal" secretions. But he also gives, in the part of the book likely to be most attractive to many readers as covering newer ground, accounts of secretions much less usually dealt with, such as the secretion of silk and similar substances as exemplified in the production of spiders' webs, of poisons used by animals for defensive or offensive purposes, of the inky fluid poured out from the ink-sac of some cephalods and of luminous substances.

This gives a touch of freshness and originality to the treatment of this much-discussed subject which is very welcome.

The Endocrine Organs. An Introduction to the Study of Internal Secretion. By SIR E. SHARPEY-SCHAFER, LL.D., D.Sc., M.D., F.R.S. [Pp. ix + 176, with numerous illustrations.] (London: Longmans, Green & Co. Price 15*s.*)

A BOOK by Sir E. Sharpey-Schafer on the Endocrine Organs is sure to be a book worth possessing, and this second edition of the work first published in 1916 (the issue of which was sold out in three years) is most welcome, and has been eagerly waited for.

A great deal of work has been done during the last ten years in connection with these structures, and, though some of it has been unsatisfactory and unscientific, much remains as work that must be considered, and no one is better qualified for its critical examination and sifting than Schafer, himself one of the earliest and most brilliant workers in this field. On account of the increase of material the new edition is in two parts, the first, the one under review, dealing with the thyroid, parathyroids and suprarenals. It is admirable in its arrangement, and in its selection of material, and is of greater use to the student than the earlier edition, not only on account of its enlarged content, but also because it includes references to books and papers which are the sources of the more important facts.

Accounts of the functions of these glands have an immense attraction for the general public and this has led to the appearance of books and articles which have accepted indiscriminately sensational claims for even greater powers than these organs can exercise, great though these are, in the chemical integration of the body. This makes all the more valuable a book, the work of a keen and critical mind, which, though written for the student of medicine

and physiology, presents the problems and functions of these organs in a manner which will make the book of great interest to students of other sciences who are attracted by the story of these wonder-working organs. The demand for Part II of this work is very great, and it is hoped that its appearance will not long be delayed.

The Parathyroid Glands in Relation to Disease. By H. W. C. Vines, M.A., M.D. [Pp. viii + 128.] (London: Edward Arnold, 1924. Price 10*s.* 6*d.*)

Although most of the endocrine organs have been dealt with in books devoted to each particular organ, this is the first time the parathyroid glands have been so honoured. This is perhaps not to be wondered at since these glands were only identified as separate structures in the year 1880, and even recently there were clinicians and surgeons who denied them any activities apart from the thyroid with which they are usually so closely associated anatomically.

The author, after a general survey of the earlier work on the connection between the parathyroids and tetany, deals chiefly with the relations between these organs and calcium metabolism, and gives an account of the beneficial effects of parathyroid therapy in certain cases which he regards as being largely due to disordered metabolism of this element. He claims success for this treatment more particularly in those cases, such as varicose ulcers, and ulcers of the stomach and intestines where the ulceration is, he thinks, due to a sepsis which in its turn is due to, or at any rate easily produced in, the presence of an underlying state of nutritional deficiency marked by a qualitative though not a quantitative change in the calcium content of the blood.

Further observations are no doubt necessary for the complete substantiation of these claims, but it is always interesting to read a book expressing the convictions arrived at by an able and enthusiastic worker in a field to which he has devoted himself, even though the interest may be tempered with criticism.

The Mechanism of the Cochlea: A Restatement of the Resonance Theory of Hearing. By G. Wilkinson, F.R.C.S., and A. Gray, F.R.S.E. [Pp. xx + 253.] (London: Macmillan & Co., 1924. Price 12*s.* 6*d.* net.)

After stating the elementary principles of resonance, the authors discuss the properties peculiar to the cochlea, treated as resonator. Good use is made of Dr. Gray's discovery of progressive increase of bulk and density in the spiral ligament from apex to base, in showing that the basilar fibres are graduated as to tension as well as to length. When they suggest that the fibres are also graduated for load, by two columns of fluid extending from each segment of the basilar membrane to the two "windows," they are treading on more debatable ground, and it is scarcely surprising that in the very intricate and delicate model, made by Mr. Wilkinson, the observed resonances are far from the values calculated from their formula, the dimensions of which, by the way, are not balanced, so that it can be numerically true only for loads of a liquid of unit density. There follow chapters describing other cochlea models, and the nerve mechanism of hearing, in which another good point for the theory is scored with the Principle of Maximum Stimulation. The last chapter—which would have been better first, as it is several times referred to in the body of the work—briefly describes the various theories of hearing.

In spite of errors on the physical side, the book forms a readable first treatise on the subject.

E. G. R.

The Physiology of Muscular Exercise. (Monographs on Physiology.) By the late F. A. Bainbridge, M.A., M.D., D.Sc., F.R.S. Second edition revised by Dr. G. V. Anrep, M.D., D.Sc. [Pp. 226, with 23 diagrams.] (London: Longmans, Green & Co., 1924. Price 10*s.* 6*d.* net.)

It is probable that the day of the authoritative textbook to a whole science is passing or past. It is certain that the many series of monographs on special branches of a particular science which now exist usurp an increasing attention of teacher and student. Where progress outstrips the agility of a single author, only the team represented by the body of able monograph writers can hope to keep the pace.

It is pleasant to be able to welcome unreservedly a new edition of the late Prof. Bainbridge's comprehensive work on the physiology of muscular exercise, and we are grateful that the care of the book has fallen, through the lamented death of the author, into hands so discriminating as those of Dr. G. V. Anrep. It is the modest claim of the editor that he has sought to preserve the general character of the original—in which course we deem him wise—whilst the subject-matter of several sections has been expanded to embrace new work. The expansion has been generous and shrewd. The new material includes that of A. V. Hill, Lupton, and others on the intimate nature of muscle contraction and the verification on man of the experimental investigation of the isolated muscle. The discussion of the circulatory changes in muscular activity is extended by a consideration of Krogh's work on capillaries, and much new material is to be found in those sections dealing with respiration during exercise. The relation of muscular activity to the acid-base equilibrium of the body has been particularly well treated.

Whilst the monograph is of profound academic interest, it is to be emphasised that the original was written from a practical standpoint to assist the physician and all interested in the health of the community. The after-effects of exercise, the effect of high altitudes, of physical training and of fatigue—these questions deserve chapters to themselves.

This able monograph has been ably edited and demands anew the attention of the physiologist and of the hygienist.

R. K. C.

The Technique of Tissue Culture in Vitro. [Pp. 80, illustrated.] (Price 7*s.* 6*d.* net.)

Tissue Culture in Relation to Growth and Differentiation. [Pp. 50, with 4 plates.] (Price 5*s.* net.)

Both by T. S. P. Strangeways. (Cambridge: W. Heffer & Sons, 1924.)

The appearance of a practical manual on tissue culture is very timely. The fascinating labours of Harrison, Carrel, Burrows, and others during the last eighteen years have but served to show the immense possibilities of this new technique which, in its extreme delicacy, will yet suffer much further refinement. At the same time, the progress has been such that the attention of the larger body of physiologists is now imperatively demanded.

The assembly and sterilisation of apparatus, the preparation of the medium, the implantation of the tissue, and the intra-vitam staining of cultures are described with such detail as to satisfy even those wholly unfamiliar with aseptic manipulation. The book does not carry the student far, it being the intention of the author that these preliminary exercises should first be mastered and that the worker should then turn to the original papers for the more elaborate technique. The main disadvantage of this is that the physiologist may fail to realise that tissue culture is feasible on a scale much grander than that defined by the limitations of a cover-slip.

Till he is persuaded that he can handle such material in bulk you are not likely to hold his close attention.

In the second of the books noted above Mr. Strangeways contributes a very careful description of the sequence of changes observed during mitotic division *in vitro*. This process is held to be the immediate reaction of the cell to an environmental stimulus, so that growth may be regarded as an accidental concomitant of mitotic division rather than its predetermining cause. The dedifferentiation of cells of *in vitro* cultures is not fundamental, for the latent potentialities can be revived by suitable means. Since, by means of X-rays, cells are capable of modification, the modified cells continuing to breed their like, and since cells are most susceptible to modification during mitosis, it is suggested that environmental factors incident upon cells during mitosis may be responsible for the phenomena of differentiation. A final chapter discusses tissue culture in relation to inflammation and repair.

R. K. C.

Human Physiology: A Practical Course. By C. G. DOUGLAS, C.M.G., M.C., D.M., F.R.S., and J. G. PRIESTLEY, M.C., D.M. [Pp. ix + 232.] (Oxford: at the Clarendon Press, 1924. Price 12*s.* 6*d.* net.)

THE present century has witnessed a notable development in experimental methods for the investigation of physiological problems in the normal man. With the refinement of chemical and physical technique which has been effected it is possible to set the student to confirm many of the fundamental conceptions of our science working on himself as subject. Clearly this offers the double advantage of ensuring a careful personal interest in technique and of emphasising the interrelation of physiological and pathological data. It is fitting that the high tradition of the Oxford school in this field, under the inspiration of Prof. J. S. Haldane, should be associated with the most complete attempt to teach practical human physiology. This book now offers to teachers and students alike the experience of the honours school at Oxford, and, whilst imitation may make too great a demand on the equipment of some laboratories, there will be few which will not be persuaded to make some use of the experiments so carefully and concisely described.

Respiration and the total respiratory exchange, the analysis of the blood-gases, circulation, and the X-ray examination of the alimentary tract are the subjects most completely dealt with. The chemistry of the blood and urine receive some attention, but we think the authors wise in refraining from an attempt towards completeness in this respect, and instead directing the student to those excellent metabolic studies in Hawk's *Physiological Chemistry*.

Teachers and advanced students will both welcome this publication, and the research worker is likely also to be grateful for a connected account of the technique of the traditional school on respiration.

R. K. C.

Civilisation and the Microbe. By ARTHUR I. KENDALL. [Pp. xviii + 231.] (New York: Houghton Mifflin Company. Price 2.50 $.)

ALTHOUGH Louis Pasteur and his predecessors began the study of bacteria with the investigation of non-parasitic organisms, the early discovery, by Pasteur and Duclaux, of the connection between micro-organisms and disease soon caused this specialised field of bacteriology to develop in a manner out of proportion to the rest of the subject. As a consequence, the study of bacteria began, in its earlier stages, to develop as a science especially applied to medicine. It is not surprising, therefore, to find, in the popular mind, the impression that micro-organisms are the enemies of humanity. More recent advances in the study of bacteria in soil, in dairy products, and

in their connection with industrial processes, illustrates how misguided and one-sided is this still common belief. The author has set out, in a non-technical review of our present knowledge of the activities of micro-organisms, to show his readers something of the benefits which humanity derives from the activity of micro-organisms, and to bring the large number of beneficial or harmless bacteria and the small group of harmful species in their proper focus in the reader's mind.

The book commences with a description of the nature and method of nutrition of bacteria, and the author illustrates, in a graphic manner, the importance of their rapid multiplication and of the ratio of surface area to the mass of a bacterial population. The energy relations of bacterial nutrition and the importance of the source of energy supply are well described. This is a very important subject, usually given too little emphasis in treatises on bacterial activity.

The author deals with the vital importance of the bacteria in the soil, and shows how the activity of the nitrogen-fixing bacteria is vital to the existence of life on the earth. In a book characterised by its good sense of proportion, it is unfortunate that the equally vital processes of cellulose decomposition and the oxidation of organic nitrogen compounds to produce nitrate are scarcely mentioned. The rôle of bacteria in the purification of sewage and in dairy products, and their usefulness in various industrial processes, such as tanning and the setting of flax, are considered. The book finishes with an account of disease-producing bacteria, where the author describes the methods by which bacteria gain entrance to the system, and discusses the means by which the body resists this, and is able to combat the parasite after its entry. The book is clearly written and readable. It fulfils a need in giving the general reader a well-balanced survey of our present knowledge concerning the relation of bacterial activities to human welfare.

H. G. Thornton.

ENGINEERING

Thermodynamique à l'Usage des Ingénieurs. Fourth Edition. By Aimé Witz. [Pp. x + 333, with 32 illustrations.] (Paris: Gauthier-Villars et Cie, 1924. Price 20 frs.)

When several editions of a text appear, it generally denotes that the work is of sound value, and the present volume is no exception to this rule. It aims at setting forth the physical side of thermodynamics in so far as this has a direct bearing on the theory of heat engines, although it does not discuss the practical application to a great extent.

The opening chapter deals with the fundamental laws of thermodynamics, a considerable portion being given to a discussion of various experimental methods of finding Joule's Equivalent. Following this is a discussion of Carnot's cycle, including as a corollary a clear development of the properties of entropy for a closed reversible cycle.

The study of gases is the title of the next section, which covers the standard gas laws. Several worked examples are given here to elucidate the text.

Some useful formulæ are developed under the headings of studies of solids and liquids, matters not usually touched upon in English works on the applications of thermodynamics.

It is noticed that in the chapter on vapours, the classical researches of Callendar are not mentioned, also that the supersaturated state of steam is not discussed, in spite of its importance in nozzle design.

The pages on the flow of gases and vapours are particularly instructive, as from one fundamental equation of flow are developed in turn expressions for the draught in chimneys, flow in nozzles, loss by friction in pipes, etc.

Another chapter gives a critical survey of the various ideal cycles and the

theoretical effects of irreversibility, while the final chapter treats of the properties and nature of energy.

As an introduction to the study of heat engines, the work is to be thoroughly recommended from the points of view of both subject-matter and method of treatment.

B. L.-E.

Theoretical Metallurgy. By R. S. Dean, B.S., M.S., Met.E. [Pp. viii + 246, with 105 illustrations.] (London: Chapman & Hall; New York: John Wiley & Sons, 1924. Price 15*s.* net.)

This book is the outcome of a demand for a second edition of the author's translation of Schenck's *Physical Chemistry of the Metals.* The change of title is due to the wider field covered by extensive rewriting and thorough revision. In its present form in two parts, it is a survey of the modern theories and research which have resulted chiefly from the application of physical chemistry to various branches of metallurgy. The first part, entitled "General Theory of the Metallic State," covers in its three chapters the properties of metals, metallic compounds, and alloys. In the first chapter the structure of the metallic atom, the structure of solid metals, crystal growth, and recrystallisation are among the subjects considered. In the third chapter space is devoted to metallic solutions and binary and ternary alloys. The second part, on "Metallurgical Processes," deals with processes involving electrical and mechanical properties and surface phenomena in one chapter, and with processes involving chemical reaction in three chapters. The chapter on the first-mentioned processes considers subjects such as metals and alloys for special electrical purposes, extrusion, hot pressing, wire drawing, forging, rolling, welding, soldering, and tarnishing and corrosion of metal surfaces. In the succeeding chapters the phase rule is applied to oxidation and reduction; reaction velocity is considered; and the radiation, the adsorption, and the intermediate compound theories of catalysis are discussed. The chapter on blast-furnace processes only deals with some of the reactions in iron smelting, viz. the equilibrium between iron, iron oxide, carbon, carbon monoxide, and carbon dioxide. The final chapter is on the reactions of sulphides, phosphides, silicides, and arsenides.

From what has been said it will be seen that there are branches of theoretical metallurgy which are not considered and that the new title does not exactly indicate the scope of the work. In the past metallurgy has been defined, either as the art of extracting metals from their ores, or as the science of smelting. Dr. Percy regarded it as an art, but stated that the knowledge of its principles was the science, founded on the sciences of chemistry, physics, and mechanics. During the past thirty years, with the increasing knowledge of the structure of metals, the development of new processes, and the attention to problems of corrosion, this foundation has become broader and now includes modern sciences such as colloid and physical chemistry, and electrochemistry. Consequently a work entitled *Theoretical Metallurgy,* dealing with the theoretical considerations of a subject so extensive as metallurgy, should be comprehensive, otherwise its title does not convey an exact meaning. The book under review does not claim to be comprehensive: it aims at being suggestive; therefore it is an introduction to the study of some modern metallurgical theories, and as such is a useful contribution to the literature of metallurgy. In its character it is different from most metallurgical text-books, and should awaken interest in the study of theoretical metallurgy, for various views extant on the subjects treated are indicated, and also there are numerous references which allow fuller information to be gained. The book, which is well printed and clearly illustrated, can be recommended to students and others interested in this aspect of the subject.

E. Courtman.

An Introduction to the Strowger System of Automatic Telephony. By H. H. HARRISON, M.I.E.E., M.I.E.S.E. [Pp. vii + 146, with 161 diagrams.] (London: Longmans, Green & Co., 1924. Price 7*s.* 6*d.* net.)

INTENDED as a class textbook on Automatic Telephony and published at a low price, a very adequate treatment is nevertheless presented.

The author wisely limits the treatment to one system only—THE STROWGER; with this choice there can be no quarrel, combining as it does excellence of mechanical design with the usual relays and magnetic devices common to all automatic telephone apparatus.

Commencing with elementary requirements, a system is evolved which satisfies them and the Strowger solution described. The immediate limits and difficulties of further development are then considered and the necessary improvements in design indicated. In this way the student is led step by step to a clear appreciation of the ultimate mechanism and the many and varied demands which it has been designed to meet.

Clear sketches illustrate the purely mechanical features down to the smallest detail, while the electrical arrangements are shown by schematic diagrams. These in all cases are as simple as possible, conventional symbols being introduced when the diagrams become complex.

The student is assumed familiar with the theory and operation of manual telephone systems. This largely covers the field of automatic working, and the theoretical treatment is therefore limited to such points as interpretation of traffic data, load curves, busy hour calls, etc., and their influence on the efficiency and design of a system. A general résumé of the problems of supervision, fault-finding and elimination, and the inter-connection of automatic and manual systems completes an excellent little volume which may be recommended with confidence.

A. N. JACKSON.

MISCELLANEOUS

An Introduction to the Study of Colour Vision. By Sir J. HERBERT PARSONS, C.B.E., D.Sc., F.R.C.S., F.R.S. Second edition. [Pp. x + 323.] (Cambridge: at the University Press, 1924. Price 25*s.* net.)

THE first edition of this book appeared in 1914, and was reviewed in SCIENCE PROGRESS, p. 177, vol. x, 1915. The present edition contains about a dozen additional pages and twenty additional diagrams, also references to new work which it was impossible to notice in the text. Allen's work on the effect of fatigue with spectral colours on the persistency curve very properly receives due notice, but the very important paper by Hecht and Williams in the *Jour. of Gen. Physiol.*, vol. v, p. 1, 1922, is not mentioned, possibly on account of its being too recent. This paper makes it plausible that the photochemical decomposition of visual purple is responsible for vision both at low and high intensities.

The subject of colour vision is of interest to physicists, physiologists, psychologists, and statisticians, who approach it from widely different standpoints, and it is not easy for workers to appreciate or keep touch with what is being done in all the fields. Sir J. Herbert Parsons' book will help to meet this difficulty in the future as in the past, and its bibliography will be of great value. The treatment is exhaustive, and the production of the book is every way worthy of the Cambridge University Press.

The field, as is well known, has been a very controversial one. The present reviewer has recently completed a survey of the colour vision of more than 2,000 students, and as a result is of the firm opinion that the division of the colour-blind into protanopes and deuteranopes is useless

and misleading, also that the colour-blind are generally trichromatic, that trichromasy passes into monochromasy directly and not through dichromasy as an intermediate stage, and that there is little more to be gained by statistical investigation or recording particular cases. Much harm has been done in the past by omitting the description of cases that do not fit in with particular theories. The chief desideratum at present is to get a model of the mechanism of vision. Sir J. Herbert Parsons states, p. 292: "As is usually the case, the three components theory becomes less plausible, the more concrete form it takes." The way of progress at present seems to lie in imagining concrete forms and testing them, and here physics and chemistry and physiology may be of great service to one another. The work of Lucas and Adrian on the nervous impulse, the passage of the impulse from the one neurone to the other, the recent advances in photo-electricity, all have bearings on the subject which have not yet been worked out, and it is possible that there are discoveries awaiting us that may transform the situation. We have successfully traced the light-wave from its incidence on the outside surface of the eye to the rods and cones, and it is not too much to hope that we may trace the impulse from the cone to the optic nerve in an equally definite manner.

One of the chief problems of modern physics is to discover the connection between the light-wave and the quantum. The eye is so sensitive and has so many characteristic phenomena, that the solution may come from colour vision and not by way of the spectroscope or electroscope. It is consequently gratifying that so great an interest is being taken at present in colour vision, an interest to which the new edition of the book will certainly add.

R. A. HOUSTOUN.

Spinoza, Descartes, and Maimonides. By LEON ROTH, M.A., D.Phil. [Pp. 148.] (Oxford: at the Clarendon Press, 1924. Price 7s. 6d. net.)

IT has long been the fashion among German philosophers and historians of philosophy to regard Spinoza's philosophy as merely the logical development of the philosophy of Descartes. The fashion was set early, already by Leibniz, and reached its culmination in Kuno Fischer. In the earlier editions of Kuno Fischer's monumental *History of Modern Philosophy*, Spinoza, although he is treated with the greatest sympathy and admiration, and actually receives more space than Descartes, is nevertheless treated as one of the Cartesian School. The method adopted was simple enough. First Cartesianism was interpreted in the light of subsequent Spinozism, and then it was easy enough to evolve Spinozism out of Cartesianism. Some indeed, like Dr. M. Joel, Sir Frederick Pollock, and others, rejected this view of the relationship of Spinoza to Descartes, but to little purpose. The fashion had on its side the powerful influence of the Hegelian tendency to see in the history of human thought one continuous stream flowing through the channels of a few select minds.

In the volume under review Dr. Roth examines anew the problem of the relationship of Spinoza to Descartes. He shows that the logical consequence of Cartesianism is really a pluralistic scepticism, that such was actually Spinoza's view of Cartesianism, and that it was precisely *in opposition* to this type of thought that Spinoza evolved his pantheistic philosophy. This view of the matter certainly harmonises most with the impression one gets from the study of the life and thought of Spinoza. In his earliest writings already Spinoza's attitude to Cartesianism is one of antagonism; and the Cartesians were among the persecutors of Spinoza. Dr. Roth, moreover, does not stop at this negative result. Following the views of Dr. Joel and others, he establishes a close kinship between Spinoza and Maimonides, the Jewish philosopher of the twelfth century, whose *magnum opus* (*The Guide*

of the Perplexed) Spinoza is known to have possessed and studied. Dr. Roth not only brings out many similarities of thought and expression between Spinoza and Maimonides, but he shows how the opposition between the philosophy of Spinoza and that of Descartes was in its essentials the repetition, at another level so to say, of the opposition between the philosophy of Maimonides and that of the Arab Dialecticians of the twelfth century.

Of late there has been a considerable revival of interest in the philosophy of Spinoza, especially in France, Germany, and Holland. It is a pleasure to welcome Dr. Roth's excellent little book as a valuable English contribution to the correct appreciation of one of the greatest thinkers of all peoples and of all ages.

A. Wolf.

The Expert Witness. By C. Ainsworth Mitchell, M.A. [Pp. xv + 188, with 23 plates.] (Cambridge: W. Heffer & Sons, 1923. Price 7*s*. 6*d*. net.)

This book is sadly misnamed, as its title gives little indication to those who do not know the author of the feast of fascinating information it contains. The sub-title, "The Application of Science and of Art to Human Identification, Criminal Investigation, Civil Actions, and History," is more illuminating, but still hides the fact that the book is not a dry text-book but an account of the application of scientific methods and knowledge that is more exciting than most detective stories. It is a page in the romance of science.

Among the subjects dealt with is evidence of identity, the Beck case, and the more remarkable one of two negroes, both named Will West, so extraordinarily alike that one claimed the photograph of the other as his own portrait; their Bertillon measurements were practically the same, but there was no difficulty in distinguishing them by their finger-prints. Scientific evidence in poison trials is discussed and the ingenious methods for detecting the falsification of documents are described with many photographs. Some account is given of the use of X-rays in the examination of paintings and a chapter is devoted to the use of modern scientific methods in the elucidation of history. The author, from an examination of the notorious "Scandal Letter" written by Mary Queen of Scots to Elizabeth, is convinced that it is in Mary's handwriting and makes interesting comments on the results of his examination of the "Casket Letters." One must regret, however, that the author has no remarks on the relation between newspaper reports of scientific evidence and what was actually said.

The book is written by an acknowledged authority in a very readable style, with many flashes of humour not infrequently directed at the non-scientific man in authority. I should make a wide appeal, and as proof of its interest the reviewer can confess to being unable to put it down until, in the small hours, he had read to the last page.

O. L. B.

of Science and Invention. By Floyd L. Darrow. [Pp. vi + 350.] (London: Chapman & Hall. Price 10*s*. 6*d*. net.)

This is a book of the frankly "popular type." Very wisely, it presents the story of science in biographical form, and taken in tabloids—an occasional chapter after meals as required—reads very interestingly and will give pleasure. It is when an attempt is made at a serious reading that the book, in our view, fails. The preface tells us frankly why this must be so. "Each chapter is an essay in itself, and, while the chapters follow in general the chronological sequence of events, they may be read in any order." It is surely impossible to expect an ordered scheme in such a work, taken as

a whole, and so we find chemists, founders of the textile industry, and astronomers rubbing shoulders, whilst between two pioneers of medicine and Joseph Henry are sandwiched Louis Daguerre, of Daguerreotype fame, and Alvan G. Clark, the maker of refracting telescopes.

The book is well produced and is liberally interspersed with photographs. There is also an index. The last chapter is headed "Briefer Biographies," and somewhat spoils the effect; for it arouses our curiosity as to why such names as Ampère, Archimedes, Aristotle, Arrhenius, and Boyle (to quote the first few) should be allotted a few lines only as compared with many a little less worthy who are allotted several pages apiece. One realises, of course, that the author could not, and should not, include everybody, but surely it would have been better to have omitted this last chapter altogether.

I. B. H.

La Méthode Générale des Sciences Pures et Appliquées. By ANDRÉ LAMOUCHE, Ingénieur Principal de la Marine. [Pp. xii + 298.] (Paris: Gauthier-Villars et Cie. Price 30 frs.)

THIS book is intended by its author, a professor in the French Naval Academy, as a call to the teachers of France to study science from a practical as well as a philosophical aspect, to avoid splitting it into water-tight compartments, and, above all, to follow American methods in education both general and technical. In respect of method of teaching pure science in the United States, M. Lamouche might with great advantage consult Sir J. J. Thomson. A great part of the introduction bears considerable resemblance to an account in the *Quotidien*, a few months ago, of a pressman's interview with the new Minister of Education. The similarity sufficiently indicates its tone and meaning. An English parallel to Lamouche's projected system of scientific education would be to combine the reading of Norman Campbell's *Elements* with a study of elementary mechanics and a course of workshop practice.

M. Lamouche has evidently read a great deal about his subject, notably the works of Mach, Poincaré, and Bergson; but many of his quotations are of "Ingénieur en Chef Marbec," and of Americans whose names have crossed the Ocean without crossing the Channel. The book was evidently a labour of love, the writing of it having given great pleasure to the author. This pleasure will probably be his only reward.

J. H. S.

Industrial History. By HARRY B. SMITH. [Pp. xii + 305.] (New York: The Macmillan Co., 1923. Price 7s. net.)

THIS little book is chiefly of interest to the student of science as an approach to the sort of work which has yet to appear, but for which there is considerable need. Many works appear at frequent intervals giving popular account of industry and invention. Yet other books frequently appear, more technical in character, on the applied sciences. Between these two types there is a gap which should surely be filled by a work surveying from the scientific standpoint the interrelationship between the history of scientific thought and the progress of organised industry. The present work under review would probably have conformed to this type were the author a student of science. The work is, however, clearly written by a student of sociology, and so treads the ground from a point of view which must not be ignored. In an elementary sort of way he does his work well, and the story of Industrial History makes interesting reading in consequence. The work is liberally illustrated and well produced and can safely be recommended.

I. B. H.

In Brightest Africa. By CARL E. AKELEY. [Pp. 267, with 68 illustrations from photographs and line drawings.] (London: William Heinemann, 1924. Price 21*s.* net.)

THE author of this book is a taxidermist of note, but, unlike any other taxidermist known to us, he is a good field naturalist, and a skilled hunter who makes a study of the animals he sets up. He is inclined to take rather a pessimistic view of the future of the fauna of Africa, but this is perhaps natural and excusable in one who has hunted big game prior to the cutting-up of the most favoured regions of East and Central Africa into private estates. With the careful preservation of the various species, however, there should be little danger of their extinction by man, the greatest enemy of the wild beasts. The chapters on Gorilla-hunting will be read by most people with sorrow. For the benefit of those so-called sportsmen who wish to go gorilla-hunting I need only quote three extracts. On page 217 the author writes, " As he ran about, one of the guides speared him. I came up before he was dead. There was a heart-breaking expression of piteous pleading on his face. He would have come to my arms for comfort."

On page 223, after the author had shot a female with young, he says, " This female had a baby which was hustled off by the rest of the band. The baby was crying piteously as it went."

Again, on page 230 he tells us, " Like all of the others, he displayed no signs of aggressiveness. He was intent only on getting away. He had not made a single sound at any time. As he lay at the base of the tree, it took all one's scientific ardour to keep from feeling like a murderer."

Those of us who saw the delightful timid creature, the gorilla of Sloane Street, at the window of his owner's house a few years ago, can well imagine the " sport " enjoyed in the killing of these large apes. How much longer this disgusting business is to be allowed to be carried on by civilised man remains to be seen. At all events, while the Belgians permit the slaughter to be continued, collectors would be well advised to say as little as possible regarding their experiences while killing these shy, timid, and to all intents and purposes inoffensive apes. In extenuation of the slaughter of these apes by the author one is bound to admit that not only their skins but their skeletons have been carefully preserved for scientific purposes by an experienced taxidermist. Furthermore, Mr. Akeley's observations on the habits of the gorilla are of real scientific value. His suggestion that the country surrounding and including the three peaks of Mikeno, Karisimbi, and Visoke should be turned into a gorilla reserve is an excellent one, and it is hoped that the Belgian authorities will take note of it, and act without further delay. (See page 492, above.)

The author seems to have had some trying experiences during his hunting career, not the least of which was his encounter with a leopard. It is invariably the wounded lion or leopard that attacks man. Apart from man-eaters, which are usually old lions that find old age a handicap to their obtaining their food on the plains, the reviewer cannot recall a single instance of an unwounded lion or leopard attacking the hunter unless it was cornered and saw no chance of escape.

The photographs which illustrate this book are all excellent, as one would expect from such a keen and experienced photographer, who has himself invented an ingenious motion-picture camera for photographing wild-animal life.

There appears to be a mistake in describing the photograph of a spearman facing page 62 as a Nandi, as he is obviously a Masai el-moran (warrior), while Mr. Akeley's band of spearmen facing page 70 are all Masai warriors.

Future generations will be grateful to Mr. Akeley for his realistic animal groups and his life-like bronzes in the Roosevelt African Hall of the American Museum.

R. E. DRAKE-BROCKMAN.

A History of Magic and Experimental Science during the first Thirteen Centuries of our Era. By LYNN THORNDIKE, Ph.D. In two volumes. Vol. I. [Pp. viii + 835.] [Vol. II. Pp. vi + 1036.] (London: Macmillan & Co. Price 42*s*. net.)

THE distinguished author of this monumental work is a Professor of History in the Western Reserve University, Ohio, and is one of the pioneers of the recently founded History of Science Society of America. Nearly twenty years ago Prof. Thorndike published a thesis which marked a starting-point for this present work, and now students of mediævalism have available to them an authoritative and exhaustive treatise on Mediæval Folk Lore and Magic which takes an easy first place in the literature of its kind. The title of the work is somewhat perplexing—almost misleading. A title is usually intended as an indication of the contents, but there is little in the volumes under review to justify the linking up of Experimental Science and Magic in the title. A reader who opens the book with the legitimate purpose and expectation, aroused by the title, of studying the history of experimental science in the early days is foredoomed to disappointment. There is no such history. There is not even a consistent discussion of the interrelationship between the philosophy of magic, with its pseudo-sciences of astrology and alchemy, and the pursuit of experimental science—a relationship not entirely one of impedance of the latter by the former. It might be argued that there was in fact little experimental science from the first to the thirteenth centuries. We cannot wholly assent to this view, but in any case why "feature" experimental science in the title?

We venture to advise would-be readers on a course considered not unusual to the sentimental reader of the sentimental novel—that of first reading the preface at the commencement of vol. i, and then skipping, temporarily of course, some 1,800 pages in order to read chapter 72, the conclusion, at the end of vol. ii. We believe that thereby a happy ending may be assured and at least the reader will appreciate the real scope of this very remarkable book. He will realise why it pertinently opens at the commencement of the Roman period with Pliny as the immediate heir to the legacy of Greece, and ends at the close of the first quarter of the fourteenth century, by which time the period of the mediæval revival of learning seems to have spent its force. He will learn, too, that Prof. Thorndike's thesis is that the period of his study was not one of retrogression, but was of a steady holding-on to the Greek tradition, amplified by discussion and criticism that were continually helpful and fresh. The work is built up in five books. The first deals with the period of the Roman Empire, the second with Early Christian Thought, and the third with the Early Middle Ages. These three form vol. i. Vol. ii comprises Book IV on the twelfth century and Book V on the thirteenth century. The author throughout gives freely of his wide reading and his scholarship, and almost every page of the work is liberally interspersed with detailed references. If the treatment is thorough the style is easy and pleasant. Exhaustive general, biographical and manuscript indexes at the end of each volume add the final note to a publication for which all students of the history of man's mental evolution in general, and of mediævalism in particular, will be profoundly indebted.

I. B. H.

Leaves from "The Golden Bough." Culled by LADY FRAZER. [Pp. xii + 249, with 16 illustrations.] (London: Macmillan & Co., 1924. Price 10*s*. 6*d*. net.)

LADY Frazer has given us an excellent series of abstracts from Sir James Frazer's great *Golden Bough*. Her book, in fact, presents a number of wise or pretty fairy-tales beautifully illustrated by Mr. H. M. Brock. It is curious

how much finer the Greek stories are than most of the others, some of which are so diffuse and disconnected as to fail in presenting that essential point in all fairy-tales, the moral. The Greek fairy-tales belong to another world—just as ancient Greek literature and art did, compared with the more barbaric attempts of African and Asiatic peoples.

Scientific Research and Human Welfare. By FRANKLIN STEWART HARRIS, Ph.D., President, Brigham Young University. With the Collaboration of NEWBERN I. BUTT, B.S. [Pp. ix + 406.] (New York: The Macmillan Company, 1924. Price 12*s*. net.)

THE book deals in separate chapters with Health, Communication, Transportation, Illumination, Agriculture, Engineering and Mining, Manufacturing, and the Home. It is well written; though of course a very small proportion of the history of science can be included in 406 pages. We doubt whether historians of science are likely to admit all the names mentioned in the book, and on many occasions the authors make the common mistake of including old writers simply because they happen to have conjectured something which was afterwards proved sound by the long-continued labours of the true discoverers. Very few of those who teach or even write on science seem to understand that science deals with proof and not with mere conjecture. The work may be useful for school children and those who are not making a special study of science, but it contains some facts about applied science which many scientific workers may be glad to remember.

BOOKS RECEIVED

(Publishers are requested to notify prices)

An Introduction to the Mathematical Analysis of Statics. By C. H. Forsyth, Assistant Professor of Mathematics, Dartmouth College. New York: John Wiley & Sons; London: Chapman & Hall, 1924. (Pp. viii + 241.) Price 11*s*. 6*d*. net.

Industrial Physics: Heat. By L. Raymond Smith, Instructor in Industrial Physics, Wm. L. Dickinson High School, Jersey City, N.J. New York and London: McGraw-Hill Book Company, 1924. (Pp. xi + 282, with 274 figures.) Price 10*s*. net.

The Structure of Crystals. By Ralph W. G. Wyckoff, American Chemical Society Monograph Series. New York: The Chemical Catalog Company, 19 East 24th Street, 1924. (Pp. 462, with 213 figures.) Price 6 dollars net.

Helmholz's Treatise on Physiological Optics. Translated from the Third German Edition. Edited by James P. C. Southall, Professor of Physics in Columbia University. Volume I. Published by the Optical Society of America, 1924. (Pp. xxi + 482, with 149 figures.)

College Manual of Optics. By Lloyd William Taylor, Instructor in Physics in the University of Chicago. New York and London: Ginn & Co. (Pp. ix + 236, with 106 figures.) Price 12*s*. 6*d*. net.

Experimental Science: I, Physics. By S. E. Brown, M.A., B.A., B.Sc., Headmaster of Liverpool Collegiate School. Section VI, Sound. Cambridge: at the University Press, 1924. (Pp. 420–530.) Price 3*s*. 6*d*. net.

Isotopes. By F. W. Aston, Sc.D., D.Sc., F.I.C., F.R.S., Fellow of Trinity College, Cambridge. Second Edition. London: Edward Arnold & Co., 1924. (Pp. xi + 182.) Price 10*s*. 6*d*. net.

L'Origine Tourbillonnaire de l'Atome et ses Conséquences. Par Jean Varin d'Ainvelle. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1924. (Pp. 215.) Price 20 fcs.

Elementary Physics for Medical, First Year University Science Students and General Use in Schools. By G. Stead, M.A., Reader in Physics, Guy's Hospital Medical School. London: J. & A. Churchill, 7 Great Marlborough Street, 1924. (Pp. iv + 453, with 288 illustrations.) Price 10*s*. 6*d*. net.

Ions, Electrons, and Ionising Radiations. By James Arnold Crowther, Sc.D., F.Inst.P., Professor of Physics at University College, Reading. Fourth Edition. London: Edward Arnold & Co., 1924. (Pp. xii + 328.) Price 12*s*. 6*d*. net.

John William Strutt, Third Baron Rayleigh, O.M., F.R.S., sometime President of the Royal Society and Chancellor of the University of Cambridge. By his Son, Robert John Strutt, Fourth Baron Rayleigh, F.R.S., late Fellow of Trinity College, Cambridge. London: Edward Arnold & Co., 1924. (Pp. xi + 403, with 5 plates.) Price 25*s*. net.

Space and Time. An Experimental Physicist's Conception of these Ideas and of their Alteration. By Carl Benedicks, formerly Professor of Physics at the University of Stockholm. With an Introduction by Sir Oliver Lodge, F.R.S. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xiv + 98.) Price 4*s*. net.

The Measurement of Fluid Velocity and Pressure. By J. R. Pannell, F.R.Ae.S., A.M.I.Mech.E., Senior Assistant in the Aerodynamics Department of the National Physical Laboratory. Edited by R. A. Frazer, B.A., B.Sc., A.F.R.Ae.S., Assistant in the Aerodynamics Department of the National Physical Laboratory. London: Edward Arnold & Co., 1924. (Pp. vii + 135.) Price 10*s*. 6*d*. net.

The Electron, its Isolation and Measurement, and the Determination of some of its Properties. By Robert Andrews Millikan, Director, Norman Bridge Laboratory of Physics, California Institute of Technology. Chicago: The University of Chicago Press. (Pp. xiv + 293, with 40 text-figures.) Price $1.75 net.

Crystals and the Fine-Structure of Matter. By Friedrich Rinne, Professor of Mineralogy in the University of Leipzig. Translated by Walter S. Stiles. London: Methuen & Co., 36 Essex Street, W.C. (Pp. ix + 195, with a drawing by A. Dürer and portraits of the leading investigators in the study of Fine-Structure and 202 figures.) Price 10*s*. net.

The Theory of Relativity. By L. Silberstein, Ph.D., Mathematical Physicist, Eastman Research Laboratory, Rochester, N.Y. Second Edition, enlarged. London: Macmillan & Co., St. Martin's Street, 1924. (Pp. x + 563.) Price 25*s*. net.

The Theory and Application of Colloidal Behaviour. Contributed by the foremost Authorities in each division of the Subject. By Robert Herman Bogue, Ph.D., Director of Research for the Portland Cement Association, formerly Associate Professor of Chemistry at Lafayette College. Volume I: The Theory of Colloidal Behaviour. Volume II: The Application of Colloidal Behaviour. New York and London: McGraw-Hill Book Company, 1924. (Pp. xi + 829.) Price 40*s*. net.

Analytical Chemistry. Based on the text of F. P. Treadwell, late Professor of Analytical Chemistry of the Polytechnic Institute of Zurich. Translated, enlarged, and revised. By William T. Hall. Volume II: Quantitative Analysis. Sixth Edition. New York: John Wiley & Sons; London: Chapman & Hall, 1924. (Pp. xiii + 811.) Price 25*s*. net.

Charts of the Chemical Reactions of the Common Elements. By John A. Timms, Ph.D., Instructor in Chemistry, Yale University. New York: John Wiley & Sons; London: Chapman & Hall, 1924. (Pp. ix + 81.) Price 10*s*. net.

The *Chemical Age* Chemical Dictionary: Chemical Terms. London: Ernest Benn, 8 Bouverie Street, E.C.4, 1924. (Pp. 158.) Price 16*s*. net.

The Principles of Applied Electrochemistry. By A. J. Allmand, M.C., D.Sc., F.I.C., Professor of Chemistry, King's College, London. Second Edition, revised and enlarged by the Author and H. J. T. Ellingham, A.R.C.S., B.Sc., A.M.I.Chem.E. London: Edward Arnold & Co., 1924. (Pp. xi + 727.) Price 35*s*. net.

Distillation du Bois. Par G. Dypont, Professeur à la Faculté des Sciences de Bordeaux. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1924. (Pp. xv + 284.) Price 25 fcs.

Thermodynamique à l'Usage des Ingénieurs. Par Aimé Witz. Quatrième Edition. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1924. (Pp. x + 334.) Price 20 fcs.

Outlines of Organic Chemistry. By E. J. Holmyard, M.A., F.I.C., Head of the Science Department, Clifton College. London: Edward Arnold & Co., 1924. (Pp. xi + 467, with 6 plates.) Price 7*s.* 6*d.* net.

A System of Physical Chemistry. By William C. McC. Lewis, M.A., D.Sc., F.Inst.P., Brunner Professor of Physical Chemistry in the University of Liverpool. Third Edition. In three volumes. Volume III: Quantum Theory. With certain Appendices by James Rice, M.A., A. McKeown, M.Sc., and R. O. Griffith, M.Sc. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1924. (Pp. x + 407.) Price 15*s.* net.

The Corrosion of Metals, By Ulick R. Evans, M.A., King's College, Cambridge. London: Edward Arnold & Co., 1924. (Pp. xi + 212.) Price 14*s.* net.

Thermodynamique. By J. A. Ewing, Principal et Vice-Chancelier de l'Université d'Edimbourg. Traduction par M. R. Duchêne. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1924. (Pp. 488.) Price 50*s.* fcs.

Elementary Qualitative Analysis for College Students. By J. B. Reedy, Assistant Professor of Chemistry in the University of Illinois. London: McGraw-Hill Book Company, 6 Bouverie Street, E.C.4, 1924. (Pp. xiv + 137.) Price 7*s.* 6*d.* net.

The Constituents of Coal Tar. By Percy Edwin Spielmann, Ph.D., B.Sc., F.I.C., A.Inst.P., A.R.C.Sc. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1924. (Pp. xii + 219.) Price 12*s.* 6*d.* net.

Practical Organic Chemistry. By Julius B. Cohen, Ph.D., B.Sc., F.R.S., Professor of Organic Chemistry, University, Leeds. London: Macmillan & Co., St. Martin's Street, 1924. (Pp. xv + 520.) Price 6*s.* 6*d.* net.

A Comprehensive Treatise on Inorganic and Theoretical Chemistry. By J. W. Mellor, D.Sc. Volume V. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1924. (Pp. x + 1004, with 206 diagrams.) Price £3 3*s.* net.

A Laboratory Manual in General Chemistry. By William Foster, Professor of Chemistry in Princeton University. Princeton: Princeton University Press, 1924. (Pp. ix + 205.) Price 10*s.* net.

Physical Chemistry for Students of Medicine. By Alexander Findlay, M.A., D.Sc., F.I.C., Professor of Chemistry, University of Aberdeen. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1924. (Pp. ix + 227, with 39 figures.) Price 8*s.* 6*d.* net.

The First One Hundred Years of American Geology. By George P. Merrill, Head Curator of Geology, United States National Museum. New Haven: Yale University Press; London: Oxford University Press, 1924. (Pp. xxi + 773, with 130 figures.) Price 27*s.* 6*d.* net.

The Transpiration Stream. By Henry H. Dixon, D.Sc., F.R.S., Professor of Botany in the University of Dublin. Being a course of three Lectures delivered, on the invitation of the Senate, before the University of London in January 1924. London: University of London Press, 17 Warwick Square, E.C.4, 1924. (Pp. 80.) Price 2*s.* 6*d.* net.

Researches on Fungi. Volume III. The Production and Liberation of Spores in Hymenomycetes and Uredinæ. By A. H. Reginald Buller, B.Sc., D.Sc., Ph.D., LL.D., F.R.S.C. London: Longmans, Green & Co., 39, Paternoster Row, E.C.4, 1924. (Pp. xii + 611, with 227 text-figures.) Price 30*s.* net.

Plant Anatomy from the Standpoint of the Development and Functions of the Tissues, and Handbook of the Micro-Technic. By William Chase Stevens, Professor of Botany in the University of Kansas. London: J. & A. Churchill, 7 Great Marlborough Street, 1924. (Pp. xv + 398, with 135 figures.) Price 18*s*. net.

A Textbook of General Botany. By Gilbert M. Smith, James B. Overton, Edward M. Gilbert, Rollin H. Denniston, George S. Bryan, and Charles E. Allen, of the Department of Botany of the University of Wisconsin. New York: The Macmillan Company, 1924. (Pp. x + 409 with 321 figures.) Price 16*s*. net.

Marine Structures: Their Deterioration and Preservation. By William G. Atwood and A. A. Johnson, with the collaboration of William F. Clapp, Robert C. Miller, H. W. Walker, H. S. McQuaid, and Marjorie S. Allen. Washington, D.C. Published by the National Research Council. (Pp. viii + 534, with 168 figures and several maps.)

The Study of Living Things. Prolegomena to a Functional Biology. By E. S. Russell, M.A., D.Sc., F.Z.S. (Pp. xx + 139.) Price 5*s*. net.

Studies in Human Biology. By Raymond Pearl, Professor of Biometry and Vital Statistics in the School of Hygiene and Public Health, and of Biology in the Medical School, the Johns Hopkins University. Baltimore: Williams & Wilkins Company, 1924. (Pp. 653.)

Genetics and Eugenics. A Textbook for Students of Biology and a Reference Book for Animal and Plant Breeders. By W. E. Castle. Professor of Zoology in Harvard University, Cambridge, U.S.A. Harvard University Press; London: Oxford University Press, 1924. (Pp. viii + 434.) Price 12*s*. 6*d*. net.

The Chemistry of Enzyme Actions. Second and Revised Edition. By K. George Falk, Harriman Research Laboratory, The Roosevelt Hospital, New York. New York: The Chemical Catalogue Company, 19 East 24th Street, 1924. (Pp. 249.) Price $3.50.

An Introduction to the Study of Recent Corals. By Sydney J. Hickson, Professor of Zoology in the University of Manchester, Manchester. At the University Press; London: Longmans, Green & Co. 1924. (Pp. xiv+ 257, with 40 figures.) Price 25*s*. net.

In Southern Seas. Wanderings of a Naturalist. By W. Ramsay Smith, M.D., D.Sc., F.R.S. London: John Murray, Albemarle Street, W. (Pp. xviii + 297, with 30 illustrations and 2 maps.) Price 16*s*. net.

An Inquiry into the Causes and Effects of the Variolæ Vaccinæ. By Edward Jenner. Facsimile of the First Edition. London, 1798. With 14 hand-coloured plates. Milan: R. Lier & Co., Via Brera 7, 1923. (Pp. 75.) Price 12*s*. 6*d*. net.

Textbook of Pathology. By Robert Muir, M.A., M.D., Sc.D., F.R.S., Professor of Pathology, University of Glasgow. London: Edward Arnold & Co., 1924. (Pp. vii + 774, with 433 figures.) Price 35*s*. net.

A Pioneer of Public Health, William Thompson Sedgwick. By E. O. Jordan, G. C. Whipple, and C. A. Winslow. With an Introduction by Mary K. Sedgwick. New Haven: Yale University Press; London: Oxford University Press, 1924. (Pp. xvi + 193.) Price 18*s*. 6*d*. net.

The Internal Secretions of the Sex Glands. By Alexander Lipschutz, M.D., with a Preface by F. H. A. Marshall, F.R.S. Cambridge: W. Heffer & Sons; Baltimore, U.S.A.: Williams & Wilkins Company. (Pp. xviii + 512, with 140 illustrations in the text.) Price 21*s*. net.

The Extra Pharmacopœia of Martindale and Westcott. Revised by W. Harrison Martindale, Ph.D., F.C.S., and W. Wynn Westcott, M.B., D.P.H. Eighteenth Edition. In Two Volumes. Vol. I. London: H. K. Lewis & Co., 1924. (Pp. xxxviii + 1163.) Price 27*s.* 6*d.* net.

Essays on the History of Medicine. Presented to Karl Sudhoff on the Occasion of his Seventeenth Birthday, November 26, 1923. By Sir T. Clifford Allbutt and 18 others. Edited by Charles Singer and Henry E. Sigerist, 1924. London: Oxford University Press; Zurich: Verlag Seldwyla. (Pp. vi + 418, with 35 illustrations.) Price 42*s.* net.

A Textbook of Physiology. By H. E. Roaf, M.D., D.Sc., M.R.C.S., L.R.C.P., Professor of Physiology at the London Hospital Medical College, University of London. London: Edward Arnold & Co., 1924. (Pp. viii + 605, with 318 figures.) Price 25*s.* net.

The Erythrocite and the Action of Simple Hæmolysins. By Eric Ponder, Lecturer in Physiology in the University of Edinburgh. Edinburgh: Oliver and Boyd, Tweeddale Court; London: 33 Paternoster Row, E.C. 1924. (Pp. x + 192.) Price 12*s.* 6*d* net.

Regeneration from a Physico-chemical Viewpoint. By Jacques Loeb, Member of the Rockefeller Institute for Medical Research. London: McGraw-Hill Book Company, 1924. (Pp. ix + 143, with 115 figures.) Price 10*s.*

The Great Plague in London in 1665. By Walter George Bell, F.S.A., F.R.A.S., with thirty Illustrations comprising Contemporary Prints, Plans, and Drawings. London. John Lane, The Bodley Head. New York: Dodd, Mead and Company. (Pp. x + 374.) Price 25*s.* net.

Reinforced Concrete and Masonry Structures. Compiled by a Staff of Socialists. Editors-in-Chief, George A. Hool, S.R., and W. S. Kinne, B.S. New York and London: McGraw-Hill Book Company, 1924. (Pp. xix + 786.) Price 30*s.* net.

Electrical Design of Overhead Power Transmission Lines. A Systematic Treatment of Technical and Commercial Factors; with special Reference to Pressures up to 60,000 volts, and Distances up to 100 miles. By William T. Taylor, M.Inst. C.E., M.I.E.E., and R. E. Neale, B.Sc., A.C.G.I., A.M.I.E.E. London: Chapman & Hall, 11 Henrietta Street, W.C.2. 1924. (Pp. vii + 266, with 25 plates and 48 Tables.) Price 21*s.* net.

Alternating Current Rectification. A Mathematical and Practical Treatment from the Engineering Viewpoint. By L. B. W. Jolley, M.A., M.I.E.E., A.M.I.C.E., Member of the Research Staff of the General Electric Company. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1924. (Pp. xviii + 352, with 244 figures.) Price 25*s.* net.

Electrical Machinery and Control Diagrams. By Terrell Croft. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1924. (Pp. xii + 305.) Price 15*s.* net.

Small Electric Generating Sets. Employing Internal Combustion Engines. By W. Wilson, M.Sc., B.E., M.I.E.E., A.Am.I.E.E. London: Ernest Benn, Limited, 8 Bouverie St., E.C.4, 1924. (Pp. 161, with 60 figures.) Price 18*s.* net.

La Soudure électrique à l'Arc métallique. Par S. Frimandeau. Paris: Gauthier-Villars et Cie., 55 Quai des Grands-Augustins. 1924. (Pp. 136, with 80 figures.) Price 10 frs.

The Science of Metals. By Zay Jeffries, B.Sc., Met.E., S.D., and Robert S. Archer, B.Ch.E., M.S. London: McGraw-Hill Book Co., 6 Bouverie Street, E.C.4, 1924. (Pp. xvii + 460.) Price 25*s.* net.

Instinct and the Unconscious. A contribution to a Biological Theory of the Psycho-Neuroses. By W. H. R. Rivers, M.D., D.Sc., LL.D., F.R.S. Second Edition. Cambridge: at the University Press, 1924. (Pp. viii + 277.) Price 7*s*. 6*d*. net.

Tantallus; or, the Future of Man. By F. C. S. Schiller, M.A., D.Sc., Fellow and Tutor of Corpus Christi College, Oxford. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C. 1924. (Pp. 72.) Price 2*s*. 6*d*. net.

The Life, Letters, and Labours of Francis Galton. By Karl Pearson, Galton Professor, University of London. Volume II. Researches of Middle Life. Cambridge: at the University Press, 1924. (Pp. xi + 425, with 54 plates.)

A Textbook of General Science. Volume II. Introduction to Chemistry, Geology, Biology. By G. H. J. Adlam, M.A., B.Sc., Senior Science Master, City of London School, and O. H. Latter, M.A., Senior Science Master, Charterhouse. London: John Murray, Albemarle Street, W. (Pp. viii + 248.) Price 3*s* 6*d*. net.

The Reparation Plan. By Harold C. Moulton. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1924. (Pp. x + 325.) Price 12*s*. 6*d*. net.

Elements of Cost Accounting. By Anthony B. Manning, C.P.A. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1924. (Pp. xi + 166.) Price 10*s*. 6*d*. net.

Seasonal Operation in the Construction of Industries. The Facts and Remedies. Report and Recommendations of a Committee of the President's Conference on Unemployment. With a foreword by Herbert Hoover. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1924. (Pp. xxx + 213.) Price 10*s*. 6*d*. net.

Graphic Statistics in Management. By William Henry Smith. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4. 1924. (Pp. vii + 360.) Price £1.

The Properties and Uses of Wood. By Arthur Koehler, B.S. London: McGraw-Hill Publishing Co., 6 Bouverie Street, E.C.4, 1924. (Pp. xiv + 354.) Price 17*s*. 6*d*. net.

Industrial History. By Harry B. Smith, Director, Department of Vocational Education, School of Education, University of Pittsburgh. New York: The Macmillan Company, 1923. (Pp. xii + 305.) Price 7*s*. net.

Practical Microscopical Metallography. By Richard Henry Greaves, D.Sc., and Harold Wrighton, B.Met., both of the Research Department, Woolwich. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1924. (Pp. x + 125, with 22 figures and 28 plates.) Price 16*s*. net.

Reason and Morals. An Enquiry into the First Principles of Ethics. By Israel Levine, M.A., D.Litt., Lecturer in Philosophy at University College of the South West, Exeter. Glasgow: Maclehose, Jackson & Co., 1924. (Pp. xi + 177.) Price 6*s*. net.

Elementary Experimental Statics for Schools. Written by A. P. McMullen, M.A., late Head of the Science Department, Royal Naval College, Dartmouth. Revised for the Press, with some Additional Matter and a Preface by E. W. E. Kempson, B.A., late Assistant Master at Rugby School. Cambridge: at the University Press, 1924. (Pp. vii + 315.) Price 8*s*. 6*d*. net.

Scientific Method, its Philosophical Basis and its Modes of Application. By F. W. Westaway. Third Edition. London: Blackie & Son, 50 Old Bailey. (Pp. xxiii + 456.) Price 10*s*. 6*d*. net.

Optical Measuring Instruments, their Construction, Theory, and Use. By L. C. Martin, F.R.A.S., D.Sc., A.R.C.S., D.I.C., Lecturer in the Optical Engineering Department, Imperial College of Science and Technology, South Kensington. London: Blackie & Son, 50 Old Bailey, 1924. (Pp. ix + 270, with 170 figures.) Price 17*s*. 6*d*. net.

Leaves from the Golden Bough. Culled by Lady Frazer. (Pp. xii + 249, with 16 illustrations.) London: Macmillan & Co., St. Martin's Street, 1924. Price 10*s*. 6*d*. net.

The Atmosphere and its Story. A Popular Presentation of the Science of Meteorology, free from Technicalities and Formulæ. By Ernest Frith, Fellow of the Royal Meteorological Society. London: The Epworth Press, J. Alfred Sharp. (Pp. 204, with 10 plates.) Price 6*s*. net.

Antiques, their Restoration and Preservation. By A. Lucas, O.B.E., F.I.C., formerly Director, Chemical Department, Egypt. Chemist, Department of Antiquities, Egypt. London: Edward Arnold & Co., 1924. (Pp. viii + 136.) Price 6*s*. net.

Modern Astrophysics. By Herbert Dingle, Fellow of the Royal Astronomical Society. London: W. Collins, Sons & Co., 48 Pall Mall. (Pp. xxviii + 420, with 46 plates.) Price 30*s*. net.

Science and the Art of Living. By Leonard Williams, M.D. London: Hodder & Stoughton. (Pp. 234.) Price 5*s*. net.

The Humanising of Knowledge. By James Harvey Robinson. London: Hodder & Stoughton. (Pp. xii + 119.) Price 5*s*. net.

A History of Bleaching. By S. H. Higgins, M.Sc. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1924. (Pp. xii + 176, with 9 illustrations.) Price 10*s*. 6*d*. net.

Principes généraux de l'Etirage et du Tréfilage. Par G. Soliman. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1924. (Pp. 235, with 50 figures.) Price 15 fcs.

North America: An Historical, Economic, and Regional Geography. By Ll. Rodwell Jones, B.Sc., Cassell Lecturer in Economic Geography at the London School of Economics, University of London, and P. W. Bryan, B.Sc., Ph.D., Lecturer in Geography, University College, Leicester. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xiii + 537, with 104 text figures.) Price 21*s*. net.

A Game Ranger's Note-book. By A. Blaney Percival, late of the Kenya Colony Game Department. Edited by E. D. Cuming, with 33 illustrations from photographs by the Author and Mrs. Martin Johnson. London: Nisbet & Co., 22 Berners Street, W.1. (Pp. xv + 374.) Price 18*s*. net.

Huia Onslow: A Memoir. By Muriel Onslow. London: Edward Arnold & Co., 1923. (Pp. viii + 247, with 5 illustrations.) Price 12*s*. 6*d*. net.

The Purpose of Education. An Examination of Educational Problems in the Light of Recent Scientific Research. By St. George Lane Fox Pitt. New Cheap Edition, Revised and Enlarged. Cambridge: at the University Press, 1924. (Pp. xxx + 92.) Price 4*s*. net.

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RECENT ADVANCES IN SCIENCE

PURE MATHEMATICS. By F. PURYER WHITE, St. John's College, Cambridge.

History.—G. A. Miller [*Scientific Monthly*, **9** (1924), 496–501] in an interesting article endeavours to show that five fundamental concepts, those of natural number, unknown, postulate, function, and group have played the most prominent part in the development of mathematics. The same author [*School Science and Mathematics*, **24** (1924) 509, 939–47] continues his practice of pointing out the mistakes in the current histories of mathematics; this time it is volume I of D. E. Smith's recent work which comes in for castigation.

Algebra.—It was shown by Bring and Jerrard that the general algebraic equation of the fifth degree can be reduced without loss of generality to the form

$$x^5 - x - a = 0,$$

and this equation has been solved by Hermite (1858) in terms of elliptic functions. The work depends upon the theory of the elliptic modular functions; H. B. C. Darling [*Proc. L.M.S.*, **23** (1924), 383–95] has recently given an investigation which begins on purely algebraic lines, and in which the elliptic functions are introduced in a more elementary way.

Major P. A. MacMahon [*Proc. L.M.S.*, **23** (1924), 290–316] applies the calculus of symmetric functions to investigate properties of prime numbers. The expression of an integer as a product of primes, say $n = p_1^{\pi_1} p_2^{\pi_2} \ldots$, determines a partition $(p_1^{\pi_1} p_2^{\pi_2} \ldots)$ of the number $\Sigma \pi p$, and thus an elementary symmetric function in the way explained in *Combinatory Analysis*, vol. i, chap. i. By summing the symmetric functions so obtained for every integer n, unity excepted, a function W is obtained and the results of operating on W and powers of W by the Hammond operators of the calculus of symmetric functions are calculated. It is found, for instance, that $D_n W^m = N_{n,m} W^m$, where $N_{n,m}$ is the number of representations of n as the sum of m primes. Thus $N_{2n,2}$ is the number of representations of the even number $2n$ as the sum of

two primes, and the celebrated Euler-Goldbach conjecture is thus reduced to the surmise that $D_{2n} W^2 \neq 0$ for any value of n. Major MacMahon does not succeed in making any progress towards establishing this result, but he obtains a number of theorems which are curious and interesting. For example, the function W can be expressed in terms of sums of powers $s_2, s_3 \ldots$ and it is found that in this expression s_1, s_3 and s_{13} do not occur, while every other s_n up to $n = 89$ does occur. If uneven primes are alone considered a function V may be used instead of W, obtained from W by striking out all symmetric functions, whose partitions involve the part 2; the use of the function V appears to possess several advantages.

In 1918 Prof. Whittaker gave a series for the root of an equation

$$0 = a_0 + a_1 z + a_2 z^2 + \ldots .$$

which has the smallest modulus, in the form

$$z_1 = -\frac{a_0}{a_1} - \frac{a_0^2 a_2}{a_1 \begin{vmatrix} a_1 & a_2 \\ a_0 & a_1 \end{vmatrix}} - \frac{a_0^3 \begin{vmatrix} a_2 & a_3 \\ a_1 & a_2 \end{vmatrix}}{\begin{vmatrix} a_1 & a_2 \\ a_0 & a_1 \end{vmatrix} \begin{vmatrix} a_1 & a_2 & a_3 \\ a_0 & a_1 & a_2 \\ 0 & a_0 & a_1 \end{vmatrix}} - \ldots .$$

A. C. Aitken [*Proc. Roy. Soc. Edin.*, **45** (1924), 14–22] has extended this result to obtain a series S_r^m for the elementary symmetric function of the m^{th} degree of the least r roots. This is done by obtaining the symmetric function as the limit of a quotient of two determinants of which the elements are homogeneous product sums, which the author denotes by P_r but for which the more usual notation is h_r; this quotient is then expressed as the quotient of two determinants in the coefficients a which differ in their first columns only and is expanded, by a theorem due to Schweins, in the form required. From the series the roots are easily obtained as we have the relation $S_r^1 - S_{r-1}^1 = -z_r$.

The convergence of the series S_r^m is, however, not rapid unless the ratio of the moduli $|z_r|$, $|z_{r+1}|$ is small, so that it is generally desirable to transform the equation first by a root-squaring process into one with roots widely separated in modulus. If two roots z_r, z_{r+1} have the same modulus (*e.g.* if they are conjugate complex numbers) the series S_m^r diverges, but we can use the relations

$$S_{r-1}^2 - (z_r + z_{r+1}) S_r^{1-1} + z_r z_{r+1} = S_{r+1}^2, \; S_{r-1}^1 - (z_r + z_{r+1}) = S_{r+1}^1,$$

and so obtain z_r, z_{r+1} by a quadratic equation.

As an example, all seven roots of a horrid-looking equation of the seventh degree are calculated to six significant figures.

It is a rare thing for the *London Mathematical Society* to unbend so far as to admit into its *Proceedings* a paper of an historical character, and so the title, " The Nuptial Number of Plato," of a paper by G. C. Young [*Proc. L.M.S.*, **23** (1924), 27–44] caused the writer a feeling of surprise. As a matter of fact, the historical part deals with eugenics, and is not particularly intelligible to the ordinary mortal; the remainder is concerned with proving that the only solution in positive integers, without a common factor, of the simultaneous equations $x^2 + y^2 = z^2$, $x^3 + y^3 + z^3 = t^3$ is 3, 4, 5, 6, a result which, the author claims, was known to Plato.

The field of algebraic numbers defined by an irreducible equation

$$a_0\theta^n + a_1\theta^{n-1} + \ldots a_n = 0$$

has an arithmetical invariant, the discriminant D, and to an assigned discriminant there belong only a finite number of these number-fields. W. E. H. Berwick [*Proc. L.M.S.*, **23** (1924) 359–78] gives a method for obtaining all the cubic fields belonging to an assigned discriminant; the theory is complete, except for deciding whether certain types of discriminant actually exist. The method is based on the representation

$$(a + b\sqrt{m})\,(j + \sqrt{m})^3 + (a - b\sqrt{m})\,(j - \sqrt{m})^3 = 0$$

of the equation defining the field, and is closely related to the classification of ideal numbers in the field $[\sqrt{m}]$. The question arises whether there exists a quadratic field, whose principal genus contains an assigned number of exponents of irregularity, each divisible by 3; in the author's opinion this cannot be completely settled without more experimental evidence than that afforded by Gauss's tables of quadratic forms.

Analysis.—T. W. Chaundy [*Proc. L.M.S*, **23** (1924), 143–8] gives a new proof of the uniformity of the function $z = sn\, u$ obtained by the inversion of the elliptic integral

$$u = \int_0^z (1 - z^2)^{-\frac{1}{2}}\,(1 - k^2z^2)^{-\frac{1}{2}}dz,$$

for $0 < k^2 < 1$.

J. Hodgkinson and E. G. C. Poole [*Proc. L.M.S.*, **23** (1924), 396–422] consider the conformal representation of the area of a plane bounded by two straight or circular slits which do not intersect. The area is doubly connected and may be represented, by a method based on Schwarz's principle of symmetric continuation, on the plane of a variable t slit along two portions of the real axis or, by an application of Dirichlet's principle, upon the area between two concentric circles. The problem was suggested by a question in electrostatics recently treated by A. E. H. Love (*ibid.*, **23** (1924), 337–69).

E. C. Titchmarsh [*Proc. Camb. Phil. Soc.*, **22** (1924), 282–6] proves some theorems on the solution of a system of linear equations with an infinity of unknowns. The general form of the equations is

$$a_1 x_n + a_2 x_{n+1} + \ldots = c_n \, (n = 1, 2, 3 \ldots),$$

where the numbers $a_1, a_2 \ldots, c_1, c_2 \ldots$ are given. For example, if $a_1, a_2 \ldots$ and $c_1, c_2 \ldots$ be non-increasing sequences of positive numbers such that

$$(a_1 + a_2 + \ldots + a_n) \, c_{n+1} \to 0,$$

then if $\lim a_n > 0$ the equations have just one solution, while if $\lim a_n = 0$, they have just one solution for which $x_n \to 0$.

Generalised hypergeometric series of the type

$$F(a, b, c; e, f) \equiv 1 + \frac{a . b . c}{1 . e . f} + \frac{a(a+1)\, b(b+1)\, c(c+1)}{1 . 2 . e(e+1) . f(f+1)} + \ldots$$

were studied by Thomae in 1879 by means of the calculus of finite differences; he showed that there is a group of 120 such series which are connected by simple relations. For example, we have

$$F(a, b, c; e, f) = \frac{\Gamma(e)\Gamma(f)\cdot\Gamma(s)\cdot}{\Gamma(a)\,\Gamma(s+b)\,\Gamma(s+c)} F(e-a, f-a, s; s+b, s+c),$$

where $s = e + f - a - b - c$. F. J. W. Whipple [*Proc. L.M.S.*, **23** (1924), 104–14] now gives a notation for the 120 series which exhibits the symmetry of the group and enables the various relations to be found with more facility.

T. A. Brown [*Proc. L.M.S.*, **23** (1924), 149–71] determines conditions under which an analytic funetion can be represented by a factorial series

$$e_0 + e_1 z + e_2 \frac{z(z-1)}{2!} + e_3 \frac{(z+1)z(z-1)}{3!} + \ldots,$$

which is a generalisation of Gauss's interpolation formula; he also investigates the relationship between the factorial series and an interpolation formula discussed by de la Vallée Poussin.

L. Neder [*Proc. L.M.S.*, **23** (1924), 172–84] deals with the significance of various Tauberian conditions in the theory of Dirichlet series. He shows, for example, that the formal product of two Dirichlet series $\sum_1^\infty a_n e^{-\lambda_n s}$, $\sum_1^\infty b_n e^{-\lambda_n s}$ converges for $s = 0$ if we have both $\sum_{n \leqq \lambda_m \leqq m} |a_n| = O(1)$ and a similar condition for b_n; this theorem includes all previous ones on the same matter given by Hardy, Littlewood, and others. He also

widens the conditions for the theorems by which from the convergence of Σa_n to sum A we can conclude that $\Sigma a_n e^{-\lambda_n s}$ tends to A as s tends to zero and conversely; and obtains analogous theorems for the integrals $\phi(s) = \int_0^\infty a(u)\, e^{-us} du$.

In a paper on the Converse of Abel's Continuity Theorem J. E. Littlewood [*Proc. L.M.S.*, **9** (1911), 434–48] was led to the conjecture that if $X(n)$ be any function which tends to infinity with n (however slowly), then the conditions

$$a_n = O\,[X(n)/n] \text{ and } \Sigma a_n e^{-ns} \to A$$

do not necessarily involve that the series Σa_n is summable by Cesàro means, and he verified his conjecture for the particular function $X(n) = (\log n)^a\ (a > 0)$. A. E. Ingham [*ibid.*, **23** (1924), 326–36] has now established the natural extension to Dirichlet's series $\Sigma a_n e^{-\lambda_n s}$ and Riesz's methods of summation by typical means of type λ. The functions used are not analytic, and instead of Cauchy's theorem the two-dimensional form of Green's theorem is employed.

If N(T) is the number of zeros of the Riemann Zeta-function $\zeta(s)$ for which the imaginary part t satisfies $0 < t < T$, then as T tends to infinity,

$$N(T) = M(T) + S(T),$$

where $M(T) = (T/2\pi)\ \{\log(T/2\pi) - 1$, and $S(T) = o(\log T)$.

Assuming the Riemann hypothesis, J. E. Littlewood [*Proc. Camb. Phil. Soc.*, **22** (1924, 234–42, 295–318], proves, among other things, that

$$S(T) = o\left(\frac{\log T}{\log \log T}\right).$$

Differential Equations.—If in the differential equation $Pdx + Qdy = 0$, where P, Q are polynomials in x, y, we write x/z for x and y/z for y, the equation assumes the homogeneous form introduced by Darboux (1878),

$$Pdx + Qdy + Rdz = 0,$$

where $$xP + yQ + zR = 0.$$

If P, Q, R have no common factor and are polynomials of order $m + 1$, m is called the *dimension* of the equation; the case $m = 0$ is trivial, for $m = 1$ the equation is equivalent to Jacobi's and the integration can be effected by elementary methods. For $m = 2$ no general method of integration is known, nor any complete criterion for algebraic integrability; but A. Berry [*Proc. L.M.S.* **23** (1924), 337–54] has obtained some special results which are of interest. The points common to

$P = Q = R = 0$, called *critical points*, play a fundamental part in the theory, the number is in general $m^2 + m + 1$, but less if the intersections are multiple. There is, however, a special kind of critical point, the *dicritic* point, of which the importance was pointed out by Autonne in 1891, namely, one for which $d(Pdx + Q\,dy + Rdz)$ vanishes for all values of $dx : dy : dz$. Mr. Berry proves that if there is a dicritic point (for $m = 2$), the equation can be integrated completely in terms of known functions (such as confluent hypergeometric functions, parabolic-cylinder functions, incomplete gamma functions, and so on) and give precise conditions for algebraic integrability.

In connection with various tidal problems H. Jeffreys [*Proc. L.M.S.* **23** (1924), 428–54] has obtained approximate solutions of the differential equation

$$\frac{d^2y}{dx^2} - (h^2X_0 + hX_1 + X_2)\,y = 0,$$

in which h is a constant and X_0, X_1, X_2 are functions of x; the case considered is when h is large. In particular the periodic solutions of Mathieu's equation $\frac{d^2y}{d\eta^2} + (R - 2h^2 \cos 2\eta)\,y = 0$ and of the equation obtained therefrom by the substitution of $i\xi$ for η are investigated, especially with regard to their zeros. As one would expect from the name of the author, these papers are of practical rather than of theoretical interest.

Of a quite different nature is a paper by E. Hille [*ibid.*, **23** (1924), 185–237] "On the Zeros of Mathieu Functions." Here the author considers the distribution in the complex plane of the zeros of any solution of Mathieu's equation; he admits that the case, $R \leq 0$, which he is able to carry furthest, is of little interest for the physical applications. The distribution of the zeros and points where the first derivative vanishes is in *strings*, each associated with a half-line $x = k\pi\,(y > 0)$ or a half-line $x = k\pi\ (y < 0)$, $(k = 0, \pm 1, \pm 2\ \ldots)$. A string is asymptotic to its associated line in the sense that the distance of a point in the string from the line tends monotonically to zero. The general solution has a string for every half-line, but to every half-line correspond two particular solutions which lack the associated string. Such a solution, said to be *truncated*, lacks two adjacent strings and tends to zero as z tends to infinity between the lines. When $R \leqslant 0$ these strings account for all the zeros; but in other cases there are infinitely many extraneous zeros associated with the real axis.

E. L. Ince [*Proc. L.M.S.*, **23** (1924) 56–74] studies a linear differential equation with periodic coefficients which bears the same relation to the confluent hypergeometric equation as Mathieu's equation bears to Bessel's.

The equation in question is

$$\frac{d^2y}{dx^2} + \xi \sin 2x \frac{dy}{dx} + (\eta - p\xi \cos 2x)\, y = o,$$

in which p, ξ, η are constants. When p is a positive integer there exist $p + 1$ distinct values of η (depending on ξ) for which the equation has a solution in the form of a polynomial in $\sin x$ or $\cos x$ of degree p. The $p + 1$ functions also satisfy a homogeneous integral equation. If p and ξ tend to infinity and zero respectively, so that $p\xi$ remains finite, we are led to Mathieu's equation, and the polynomials in $\sin x$ and $\cos x$ become the series for the Mathieu functions.

W. McF. Orr [*Proc. Roy. Irish Acad.*, **36** (1923), 115–30] investigates the solution of systems of ordinary linear differential equations by means of contour integrals ; he does not seem to be aware of the work of Bromwich [*Proc. L.M.S.*, **15** (1914), 401–48] along the same lines.

Integral Equations.—J. Hyslop [*Proc. Camb. Phil. Soc.*, **22** (1924), 169–85] gives a general treatment of the integral expansions of arbitrary functions from the point of view of integral equation theory ; he first summarises the principal results of Hahn, Weyl, and Carleman, and then applies them to establish, under wide conditions, an integral expansion of Fourier form, having as kernel the derivative of the characteristic differential of a complete singular symmetrical kernel. An example of the theory is given, for expansions connected with the kernel e^{-st}.

G. H. Hardy and E. C. Titchmarsh [*Proc. L.M.S.*, **23** (1924) 1–26] discuss, first by the theory of Fourier and Hankel transforms, and secondly by more powerful methods depending on the theory of analytic functions, the solutions of certain integral equations which have been considered by Bateman, Kapteyn, and Milne.

If two functions $f(x)$, $F(x)$ are connected by the relation

$$f(x) = (2/\pi)^{\frac{1}{2}} S_0^\infty \cos xt\, F(t)dt,$$

$f(x)$ is called the Fourier cosine transform of $F(x)$, and by Fourier's integral theorem we see that $F(x)$ is the transform of $f(x)$. This representation has been usually studied for functions which are integrable over the range (o, ∞), but Plancherel has developed the theory for functions whose squares are integrable ; in this case the transform of $f(x)$ has to be obtained from the formula

$$F(x) = \left(\frac{2}{\pi}\right)^{\frac{1}{2}} \frac{d}{dx} \int_0^\infty \frac{\sin xt}{t} f(t)dt,$$

as the integral similar to the one above may be divergent. It

can be proved that the square of $F(x)$ is also integrable, and we have a complete reciprocity between the two transforms. Important extensions have been made by E. C. Titchmarsh [*Proc. L.M.S.*, **23** (1924), 279–89] who obtains theorems analogous to generalisations, due to Hausdorff, of the Riesz-Fischer theorem and the Parseval theorem in the theory of Fourier series. He shows that (1) if the function $f(x)$ has its p-th power $(1 < p \leq 2)$ integrable over $(0, \infty)$, then there is a function $F(x)$ whose $p/(p-1)$-th power is integrable such that

$$f(x) = (2/\pi)^{\frac{1}{2}} \frac{d}{dx} \int_0^\infty \frac{\sin xt}{t} \mathrm{F}(t) dt,$$

and (2) under the same conditions for $f(x)$ the function

$$\mathrm{F}(x) = (2/\pi)^{\frac{1}{2}} \frac{d}{dx} \int_0^\infty \frac{\sin xt}{t} f(t) dt$$

has its $p/(p-1)$-th power integrable.

The function $F(x)$ is the same in the two theorems, but, except for $p = 2$, the reciprocity between the two functions is by no means complete. The theorems are definitely false if $p > 2$.

Geometry.—W. P. D. MacMahon [*Proc. L.M.S.*, **23** (1924), 75–93] has carried farther the work of P. A. MacMahon [*New Mathematical Pastimes*, Cambridge, 1921] on repeating patterns in two and three dimensions. A repeating polygon (triangles and quadrilaterals being included) is a closed rectilinear figure satisfying such conditions of form and construction that any number of other figures directly similar to it may be assembled together, so as to cover continuously two-dimensional space; it is proved that a closed polygon may be a repeat if, and only if, its angles are such that they may be associated together in sets of two, three, or four differently lettered angles, the sum of the angles in each set being π or 2π. The author then deduces that no *convex* repeat polygon exists of order higher than the sixth, and determines the number of re-entrant angles for repeat polygons of any order. It is further found, for example, that every triangle, every quadrilateral, and every hexagon with one pair of opposite sides equal and parallel is a repeat.

W. Burnside [*Proc. Camb. Phil. Soc.*, **22** (1924), 163–6] examines from an algebraic point of view the problem of inscribing a polygon in one conic which shall be circumscribed to another, showing that the condition of closing of the polygon depends solely on a difference equation of the form

$$(p_n - p_1)^2 p_{n-1} p_{n+1} = (p_n p_1 - 1)^2.$$

R. Vaidyanathaswamy [*Proc. L.M.S.*, **23** (1924), 317–25] studies pencils of binary quartics which have the property that any two quartics of the pencil are apolar; he calls this a ***null* pencil**. He shows that the Jacobian of such a pencil is the square of a cubic, and conversely a non-singular pencil of quartics whose Jacobian is a perfect square is a null pencil. This theorem can be extended to systems of $(n-1)$ dimensions of binary $2n$-ics, any two of the forms of which are apolar—its Jacobian is also a perfect square, and conversely, excluding singular systems. The author also discusses theorems which result by the familiar representations of binary n-ics by points in space of n dimensions.

A general plane quartic curve has twenty-four points of inflexion, but little is known of the properties of the configuration of these points or of the inflexional tangents. By an application of Clebsch translation principle for obtaining contravariants of ternary forms from invariants of binary forms, it is easily seen that the inflexional tangents are the common tangents of a curve of class 4 and one of class 6, the envelopes respectively of straight lines which meet the quartic in points of an equiharmonic or a harmonic range [see Clebsch-Lindemann, *Vorlesungen über Geometrie* (1910) t. 1, p. 500, or Grace and Young, *Algebra of Invariants*, p. 268]. A. E. Jolliffe [*Proc. L.M.S.* **23** (1924), 250–78] has now found a curve of class 6 which touches the inflexional tangents and the twelve double tangents of a Steiner complex. This occupies four pages, and is important; the remaining 25 examine in some detail the form assumed by the theorem when the quartic has singular points. The work is heavy, and is of very slight interest.

The curve of intersection of a quadric with a given quadric S is an elliptic quartic which meets every generator of either system of S in two points; moreover, there are four generators of either system which touch the curve. This last fact may perhaps be most easily seen by taking one of the four quadric cones which contain the curve and drawing the four common tangent planes of this cone and of the enveloping cone to S from the vertex. Each of these planes will clearly meet S in two generators, one of either system, which touch the curve. Also, taking the tangent at any point P of the curve, there is one quadric containing the curve and this line and there are thus three points of the curve co-tangential with P, in the sense that the tangents thereat are generators of the same system of a quadric through the curve. There are also four other points Q of the curve, the tangents at which are generators of the other system of the same quadric, and the join of any point P to any point Q passes through one of the four vertices of quadric cones through the curve. Projecting from a point of the curve

on to a plane we get a cubic curve and four co-tangential points become the points of contact of the four tangents forming a point of this cubic curve. A. C. O'Sullivan [*Proc. Roy. Irish Acad.*, **36**A (1924), 131–54] has recently given an elaborate analytical discussion of the sets of points P, determining, for example, the locus of the joins of two co-tangential points; many of his results might have been obtained much more simply from the geometry of the curve as sketched above.

In a remarkable paper "On Canonical Forms" E. K. Wakeford [*Proc. L.M.S.*, **18** (1920) 403] gave a method by which it can be discovered whether any given canonical form F is a possible form for a general quantic; the number of parameters l in F must be at least equal to the number of the coefficients of the quantic, and there must not exist a form ϕ in the contragredient variables which is apolar to each of $\frac{\partial F}{\partial l}$. The method, however, throws no light either on the number of ways in which the reduction is possible or on how it is to be carried out. H. W. Turnbull [*Proc. Camb. Phil. Soc.*, **22** (1924), 92–100] has applied Wakeford's method to determine possible canonical forms of the equation of the general cubic surface. Sylvester's expression, as the sum of five cubes, has been known since 1851, and another, $F \equiv X_2X_3X_4 + X_3X_1X_4 + X_1X_2X_4 + X_1X_2X_3 + KQ$, where X_1, X_2, X_3, X_4, K are properly chosen planes and Q is an arbitrary quadric, has recently arisen in the work of W. P. Milne on the curve of intersection of a cubic surface and a quadric, the surface $X_2X_3X_4 + X_3X_1X_4 + X_1X_2X_4 + X_1X_2X_3 = O$ being a four-nodal cubic surface through the curve of intersection of $F = O$ and $Q = O$. Mr. Turnbull proves the possibility of this reduction, and finds five other forms, each involving one or more arbitrary quadrics. It would be interesting to discover in how many ways these reductions are possible; for Sylvester's form the reduction has been shown by Richmond to be unique, and indeed this follows from the geometry of the surface; also the form $K_1Q_1 + \ldots + K_5Q_5$ is unique, being linear in the 20 parameters; but the intermediate cases present great algebraic difficulties. It would appear that Milne's reduction is possible in 255 ways.

F. Bath [*Proc. Camb. Phil. Soc.*, **22** (1924), 189–99] shows that if five lines of four-dimensional space are chords of a quartic curve in that space they lie upon a quadric variety which contains a doubly infinite set of quartic curves of which the five lines are chords, one such quartic having also an arbitrary sixth line of the quadric as a chord. By projection known theorems in three dimensions and in the plane are obtained.

In discussing the triply-infinite systems of conics (complexes) in space of four dimensions which are linear in the sense that

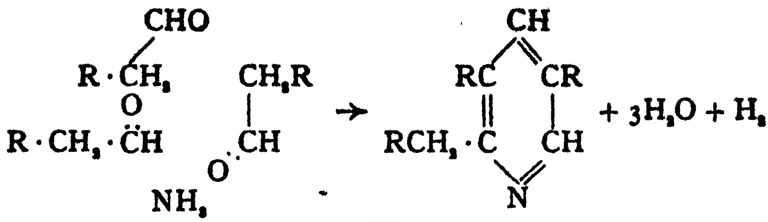
one conic of the family passes through an arbitrary point, C. G. F. James [*Proc. Camb. Phil. Soc.*, **22** (1924), 201–16] is led to consider a curve of order 6 and genus 2 which is obtained as the intersection by a general quadric variety of a normal cubic ruled surface. The conics of the complex meet this curve in 4 points, and those which touch a three-dimensional space do so in the points of a Weddle surface, having for nodes the six points in which the curve meets the space. The cubic surface contains a line, the directrix, which meets all its generators; if this meet the quadric in A, B and we join these points to the two points where a generator meets the curve we get two further points which lie as a second sextic curve of the same kind, uniquely determined by the first curve. Its cubic surface has the same directrix as that of the first, and the two surfaces meet again in a normal quartic curve. The sextics meet in two points on the directrix and in the six points where each sextic is touched by a generator.

Differential Geometry.—It was shown by Beltrami that corresponding to any ruled surface there is another applicable on it, *i.e.* having its element of arc given by the same formula $ds^2 = Edu^2 + 2Fdudv + Gdv^2$, with corresponding generators parallel, but with a parameter of distribution of opposite sign. It is clear, however, that one surface cannot be deformed into the other. B. M. Sen [*Proc. Camb. Phil. Soc.*, **22** (1924), 243–7] investigates the distinction between applicability and deformability in general, showing that Beltrami's associated ruled surface is a particular case.

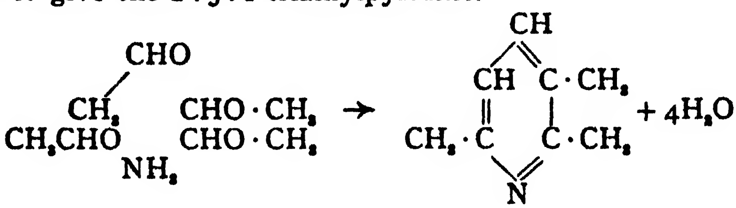
T. Kubota [*Proc. Camb. Phil. Soc.*, **22** (1924), 113–23] determines conditions that two plane curves should be transformable into each other by a Laguerre transformation (for which see *Encyk. Math. Wiss.*, iii, 1, i, p. 318); he finds that $\frac{dsdr}{r}$, r being the radius of curvature, must be the same at corresponding points, and that a function I involving derivatives of r up to the third is also invariant.

ORGANIC CHEMISTRY. By O. L. Brady, D.Sc., F.I.C., University College, London.

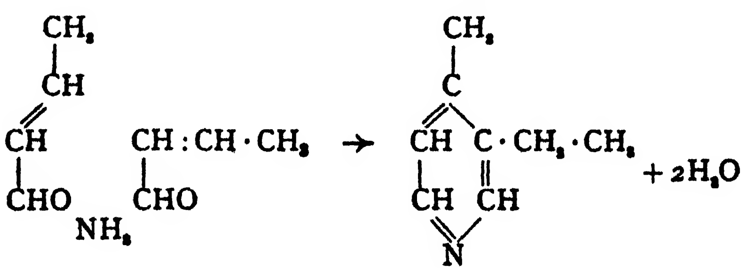
Synthesis of Pyridine Derivatives.—The synthesis of pyridine derivatives from aldehydes and ammonia has been investigated by Tschitschibabin and his co-workers (*J. Russ. Phys. Chem. Soc.*, 1905, **37**, 1229; 1923, **54**, 402–46; 1924, **54**, 601). It has been found that the reaction can proceed in several ways. With saturated aldehydes of the type $RCH_2 \cdot CHO$, three molecules react with one of ammonia leading to the formation of 2 : 3 : 5-trialkylpyridines.



Thus acetaldehyde, where R = H, gives α-picoline, but the reaction can take an alternative course giving γ-picoline. Further, four molecules of the saturated aldehyde can take part to give the 2 : 3 : 6-trialkylpyridine.



Thus, when the vapour of acetaldehyde mixed with ammonia was passed over alumina at 300° and the product fractionated, the pure compounds isolated formed 60 per cent. of the mixture, and were in the proportions α-picoline 28 per cent., γ-picoline 30 per cent., 2-methyl-5-ethylpyridine 33 per cent., 4-methyl-3-ethylpyridine 6 per cent., and small quantities of 2 : 3 : 6- and 2 : 4 : 6-trimethylpyridine. The formation of the methyl-ethylpyridines is apparently due to the production of crotonaldenyde by an aldol condensation, $2CH_3CHO \rightarrow CH_3 \cdot CHOH \cdot CH_2 \cdot CHO \rightarrow CH_3 \cdot CH : CH \cdot CHO$, two molecules of which condense with one of ammonia.

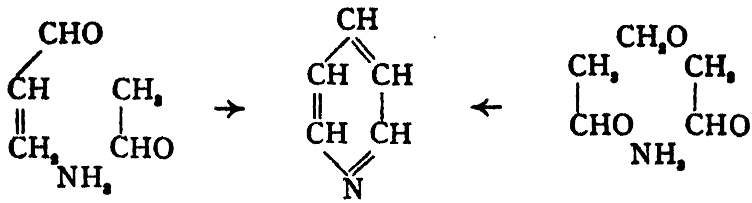


The products of the interaction of acetaldehyde and ammonia are therefore essentially the same as those obtained by Tschitschibabin by the action of ammonia on acetylene. (*J. Russ. Phys. Chem. Soc.*, 1915, **47**, 703 ; 1924, **54**, 611.)

The study of the interaction of propyl aldehyde and ammonia confirms the above explanation of the mechanism.

Recently pyridine itself has been synthesised by this

reaction from mixtures of acrylaldehyde and acetaldehyde, and of acetaldehyde and ethylal (formaldehyde and alcohol).



In both cases pyridine was the main product of the reaction; in the former some α- and β-picoline was formed, and in the latter small amounts of α-, β-, and γ-picoline and 3:5-dimethylpyridine.

Reactions of Benzoyl Peroxide.—An interesting example of the formation of a carbon-carbon linking in the benzene series has been observed by Boëseken and Gelissen (*Rec. Trav. Chim.*, 1924, **43**, 869). When benzoyl peroxide is boiled with carbon tetrachloride ω-trichloro-*p*-toluic acid is formed, together with small quantities of hexachloroethane, chlorobenzene, and phthalic acid, carbon dioxide and phosgene being evolved. The authors suggest that the reaction proceeds through the compound $C_6H_5CO \cdot O \cdot CCl_3$, the CCl_3 group migrating to the *p*-position in the benzene ring giving $CCl_3 \cdot C_6H_4 \cdot COOH$.

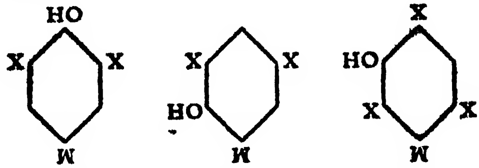
Benzoyl peroxide with dry benzene yields benzoic acid, carbon dioxide, and diphenyl, whilst *p*-chloro- and *m*-nitrobenzoyl peroxides give mono-substituted diphenyls, hence the reaction is probably in accordance with the following equation:

$$(XC_6H_4CO_2)_2O_2 + C_6H_6 \rightarrow XC_6H_4CO_2H + C_6H_5 \cdot C_6H_4X + CO_2$$

This reaction seems to provide a synthesis of the mono-substituted diphenyls which are not otherwise readily obtainable, and will probably repay study in view of the interesting examples of stereoisomerism exhibited by these compounds (Cain and others, *J. Chem. Soc.*, 1912, **101**, 2298; 1913, **103**, 586, 2074; 1914, **105**, 1437; Kenner and others, *ibid.*, 1921, **119**, 593; 1922, **121**, 614; 1923, **123**, 779; this Journal, No. 62, Oct. 1921, p. 195.)

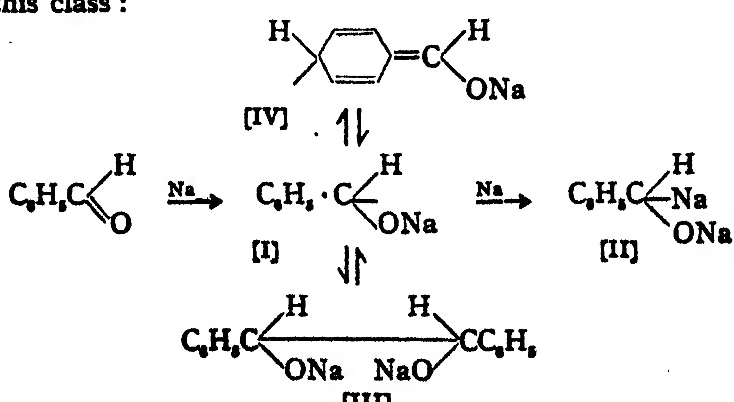
Determination of the meta-Isomeride in a Mixture of Isomerides of Some Disubstituted Derivatives of Benzene.—A chemical method for this purpose has been studied by Francis and Hill (*Jour. Amer. Chem. Soc.*, 1924, **46**, 2498) which promises to be very useful in the study of the entry of groups into the benzene ring. It depends on the fact that the hydroxyl- or amino-group has so great a directive influence that in aqueous solution, when one is present, all available ortho- and para-positions are substituted quantitatively with bromine. If the group occupies the meta-

position to another, three places are available; but if ortho- or para-, only two.



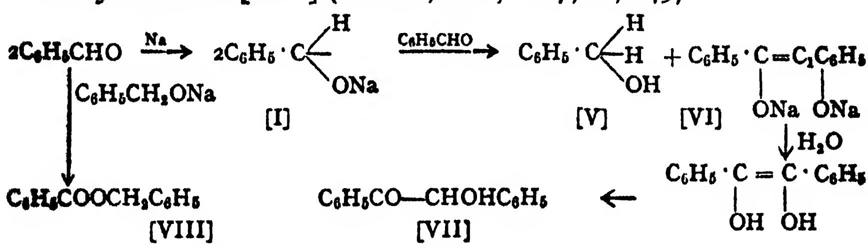
The amount of bromine absorbed can, therefore, be made a measure of the amount of *m*-isomeride present. The method can be extended to compounds containing a nitro-group, since this can be first reduced to the amino-group. A mixture of potassium bromide and bromate, subsequently acidified, is recommended for use instead of bromine. Some difficulty is experienced in certain cases owing to oxidation of the compound (*o*- and *m*-toluidines, *o*- and *p*-aminophenols), but this can sometimes be overcome. On other occasions the displacement of such groups as -COOH and -SO_3H causes trouble, but with care, and by working at a low temperature, this can be avoided.

The Action of Sodium on Benzaldehyde.—The formation of a green sodium compound without evolution of hydrogen by the action of sodium on benzaldehyde in the absence of oxygen, carbon dioxide, and moisture has long been known (Church, *Annalen*, 1863, **128**, 295; Beckmann and Paul, *ibid.*, 1891, **266**, 25). Bernoulli and Schaaf (*Helv. Chim. Acta*, 1922, **5**, 721) have also described a green addition compound obtained by the action of metallic copper on benzaldehyde. Blicke (*J. Amer. Chem. Soc.*, 1924, **46**, 2560) has studied the action of sodium more thoroughly. One or two atomic proportions of sodium react, and the author suggests the following course for the reaction, the mono-sodium compound containing tervalent carbon and behaving in an analogous way to other compounds of this class:

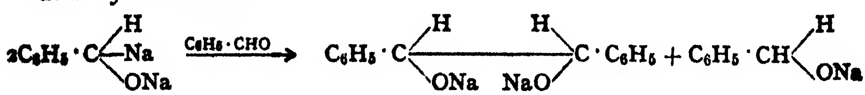


The green colour of the compound is considered justification for the quinonoid structure [IV], whilst the others are based on the decomposition by water. The first result of hydrolysis on [I] is the replacement or ONa by OH, the resulting compound then undergoing intramolecular oxidation and reduction giving benzaldehyde and benzyl alcohol (compare Schlenk and Weichel, *Ber.*, 1911, **44**, 1187); compound [III] undergoes hydrolysis with the formation of hydrobenzoin, whilst [II] gives benzyl alcohol.

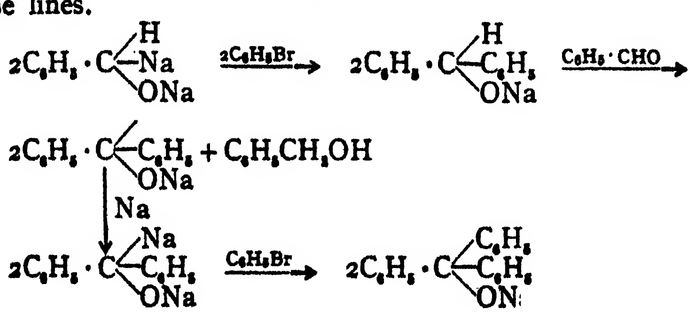
The reaction is, however, more complicated than is represented above: [I] acts as a reducing agent towards unchanged benzaldehyde, being converted to [VI], which with water gives benzoin [VII]; the benzaldehyde is reduced to benzylalcohol [V], which reacts with sodium to give sodium benzylate and hydrogen; this hydrogen reduces the sodium compound [I] with the production of more sodium benzylate; whilst, finally, the sodium benzylate converts some of the benzaldehyde into benzylbenzoate [VIII] (Claisen, *Ber.*, 1887, **20**, 649).



In the case of the disodium compound [II] this also reacts with excess of benzaldehyde, giving ultimately benzyl alcohol and hydrobenzoin.



The unexpected formation of triphenylcarbinol from benzaldehyde, bromobenzene, and sodium is explicable on these lines.



The amount of benzyl alcohol formed in this reaction corresponds to that calculated from the above.

GEOLOGY. By G. W. Tyrrell, F.G.S., A.R.C.Sc., Ph.D., University, Glasgow.

Chemistry of the Earth.—V. M. Goldschmidt has continued his investigations upon the geochemical relations of the elements in a series of papers ("Geochemische Verteilungsgesetz des Elemente, I." *Vidensk.-selsk. Skr. I. Math.-Nat. Kl.* Kristiania, 1923, No. 3, pp. 17. II. "Beziehungen zwischen den Geochemischen Verteilungsgesetzen und dem Bau der Atome." *Ibid.*, No. 4, 1924, pp 37. III. "Röntgenspektrographische Untersuchungen über die Verteilung der Seltenerz Erdmetalle in Mineralien." *Ibid.*, No. 5, 1924, pp. 58). Goldschmidt views the earth as a gigantic physico-chemical system, in which there has been a more or less complete separation of shells of different densities. These are the metallic kernel (chiefly nickel-iron), a sulphide-oxide zone, a shell of dense compressed silicates, and finally the thin crust of light silicates and silica. Founding upon this view of the original distribution of earth-matter, he divides the elements into four groups: *siderophile* (*e.g.* cobalt, platinum) concentrated in the nickel-iron core; *chalcophile* (*e.g.* copper, zinc, lead, gold, etc.), concentrated in the sulphide shell; *lithophile* (*e.g.* magnesium, calcium, and the alkali metals) concentrated in the silicate shells; and *atmophile* (*e.g.* nitrogen, argon), concentrated in the vapour envelope.

A further separation of the elements ensues with advancing physico-chemical differentiation of the lithosphere. The residue of the chalcophile elements which are entangled in the silicate shell collect in the sulphide phases of differentiation; the residues of the atmophile elements pass into the atmosphere; and the *lithophile* elements separate into definite groups depending on the course of magmatic development. The fate of the rarer elements depends first on whether they can enter isomorphously into crystallised compounds of the common rock-forming elements. Thus Cr, V, Ni, etc., isomorphous with Fe″ and Mg, are concentrated in the minerals of early crystallisation; Ba, Sr, Rb, etc., enter into isomorphous relations with the felspar-making elements, and are thus concentrated with the felspars; whilst many elements which are not isomorphous, or only feebly isomorphous, with the common lithophile elements, are concentrated in the residual liquids and gases, and thus appear in pegmatites and ore deposits. The histories of V, Fe, Ni, Cu, and Zr (as sample elements) are sketched, in regard to their physico-chemical relations, from the initial earth-mixture to their present distribution within the crust.

Prof. J. H. L. Vogt deals with a cognate but more specialised topic in his paper on nickel in igneous rocks (*Econ. Geol.*, **18**, 1923, pp. 307–53). He shows that one of the most important

factors in determining the tenor of nickel in igneous rocks is the amount originally present in the ferro-magnesian silicates. The nickel content of these minerals in peridotites is higher than in those of norites, and higher in norites than in the diallagic gabbros. In norites and other hypersthene-bearing rocks the amount of nickel increases with the amount of hypersthene. The metal also occurs in the olivine and biotite which may be present.

W. Lindgren has also sketched the concentration and circulation of the elements, but from the standpoint of economic geology (*Econ. Geol.*, **18**, 1923, pp. 419–42). Almost all metalliferous deposits are exceptional concentrations. The effect of fractional crystallisation in magmas is to produce concentrations of magnetite, ilmenite, pyrrhotite, and sulphides, which carry with them the bulk of the Ni, Co, Au, Ag, and Pt, and tend to sink out of reach of humanity into the depths of the crust. The gaseous constituents of magmas, however, work upwards with silica and the felspathic constituents, and all metals which affiliate with acid rocks and form gaseous compounds with halogens will be carried upwards into accessible regions. The aqueous salic extracts from these form pegmatites with numerous metals (*e.g.* Sn, Mo, Wo, etc.), and rare-earth elements. The more gaseous constituents are carried up in persilicic waters and ultimately form the great metalliferous deposits of the globe.

Lindgren emphasises the intense and dramatic part which is played by plants and animals in the concentration, distribution, and circulation of many elements, and follows out the stories of iron, sulphur, and vanadium from this point of view.

From a study of geophysical data, especially considerations of density and earthquake vibrations, E. D. Williamson and L. H. Adams (*Jour. Wash. Acad. Sci.*, **13**, 1923, pp. 413–28) have arrived at a conception of the distribution of matter in the earth which is analogous to that of Goldschmidt, except that no place is asssigned in it for a sulphide shell. They regard the earth as built of four layers : a thin crust of light silicates and silica, followed downwards by a thick zone of dense, heavy silicates, which, by admixture with nickel-iron, forming a pallasite zone, passes gradually into the nickel-iron core.

Goldschmidt views the earth as the result of a gigantic metallurgical operation analogous to the smelting of copper, with the production of metal, matte, and slag ; the American workers, however, regard it as analogous to the smelting of iron, with the production of silicate slag and metal only.

In a paper on the distribution of iron in meteorites and in the earth, L. H. Adams and H. S. Washington (*Jour. Wash. Acad. Sci.*, **14**, 1924, pp. 333–40) discuss the analogy between

the composition of meteorites and that of the earth. The two chief constituents of meteorites are nickel-iron and silicates, which appear to be immiscible. These constituents may be found practically unmixed in the siderite and aerolite types of meteorite respectively. In siderolites, however, the silicate may appear as scattered fragments within a nickel-iron continuum (*lithospor*), or the nickel-iron as discrete particles within a silicate continuum (*ferrospor*). Examples of the former are the pallasite types of meteorites; of the latter aerolites and the iron-bearing basalts of Greenland. The earth is believed to have a core of nickel-iron which passes up gradually, through the lithosporic stage and the ferrosporic stage, to the silicate crust. On disruption planetary bodies built similarly to the earth would yield all the known types of meteorites.

Washington has also discussed the radial distribution of certain elements in the earth (*Jour. Wash. Acad. Sci.*, **14**, 1924, pp. 435–42), and has shown that there exist inverse progressions of silicates and nickel-iron, the first increasing outwards, the latter inwards. Further, amongst the elements of the crust (except aluminium), iron, magnesium, calcium, sodium, and potassium reach maximum proportions in succession towards the crust. This is also the order of oxidisability of these elements, and their order of characteristic occurrence in the electrochemical series.

Tamman has also expressed the idea that the elements in the earth are arranged in the order of their electro-affinities, those that are more electro-positive than iron forming the mass of the crust. This general distribution has been compared to slag and iron in iron-smelting, and to slag, matte, and metal in the smelting of copper. Goldschmidt (see above) and Tamman have even suggested that a matte or sulphide zone exists between the nickel-iron core and the silicate shell.

"It would thus appear that the earth, as a whole, is mainly composed, to the extent of over 98 per cent., of only seven elements. These seven, in their order of abundance, are: iron, oxygen, silicon, magnesium, nickel, calcium, and aluminium. Smaller amounts of four others, sulphur, sodium, chromium, and potassium, bring the percentage to 99·60. Most of the other eighty-one elements may, therefore, be regarded as 'impurities,' possibly produced, as Clarke has suggested, by some process of evolution. It is noteworthy, also, that the above elements, with hydrogen and cobalt, appear to be those most abundant in the sun's atmosphere."

F. W. Clarke and H. S. Washington have now published their complete computations regarding the composition of the earth's crust (*U.S. Geol. Survey, Prof. Paper No.* **127**, 1924, 117 pp.). It is here only possible to mention some of the topics

dealt with. The average composition of the crust, of the component igneous rocks, sedimentary rocks, the lithosphere, hydrosphere, and atmosphere, and also of stony meteorites, are given. Then there is a geographical résumé and characterisation of the data which bears intimately on the problem of petrographic provinces. The final section treats of the association and distribution of the elements, and ends with a discussion of their correlation and evolution. The book will be invaluable to petrologists, and to geologists dealing with the problems of the crust as a whole.

L. H. Barnett has attempted to determine by statistical means the nature and percentage of the constituents of the earth's core, starting with the known facts concerning the mass and density of the earth, the composition and density of the outer ten miles of crust, and the Sp. Gr. of the elements, and premising that chemical laws are universal in time and space (*Jour. Geol.*, **32**, 1924, pp. 615–35). His results indicate a core consisting of approximately 90 per cent. iron, 3 per cent. Ni, Co, and Cu, and 3 per cent. of the remaining elements.

Orogeny and Epeirogeny.—G. Steinmann and his collaborators have recently produced several memoirs on Andean geology. In a paper entitled, " Über die junge Hebung der Kordillere Südamerikas " (*Geol. Rundschau*, **13**, 1922, pp. 1–8) Steinmann discourages the view of great Andean uplifts of recent date, and inclines to a moderate estimate of from 1,000 to 2,000 metres in Late-Tertiary or Quaternary times. The few hundred metres uplift of the Pacific coast since the Middle Tertiary is as much orogenic as epeirogenic. The eastern Cordilleras show the maximum of upward movement.

In his work entitled, " Umfang, Beziehungen, und Besonderheiten der andinen Geosynclinale " (*Geol. Rundschau*, **14**, 1923, pp. 69–82), Steinmann gives what is perhaps the clearest modern account of Andean structures, and institutes an instructive comparison between the Andes and the Alps. The Andes do not show the complicated " decken " structure so characteristic of the Alps, and this contrast probably has its foundation in the pre-history of the ranges, In the Alpine type there is an astonishing variety of stratigraphical facies, and rapid alternations of facies within confined regions ; whereas in the Andes the variations of facies are few, and are regional in their extent.

Steinmann's results may be summarised as follows : the Andean geosynclines of Mesozoic time from Cape Horn to the Gulf of Guayaquil were bounded to the west by an old continental mass composed of ancient crystalline rocks and probably Palæozoic sediments. In this mass the distinctive Andean features are absent. The Andean characteristics are, briefly, marine deposits of Triassic, Jurassic, and Cretaceous ages,

with several breaks of disconformable type; basic eruptives in the Jurassic and Cretaceous, granodioritic intrusions and andesitic lavas in the Tertiary associated with youthful folding. The late Tertiary and Quaternary transgressions show mostly Pacific, but partly Patagonian and Antarctic characters, and only encroach here and there on the Andean region proper. The succession agrees fairly well with the interrupted sedimentary succession of the great Brazilian Mass which bounds the Andes to the east.

H. Backlund (" Kristalline Massengesteine und die andine Geosynclinale " *Geol. Rundschau*, **14**, 1923, pp. 395–301) has attempted to correlate Andean diastrophism with the igneous activity displayed during the Mesozoic and the Cainozoic, and this paper forms a useful summary of his larger work noticed in *Science Progress*, Jan. 1925, p. 392. He distinguishes five periods of volcanic activity: plateau basalts of Pleistocene and Recent age; andesites of late Cainozoic age; " labradorite-porphyrite " lavas and tuffs of the Middle and Upper Cretaceous; " andesine-porphyrites " and quartzporphyries of the Oxfordian; and, finally, the great group of the Trias " porphyrites." Four irruptions of plutonic rocks are also known: mildly-alkalic granites of Pre-Liassic age; granitites which penetrate sediments older than the Oxfordian; adamellites intruded into strata up to the Neocomian; and, finally, a tonalite to quartz-diorite group which penetrates upwards to the Upper Cretaceous. In both plutonic and volcanic manifestations there is a tendency to vary with time towards the basic pole of the igneous series. The Cainozoic andesites and tonalites are correlated with the orogenic, and the plateau basalts with the epeirogenic movements of the Andean region.

The contents of W. Penck's article are sufficiently indicated by its title (" Über die Form andiner Krustenbewegungen und ihre Beziehungen zur Sedimentation," *Geol. Rundschau*, **14**, 1923, pp. 301–15).

A good summary of past and recent geological work on the Peruvian Cordillera in the departments of Junin and Lima is given by D. H. McLaughlin (*Bull. Geol. Soc. Amer.*, **35**, 1924, pp. 591–632). Carboniferous, Mesozoic, and Cainozoic sediments are described, and igneous activity in Carboniferous and Cainozoic ages is indicated. The physiographic history, especially in its relation to the enrichment of sulphidic ore deposits, has been closely studied.

A paper by R. T. Chamberlain on the significance of the framework of the continents (*Jour. Geol.*, **32**, 1924, pp. 545–74) points out that existing mountain-ranges so often follow the borders of continents as to indicate some genetic relationship to those borders. This, in its turn, leads to the belief that

ancient mountain-ranges also arose along continental margins, and that their trend lines may, therefore, have considerable palæo-geographic significance. A comparison of the trend lines of successive foldings from the Pre-Cambrian to the present time shows that, in many cases, the later folds have developed on the site of, or in closely adjoining parallel belts to, the older folds, indicating repetition of similar stress conditions. Judged by this criterion, the main lines of the continental platforms do not seem to have changed greatly during known geological time, and postulated ancient continents, such as Gondwanaland, cannot be recognised in this way.

In a paper on Pre-Cambrian Folding in North America (*Bull. Geol. Soc. Amer.*, **34**, 1923, pp. 679–702) W. J. Miller traverses many of the conclusions reached by Ruedemann (see SCIENCE PROGRESS, Jan. 1924, p. 383). Especially does he controvert the idea that the strike of Pre-Cambrian folding and foliation follows a "grand and simple curvature" more or less parallel to the outline of the North American continent. It is not possible in many cases to distinguish between Pre-Cambrian and Post-Cambrian foldings impressed upon the ancient rocks. Hence Miller believes there is insufficient warrant for Ruedemann's statement that North America acted as a unit in response to Pre-Cambrian diastrophic forces.

In the ensuing discussion Ruedemann answered that the Pre-Cambrian trend lines not only included the folding directions but also the directions of foliation, schistosity, the major axes of the batholiths, etc., and when thus comprehensively considered, the trend lines do show simple curvatures corresponding in general with the outlines of the continent.

Baron G. de Geer has noted a very remarkable fact in his study of the Post-Algonkian Oscillations of Land in Fennoscandia (*Geol. För. Stockholms Förh.*, **46**, 1924, pp. 316–24), namely, that the latest Glacial uplift of Fennoscandia correponds with the Pre-Cambrian peneplain of that region in its situation, altitude, and eohypses. Hence the Botnian, Baltic, and East Scandic (off west coast of Norway) flexures were already marked out in the Pre-Cambrian ; and the situation of the Algonkian uplift has dominated all subsequent movements in the same region.

Prof. E. Krenkel has assembled the topographical, geological, and geodetic data in regard to the great rift valley region of East Africa in a volume entitled, "Die Bruchzonen Ostafrikas" (Berlin : 1922, pp. 184). A summary of the work is to be found in *Geol. Rundschau*, **14**, 1923, pp. 209–32 ; and good reviews by H. Reck (*Zeitschr. f. Vulk.*, **7**, 1924, pp. 247–56) and by Prof. J. W. Gregory (*Nature*, Oct. 6, 1923, pp. 514–6), whose recent book on the Geology and Rift Valleys of East

Africa should be mentioned in connection with Krenkel's work, further help in the presentation of Krenkel's views. Krenkel adopts an intermediate position between those of Suess and Gregory. He accepts two periods of faulting and three of volcanic activity for the Nyassa basin, and he regards all the volcanic rocks as Miocene or later. The gravity observations summarised by Krenkel support Gregory's view that the Great Rift Valley began as a gentle linear upswelling of the earth's crust, and was formed by a series of infalls along the crown of the arch.

Useful summary articles on the structural plan of the mountains of Eastern Europe, on the Caledonian mountains of N.W. Europe, and on the Rocky Mountains, respectively, have been written by S. von Bubnoff (*Geol. Rundschau*, **15**, 1924, pp. 147–74), A Born (*Geol. Rundschau*, **14**, 1923, pp. 272–84), and by R. F. Flint (*Jour. Geol.*, **32**, 1924, pp. 432–41).

BOTANY. By E. J. SALISBURY, D.Sc., University College, London.

Ecology.—In the *Trans. Norfolk and Norwich Nat. Soc.* (vol. xi, part 5) Oliver compares the two dune-encumbered shingle systems of Blakeney Point and Scot Head. The similarity in general construction is most pronounced, but the Scot Head dunes as a whole present a later phase in succession than those at Blakeney. To this fact is attributed the richer flora of the former, which includes a number of shrubs not met with on the Blakeney dunes.

The changes in a county flora are dealt with by Salisbury in *Trans. Herts. N.H.S.*, vol. xviii, 1924. It is pointed out that the Herts flora, in common with that of the country as a whole, has been built up by a succession of invasions from the continent and elsewhere which is even now continuing. It is suggested that the succession of immigrating races from Neolithic man to the Normans were unconscious if not conscious agents in this dispersal. Of introductions in historic times it is significant of the influence of man that the more recent are chiefly of American provenance, the older of Continental. Only one native species, viz. *Epilobium angustifolium*, appears to have increased, a feature associated with the frequency of heath fires and the felling of woods. On the other hand, there is definite evidence of the decrease of eighty species, of which no less than 62·5 per cent. are species of damp habitats, 25 per cent. are plants of uncultivated land, 7·5 per cent. are weeds, and only 3·7 per cent. woodland plants. Data is furnished to show that the area of woodland has remained practically constant during the period that the flora has been under observation, whilst the area of heath, etc., has diminished by

at least 50 per cent. The high proportion of species of damp habitats is correlated with the lowering of the permanent water table due to (*a*) increased facilities for rapid drainage, (*b*) the increasing demands for water by the densely populated areas.

G. A. Pearson (*Ecology*, vol. v, Oct. 1924) describes experiments on seedlings of various conifers, in which it was found that there was no appreciable difference in the capacity of the different species to extract water from the soil. Thus on a clay-loam soil the wilting coefficient was 10·25 per cent. for *Picea engelmanni* and 10·56 per cent. for *Pinus aristata*, whilst that for *P. scopulorum* and *Pseudotsuga taxifolia* was between these values. On a lighter soil composed of half clay-loam and half sand, the wilting coefficients were between 5·8 and 6·35 per cent. The water transpired per unit increase on dry weight varied between 359 gms. for *P. engelmanni* to 440 for *P. aristata*, and the rate of transpiration varied with the supply of water to the soil.

Cannon in the same journal gives a brief note recording observations indicating that the ratio of growth of roots at a given partial oxygen pressure to growth at normal aeration decreases with the increase of temperature and varies directly with the partial oxygen pressure when the temperature is constant.

To test the availability of the nitrogen content of peat, Lipman and Wank (*Soil Sci.*, Oct. 1924) determined the yield of barley in dry weight grown on soil known to respond to nitrogenous fertilisers. Tests were made on untreated soil; on soil to which peat, both untreated and prepared in various ways, was added; and on soil treated with nitrogenous manures, of which the nitrogen content was equivalent to that of the peat. Whilst the last named gave yields more than twice those of the untreated soil, the peat gave only a very slightly increased yield.

In the *Arkiv für Botanik* (Dec. 1924) Svedelius discusses the discontinuous distribution of sub-tropical and tropical marine algæ. The author lays stress on the many resemblances between the marine flora of the Indian Pacific and the West Indian Oceans. Börgesen showed that 46 of the 90 West Indian Chlorophyceæ also occur in the Indian Pacific. Of the 45 Phæophyceæ, 18 are common to both regions, and this applies to 47 of the 108 Rhodophyceæ. Smaller proportions are common to the West Indies and the Mediterranean. In the genus Neomeris, for example, two species are confined to the W. Indies, two are restricted to the Indian Pacific, and two are found in both areas. A similar distribution is shown by species of *Valonia*, *Udotea*, *Halimeda*, and other genera, as also

by the marine Phanerogams. These distributions are attributed to the former connection between the Indian Pacific and the Atlantic Oceans, which implies that the present distribution dates from at least Tertiary times. Moreover, the fact that the two areas possess most Chlorophyceæ in common (51 per cent.) and least Phæophyceæ (40 per cent.) indicates that the former represent the oldest of the three groups, and the Phæophyceæ the youngest.

Cryptogamic.—New species of algæ belonging to the genera *Pithophora*, *Protoderma*, *Chondrocystis*, and *Dichothrix* are de-described by Howe (*Bull. Torrey Bot. C.*, vol. li, no. 8) and in the *Bull. d. J. Soc. Bot. Suisse*, Dec. 1924, H. Müller figures a new species of *Coccobotrys* found on calcareous rocks.

In the *Transactions B. Mycol. Soc.* for Sept. 1924 new species of *Cordyceps* and *Myriangium* are described by Petch. Two species of *Cephalosporium* which cause diseases of cultivated mushrooms are described by F. E. Smith; whilst M. N. Kidd and A. Beaumont, dealing with the fungi responsible for rot in storage apples, of which they list no less than forty-three, describe new species of *Phoma*, *Oospora*, *Cephalosporium*, *Hyalopus*, *Sporotrichium*, *Tilachlidium*, and *Graphium*.

Miss E. M. Doige and Dr. Butler describe the fungus responsible for "Citrus Scab" under the name *Sporotrichium citri*, whilst C. C. Brittlebank and D. B. Adam describe a *Pleosphæria* which causes disease in cereals and other grasses.

G. B. Sartoris (*Amer. Jour. Bot.*, Dec. 1924) finds that the normally parasitic smuts *Ustilago hordei*, *U. tritici*, and *U. heufleri* can be grown as saprophytes in artificial media.

Taxonomy.—An interesting quilled variety of the common Dandelion is described by Prof. Weiss under the name of *Taraxacum officinale* var. *cucculatum* (*Jour. Bot.*, Oct.). In the same number Dr. Ridley describes a number of new species from the Malay Peninsula, belonging to the following genera: *Sterculia*, *Tetractomia*, *Connaropsis*, *Ilex*, *Smythea*, *Micromeles*, *Eugenia*, *Oxyspora*, *Casearia*, *Canthium*, *Agapetes*, *Ardisia*, *Trachelospermum*, *Tylophora*, *Gaertnera*, *Æschyanthus*, *Gomphostemma*, *Piper*, *Baccaurea*, *Ficus*, and *Pasania*.

A large number of new species from Florida are described by J. K. Small (*Bull. Torrey Bot. Cl.*, Oct. 1924), who also describes the following new genera: *Deutoceras*, *Deeringothamnus* (Annonacæa), *Sanidophyllum* (Hypericaceæ), *Litrisa*, and *Ammopursus* (Compositæ). In the same journal new S. American plants are described by Blake and Gleason.

Anatomy.—Wilson (*Bot. Gaz.*, Oct. 1924), from a study of the Chenopodiaceæ and Amarantaceæ, arrives at the conclusion that the medullary bundles have arisen phylogenetically by the gradual centripetal displacement of peripheral bundles

into the pith. This change, supposed to have originated at the nodes, has in some species extended both above and below till the entire internode becomes involved. An early stage in this hypothetical series is presented by *Chenopodium album*, where the internal displacement is a temporary one in the nodal region; *C. ambrosioides* presents an intermediate condition, whilst the extreme modification is found in *Amaranthus*.

The structure of the pneumathodes which are produced on the adventitious roots of the oil palm has been investigated by Yampolsky (*Amer. Jour. Bot.*, Oct.). He finds they are organs of gaseous exchange whose formation can be induced experimentally by excessive soil moisture. They arise endogenously and at first possess typical root caps. The cortical region exhibits internal and external parenchymatous zones between which is situated the aerenchyma, consisting of thickened cells with copious intercellular spaces. Many other palms bear similar structures on their adventitious roots.

Genetics, etc.—Peculiar irregularities in the pollen formation of *Solandra grandiflora* are described by M. G. Campin in the *N. Phyt.* for Dec. 1924. The haploid number of chromosomes appears to be eleven or twelve, and of these the same or a higher number may arrive at one pole and five, six, or even fewer, at the other. Associated with this the tetrad frequently consists of two large and two small pollen grains. About half the pollen grains are binucleate, and triads or diads in place of tetrads are of frequent occurrence. As in *Callitriche verna*, chromosomes are sometimes extruded during the heterotype division.

In the *Journal of Genetics* Watkins deals with the chromosome variations in a cross between *Triticum turgidum* (haploid number 14) and *T. vulgare* (haploid number 21), in which the diploid number is 35. In the reduction division fourteen pairs of chromosomes are formed, but the seven remaining univalents lag behind in the passage to the equatorial region. On reaching the latter the univalents undergo longitudinal fission; all or only some become included in the daughter nuclei. In the F_2 generation, plants with less than thirty-five chromosomes have fourteen bivalents, whilst those with more exhibit a sum-total of bivalents and univalents which is twenty-one.

ZOOLOGY. By Reginald James Ludford, Ph.D., B.Sc., University College, London.

The Influence of Hydrogen-ion Concentration on the Metabolism of Protozoa.—Since it has been shown recently that the growth of tissue cultures is dependent upon an appropriate H-ion concentration of the culture medium, it is interesting to note that M. Koffmann has demonstrated that certain ciliates are

powerfully influenced by the pH of the water in which they live. Koffmann finds that the ciliates usually require a definite range of pH, with a certain optimum, for their development. Above the optimum and also below the minium, these organisms become encysted, or else they die. In certain of the Infusoria, Koffman found it possible, by adjusting the pH of the water, to bring about encystment, and by a further change, he could bring about the transformation of the encysted organism into the active free-living form ("Über die Bedeutung der Wasserstoffionen-konzentration für die Encystierung bei einigen Ciliatenarten," *Archiv für Mikr. Anat.*, 103 bd., 1924).

The Physical Character of Protoplasm.—W. Seifriz has devised a method for measuring the elastic value of living protoplasm. By means of micro-dissection needles, he introduced a minute particle of nicol into the interior of an *Echinarachnius* egg, and studied its movement under the influence of an electromagnet placed on the stage of the microscope. Protoplasm, by this method, was found to exhibit the property of elasticity, and, since emulsions are not elastic and jellies are, he has been forced to the conclusion that the ultra-microscopic structure of protoplasm is not that of a fine emulsion as has been generally believed (*Brit. Jour. of Expt. Biol.*, vol. 11, 1924).

The Behaviour of the Golgi Bodies during Cell Division.—As the result of the study of the Golgi apparatus during mitosis in transplantable tumour cells of the rat and mouse, R. J. Ludford has suggested that the manner of distribution of the Golgi bodies, into which the apparatus breaks up during mitosis, affords an indication of the rapidity of the mitotic process. In the slow-growing tumours studied, there was found to be no apparent relation between the movement of the centrosomes and the Golgi bodies. In rapid-growing tumours, however, the Golgi apparatus was separated into two approximately equal parts by the centrosomes, which carried the two parts with them to opposite poles of the spindle. Disintegration of the two parts of the apparatus, and dispersal of its elements, occurred subsequently. Since, in some rare cases, the apparatus remained intact, until the telophase of mitosis, it is concluded that the attraction between the apparatus and the centrosomes is not of fundamental importance in the mechanism of the cell (*Proc. R. Soc.*, vol. 97, 1924).

The Effect of Parasites on Tissues.—Although the various types of organisms parasitic upon animals have been extensively studied, the nature of the histological reactions of the host to infection has been comparatively neglected. Considerable interest, therefore, attaches to the work of H. P. K. Agersborg, who has investigated the response of the tissues of the fresh-water snails *Physa gyrina* and *Planorbis trivolvis* to infection

by trematode larvæ. All the tissues react by forming a secretion, which possibly has the function of an antidote to the parasitic excretions. The secretion appears in the cells of the host as granules. They are most abundant during the early infection stages, and decrease subsequently. Agersborg suggests that the secretion originates in the nuclei of the cells of the host, and is further developed in the cytoplasm, before it is discharged into the intercellular spaces (*Q.J.M.S.*, vol. 68, 1924).

Factors Regulating the Morphology of Tissues.—E. Barta has worked on the problem of the relation between the medium and the morphology of tissues and cells growing *in vitro.* In this work he used the fragments of the ureters of three-weeks-old rats, which contain epithelial cells, connective tissue, and smooth muscle. In a heteroplasmic medium without embryonic-tissue extract, the most active cells were the fibroblasts and muscle cells : the epithelium degenerated in a few days. When the same medium was used, with embryo-extract in addition, there resulted membrane formation by the epithelial cells, but no growth and division. In homoplasma and embryo-extract there was an active irregular growth, and an invasion by the epithelial cells of the other tissues present, suggestive of the invasion by carcinomatous cells of subjacent tissues (*Anat. Rec.*, vol. 29, 1924).

Dedifferentiation of Medusæ.—G. R. de Beer and J. S. Huxley have investigated dedifferentiation in the medusa *Aurelia.* They kept a number of these organisms without food in aerated sea-water. Under these conditions there accrued a marked decrease in size, and a striking change in general form, due to starvation affecting the various organs to a different extent. There occurred also a definite histological dedifferentiation, well shown in the tentacles (*Q.M.J.S.*, vol. 68, 1924).

Regeneration in Amphibian Embryos.—A. Naville has studied regeneration of the tail of the tadpole of *Rana temporaria* following experimental amputation. He finds no evidence for the view that there is cellular dedifferentiation at the beginning of regeneration. No tissue returns to an embryonic condition, such as would be the case if regeneration were a recapitulation of ontogeny. On the contrary, regeneration appears to the writer to result from an acceleration of the growth of the tissues (*Archiv de Biol.*, t. 34, 1924).

The Early Development of the Cat.—A considerable contribution to our knowledge of the embryology of the Carnivora has been made by J. P. Hill and M.Tribe, who have worked out the early development of the cat. Their paper begins with a description of the ovum, its maturation, ovulation, and fertilisation. The process of cleavage is described, and also the

structure of the late morula, and early blastocyst stages. Development has been traced up to the formation of the didermic blastocyst. The observations detailed in this paper have been discussed by the authors in relation with previous work on relevant problems (*Q.J.M.S.*, vol. 68, 1924).

Human Embryology.—T. H. Bryce has contributed the results of his recent investigations in the field of human embryology in a paper entitled, "Observations on the Early Development of the Human Embryo." This paper contains a description of three normal human embryos, belonging to the period of development preceding the cleavage of the paraxial mesoderm into the primitive segments; reference is also made to certain abnormal specimens of the same periods. The earliest of the embryos described has been the subject of a former paper, in collaboration with Prof. Teacher. In view of the importance of this specimen, the material has been re-examined in the light of later researches. Two of the embryos, however, are described for the first time. They do not exactly correspond to any mentioned in the literature of the subject, and Prof. Bryce has, therefore, been able to furnish a number of new facts of general embryological importance. The value of this paper has been enhanced by a survey of the result of other investigations on the early development of the human embryo (*Trans. R.S., E.*, vol. 53, 1924).

The Swim-bladder of Fish.—F. G. Hall has carried out a number of experiments with perches with the view to determining the functions of the swim-bladder. He is of the opinion that the primary function of the swim-bladder is undoubtedly a hydrostatic one. Since a higher oxygen content was found in the swim-bladder, under certain conditions, than existed in the blood, it is assumed that oxygen must be secreted, or transferred from the blood to the swim-bladder. Experiments carried out by the writer showed that the hydrogen-ion concentration of the swim-bladder gland is increased by external stimulation. This, he considers, indicates the secretion of a substance by the gland which may aid in the secretion of gases into the swim-bladder. The secretion is believed to be brought about by (*a*) an increased flow of blood because of the dilatation of the capillaries of the "rete mirabile," and (*b*) an increased tension of the oxygen, due to the local dissociation of oxygen from oxy-hæmoglobin (*Biol. Bull.*, vol. 47, 1924).

The Morphological Aspect of Secretion.—The problem of the origin of secretion granules has received the attention of a large number of investigators who have expressed widely divergent opinions on the matter. Some recent researches have added considerably to our knowledge in this field of inquiry. Nassonov has studied the formation of secretion

granules in several glands of Amphibians, and in a later research he has investigated the same process in some mammalian glands. In general, he finds that the secretion, whether it be of a fluid or of a granular character, makes its appearance in intimate relationship with the Golgi apparatus. The mitochondria are undoubtedly involved in this process, and Nassonov suggests that they may provide part of the material which, under the influence of the Golgi apparatus, is converted into the secretion (*Archiv für Mikr. Anat.*, bd. 97, 1923; bd. 98, 1924).

Nassonov's general conclusions as to the importance of the part played by the Golgi apparatus in secretion have been confirmed by R. H. Bowen. This investigator has called attention to the similarity between the formation of secretion granules in gland-cells, and the acrosome in the spermatid. Gatenby first showed that the acrosome of moths, molluscs, and mammals was formed in relation with the Golgi apparatus, and this observation has been confirmed by Bowen working on insect spermatogenesis. As to the function of the acrosome, Bowen says that "it is essentially a secretory product the principal function of which is to initiate the physico-chemical reactions of fertilisation" (*Anat. Rec.*, vol. 28, 1924; *Am. Jour. Anat.*, vol. 33, 1924; *Proc. National Acad. of Sciences*, vol. 9, 1923).

On the basis of a comparison between the excretion by means of contractile vacuoles in the Protozoa, and the extrusion of secretory products from cells of the higher animals, Nassonov has sought to homologise an osmophil zone of cytoplasm, surrounding such vacuoles, with the Golgi apparatus. He points out that this osmophil periphery of the Protozoon contractile vacuole, and the Golgi apparatus of the mammalian cell, are both alike demonstrable by the same technique, and further the products formed within these cell-organs are discharged from the cell (*Archiv für Mikr. Anat.*, bd. 103, 1924).

The Internal Secretion of the Ovary.—A group of investigators, including E. Allen, B. F. Francis, L. L. Robertson, C. E. Colgate, C. G. Johnston, E. A. Doisy, W. B. Kountz, and H. V. Gibson have been engaged in the investigation of the internal secretion of the ovary, upon which subject they contribute a joint paper (*Am. Jour. Anat.*, vol. 34, 1924). They have arrived at the conclusion that the seat of production of the principal ovarian hormone is localised in the follicle. Several injections of the liquor folliculi into spayed rats and mice induce accelerated growth, hyperæmia, and secretion in the genital tract characteristic of the period of œstrous. While in this induced œstrous condition the test animals experienced typical mating instincts, so that the sex instincts

appear to be ultimately dependent upon this hormone. Additional evidence for this assumption is afforded by the fact that injections into immature animals, both normal and spayed, caused the premature attainment of puberty. The follicular hormone is not species specific. It is regarded as the principal female sex hormone, since the investigators were unable to find any active substance with similar properties in the corpora lutea of either œstrous or pregnancy.

The Islets of Langerhans of Fish.—D. J. Bowie has studied the islets of Langerhans in a Teleost, *Neomœnis griseus*. By staining sections with neutral ethyl violet and Biebrich scarlet, he found similar types of cells occurred as have been described in mammalian islet tissue, and designated alpha, beta, and gamma cells. Bowie suggests that the gamma cells, which seem to be the first to appear in the islet, may give rise to the other two kinds of cells. It is interesting to note that no evidence was found of the transition between acinar and islet cells in the fish (*Anat. Rec.*, vol. 29, 1924).

The Germ-plasm Theory, and the Origin of Germ-cells.—From an investigation into the origin of the spermatogonia of adult salamanders, G. T. Hargitt adduces further evidence against the validity of the germ-plasm hypothesis. According to this writer germ-cells are to be regarded as differentiating from cells of the embryo, or from slightly differentiated or dedifferentiated cells of the adult. "The fundamental property of growth and differentiation would then explain germ-cell formation, cell-differentiation, and organogeny without the assumption of a hypothetical germ-stuff as distinct from body substance."

C. S. Simkins has studied the orign of the germ-cells in a colonial ascidian, *Ecteinascidia turbinata*. No evidence was found to support either an extra-regional origin, or early segregation of the germ-cell. The author states, however, that no cells were found before the appearance of the germinal epithelium which could be shown to have any relation to the germ line (*Jour. Morph. and Phys.*, vol. 39, 1924).

ENTOMOLOGY. By J. Davidson, D.Sc., Rothamsted Experimental Station, Harpenden.

General Entomology.—G. H. Carpenter has brought out a second edition of his valuable book *Insects, their Structure and Life*, (London and Toronto: J. M. Dent & Sons, Ltd., 1924, 335 pp., price 10*s*. 6*d*. net). The first edition appeared about twenty-five years ago, and much of the subject-matter has now been revised. A bibliography of about 250 titles is given. J. H. Comstock has published a valuable textbook entitled *An Introduction to Entomology* (Ithaca, N.Y.: The Comstock Pub.

Co., 1924, pp. xix + 1044). The first part of this work was published separately in 1919, but several important changes have been made in this new edition ; it deals with the structure and metamorphoses of insects. Part 2 deals with the classification and life-histories of the insects under twenty-five orders. W. C. Cook (*Canad. Entom.*, **56**, 229–34), as a result of certain observations of the influence of climatic variations on the flight of moths at Bozeman, Montana, concludes that the insect fauna of a given region is not a stable unit varying only in abundance from year to year, but is composed of smaller groups, each of which has a more or less definite optimum, and the composition of the fauna in a given season is very definitely related to the climatic conditions prevailing at the time of growth. D. Keilin (*Bull. Soc. Entom. France*, 1924, 125–8) discusses the primitive position of the stigmata amongst insects, and the fate of the thoracic stigmata. H. B. Weiss and E. West (*Jour. N.Y. Entom. Soc.*, **32**, 93–104), in continuation of their insect surveys in different regions of New Jersey, treat here of the plants and associated insects of a salt marsh on the coastal plain. The insect groups represented in order of importance are Diptera, Coleoptera, Hymenoptera, Homoptera, and Hemiptera. Of the collections made, 39 per cent. were phytophagous species, 21 per cent. saprophagous, 26 per cent. harpactophagous, and 13 per cent. parasitic. It is considered that the relationship between the food habits and the vegetation will probably be found to be the same for other marsh areas.

Orthoptera.—N. Ford (*Trans. Roy. Canad. Inst.*, **14**, 207–319) has made an extensive and detailed study of the muscles in Orthopteroid groups. It has been found that the characteristics of the musculature in the various groups afford evidence of their phylogenetic relationships. The muscles of the spiracles, as being less variable, are of particular interest in this respect. The position of the vaginal orifice and the nature of the subgenital plate and other neighbouring plates are more clearly understood from information regarding the muscles. The musculature of the cerci indicate that the cerci belong to the eleventh abdominal segment.

B. P. Uvarov (*Min. Agric. Egypt. Techn. and Sci. Service Bull.*, **41**) describes some new interesting Orthoptera from Egypt ; descriptions of five new species and one new genus are included.

Coleoptera.—R. T. Cotton (*Proc. U.S. Nat. Museum*, **66**, 1–11) deals with the classification of the weevil larvæ of the sub-fam. Calendrinæ occurring in N. America, which is represented by eleven genera and ninety species ; the generic characters of nine genera are given. F. van Emden (*Treubia*, **6**, 1–7) deals with the biology of *Thorictodes heydeni* Reitt., the young

stages of the larva and pupa being described for the first time. This genus together with *Thorictus* form the family Thorictiden, the exact position of which is uncertain until more is known of the larval stages. Heller, K. M. (*Philippine Jour. Science*, **25**, 287–307), describes four new genera and twenty-nine new species of Philippine Curculionidæ. An exhaustive monograph on *Dytiscus marginalis* L., dealing with practically all aspects of the subject by zoologists of the Marburg Zoological Institute, is published by E. Korscheldt (Leipzig, 1924, 8vo, 1834 pp. and 876 figs.). The Australian Coleoptera, by A. M. Lea (*Records South Austr. Mus. Adelaide*, **2**, 523–46) is paper No. 5 in the series.

Jan Obenberger (*Philippine Jour. Science*, **25**, 539–660) has an important paper on Buprestidæ collected in Singapore, Borneo, and the Philippines; keys are given for determination of the species. A. Reichensperger (*Revue Suisse de Zool.*, **31**, 117–53) gives descriptions of some new South American Histeridæ, found as guests of ants and termites. This paper is part 2, the first part having appeared in *Schweiz Ent. Mittl.*, **13**, 313. W. Schultze (*Philippine Jour. of Science*, **25**, 359–90) gives part 2 of his important monograph on the Pachyrrhynchid group of the Curculionidæ, eight genera being dealt with. H. Singh-Pruthi (*Proc. Zool. Soc.*, London, **46**, 827–69) has an important paper on the post-embryonic development and homologies of the male genital organs in *Tenebrio molitor*. The copulatory organ in insects originates as a pair of appendages which subsequently fuse to form the single tubular organ of the adult. Muir (*Psyche*, **29**, 147, and *Trans. Entom. Soc. London*, 1918, pp. 223–31) claims, however, that in Coleoptera the genital tube is a hollow diverticulum from the beginning, and at no stage shows the appearance of paired appendages. Singh-Pruthi has now shown, in the species he examined, that the genital organ does originate as a pair of appendages, and even retains its paired appearance until the pupa is three or four days old. The latter part of this paper deals with the development of the ovipositor and the efferent genital ducts in *T. molitor*. Another paper by the same author (*Proc. Camb. Philos. Soc. Biol. Sciences*, **1**, 139–47) deals with the question of Prothetely in this species and other insects. In hemimetabolic insects, the nymphs, at an early stage in development, possess rudiments of the external appendages of the adult; in holometabolic forms the appearance of these organs occur at a later stage. Sometimes the larvæ of the latter group show some imaginal and pupal organs, which phenomenon was termed by Kolbe Prothetelie, implying that the organs appeared prematurely, due to an accelerated development. Various theories have been proposed as an explanation of the phenomenon. Singh-Pruthi carried out experiments, the results of which show that the

appearance of the imaginal organs on the larvæ is not due to accelerated development, but rather to retardation of development, and the term Prothetely, as applied to the phenomenon, is incorrect. Schulze, in 1922, proposed the term Hysterotelie for the phenomenon in which larval organs appear in the imago. The known cases of Prothetely and Hysterotely are really cases of the same phenomenon, in which the pupation or metamorphosis of the larva is inhibited, and therefore come under the term Nootemy, the other terms being superfluous.

Lepidoptera.—A. F. Braun (*Ann. Entom. Soc. Amer.*, **17**, 234–58) gives an account of the frenulum and retinaculum in both sexes, in the more generalised families of Lepidoptera, especially the so-called Microlepidoptera. It seems probable that a study of the progress of development and specialisation within the order may throw some light on the phylogenetic relationships in the Lepidoptera. The male retinaculum hook or frenulum is uniform in structure throughout the order, and it is rare to find a deviation from the general type; the same uniformity in the structure of the sub-dorsal retinaculum may be observed, but on the female other retinacula have developed in the course of evolution.

J. R. Eyer (*Ann. Entom. Soc. Amer.*, **17**, 275–343) has a paper on the comparative morphology of the male genitalia in primitive Lepidoptera which will be of interest to the taxonomist and comparative morphologist. W. T. M. Forbes (*Jour. N.Y. Entom. Soc.*, **32**, 146–58) gives a systematic study of the genus *Mechanitis* Fabr., keys to the various forms with notes on the species being given. Two papers dealing with the Lepidoptera fauna of the Galapagos islands, by W. Schaus (*Zoologica*, **5**, 23–48) and W. Beebe (*Zoologica*, **5**, 51–59), give descriptions of about fifty new species.

Diptera.—The following papers are of interest to the systematist: M. Bezzi (*Bull. Entom. Res.*, **15**, 73–155) gives further notes on the Ethiopian Trypaneidæ, a complete synopsis of all known species (about 390) being given in the form of keys. G. F. Ferris (*Philippine Jour. Science*, **25**, 391–401) describes and figures five species of Diptera-pupipara from Philippines, four of the species being from bats. G. H. Hardy (*Proc. Linn. Soc. N.S. Wales*, **49**, 360–70) gives a revision of the Australian Chiromyzini. J. C. H. Meijere (*Tijdschr. v. Entom.*, year 1924, *Suppl.*, pp. 1–64) has a third paper on the Diptera of Sumatra. A revision of the sub-fam. Sarcophaginæ in the Oriental region, especially the genus *Sarcophaga*, is dealt with by R. Senior-White (*Rec. Indian Mus.*, **26**, 193–283). R. C. Shannon and J. D. Dobrosky (*Jour. Washington Acad. Sci.*, **14**, 247–53) deal with a systematic treatment of the N. American species of Protocalliphora, the larvæ of which are parasitic on nestling

birds, and J. D. Tothill (*Canad. Entom.*, **56**, 257–69) has a revision of the nearctic species of the Tachinid genus *Fabriciella*.

F. Bodenheimer has published further detailed studies of *Tipula oleracea* dealing with the synonymy, distribution, and anatomy of the adult (*Arch. Naturgesch.*, **90**, Abt. A, 61–108), and the morphology and biology of the larva (*Zool. Jahrb.* 1924 *Abt. System.*, pp. 129–54). J. G. H. Frew (*Ann. Appl. Biol.*, **11**, 175–219) describes the life-history of *Chlorops tæniopus* in detail and gives an account of experiments showing the effect of different manurial treatment on the degree to which barley plants suffer from the attacks of this insect. Treatment with certain manures, particularly superphosphate, results in a reduction of the infestation of summer barley, due to the stimulating effect on the maturing of the ear and the growth of the ear-bearing internode.

The question of the sex of adult forms arising from parthenogenetically and pædogenetically produced individuals has been the subject of much speculation. Parthenogenesis among diptera was first noted in *Miastor*, by Wagner, in 1863, the larvæ being found to produce young (pædogenesis) parthenogenetically. Imagines of both sexes are found, but it was not clear that males arose from larvæ which had been produced by pædogenesis. R. G. Harris (*Psyche*, **31**, 148–54), working with *Oligarces* sp. (Cecidomyidæ), has shown that males and females may arise from pædogenetic larvæ, but that both sexes are not, under normal conditions, produced by the same individuals. Two types of pædogenetic larvæ, in respect to the sex of the pupæ and adults they produced, were found ; these male-producing and female-producing types are indistinguishable morphologically, and the evidence indicates that the distinction is genetic, the potentiality for producing males only or females only being inherited, resulting in male-producing and female-producing strains.

R. Newstead (*Liverpool School Trop. Med. Memoir*, I, New Series, 334 pp.) gives a comprehensive account of the biology, distribution, and bionomics of the known species of *Glossina* (Tsetse-flies). J. H. Stekhoven Schuurmans (*Treubia*, Buitenzorg, **5**, 299–330) describes five Tabanid species from the island of Buru, Moluccas, only one Tabanid species having been hitherto recorded from this island. The œsophageal diverticula in mosquitoes is the subject of a paper by W. R. Wright (*Ann. Trop. Med. and Parasitology*, **18**, 77–82). These diverticula arise as three sacs from the posterior portion of the œsophagus in both sexes ; Wright considers that these organs are food reservoirs, but that this function is in abeyance in the blood-sucking females.

Hymenoptera.—E. Bugnion (*Mitt. d. Schweizerischen Entom. Gesell. Bull. Soc. Entom. Suisse*, **13**, 368–96) gives the second part of his studies on the mouth parts of *Scolia*. F. Santschi (*Mem. Soc. Vaudoise Sci. Nat.*, 1923, No. 4, 137–75) has a lengthy paper on the question of sidereal orientation in ants. The modes of orientation are different, depending on the stimuli utilised and the receptors affected. Topo-æsthetic orientation is that in which the animal goes in direct contact with the source of stimulation. Tel-æsthetic orientation is due to the reaction to distant stimuli. The former type may result from (*a*) the testing of the consistence of the surroundings or by feeling the shape or texture of things, (*b*) the recognition of traces of odours, (*c*) the registration of the intensity of muscular contraction so that the contractions can be repeated, thus, for example, giving an estimation of distance. The latter type may be due to the effects of gravitational influences, to distant odours, to heat, sound, and light.

W. M. Wheeler (*Psyche*, **31**, 136–9) describes a gynandromorph of the ant *Tetramorium guineense* F., which, unlike previously recorded ant gynandromorphs, is a pure antero-posterior type, the head being that of a normal male and the remainder of the body being female with well-developed wings.

Hemiptera.—The first report on the Coccidæ of Palestine by F. S. Bodenheimer (*Zionist Organisation Agric. Expt. Sta.*, 1924, *Bull.*, **1**, 100 pp.) includes a history of the subject and distribution of the family in that country.

Students of the aquatic Hemiptera will be interested in three papers by H. M. Hale (*Records South Australian Museum*, Adelaide, **2**, 309–30, 397–424, 503–22).

H. N. Knight (*Ann. Entom. Soc. America*, **17**, 257–74) has an interesting paper on colour patterns in Heteroptera. Breeding experiments with *Perillus bioculatus* show that the colour pattern of individual bugs is influenced more by external conditions than by inheritance; black and brown colours are located in the cuticula and are not subject to changes once the adult stage is reached; white, yellow, and red colours are located in the hypodermis and show through the areas where the black colouring is absent in the cuticula; the white colour occurs in the absence of pigment granules or globules in the hypodermal cells, while in the yellow and red varieties the hypodermal cells are filled to a greater or a less degree with minute coloured globules; the accumulation of pigment globules in the hypodermis is influenced largely, if not entirely, by the physiological activities of the bug, which activities are governed very largely by the temperature of its environment, and the colour of the adult is determined by the conditions under which the nymph has developed. J. M. Puri (*Parasitology*, **16**, 269–78), in the

second part of his studies on the anatomy of *Cimex lectularius* L., deals with the stink organs, which are considered as having a defensive function and probably also a sexual function in the adult. An important paper on symbiotic fungi in coccids, by W. Schwartz (*Biol. Zentralbl.*, **44**, 487–528), deals with the physiological aspect of the problem, previous investigators having studied the question from the anatomical and developmental point of view. The author concludes that the symbionts are in full relationship with the animal organism, but that the condition obtaining is not symbiosis in the restricted sense. It would appear that the fungus is essentially concerned with the breaking down of the albuminoid end-products of the animal's metabolism. L. M. Staniland (*Bull. Entom. Res.*, **15**, 157–70) shows the relationship between the relative resistance of various varieties of apple stocks to *Eriosoma lanigerum* and the different distribution of the sclerenchymatous tissue in these varieties; the factor appears to be important as affecting the accessibility to the aphis of the tissues rich in sap. Part 3 of "The Aphididæ of Formosa," by Takahashi Ryoichi (*Dept. Agric. Govt. Res. Inst. Formosa*, Rept. **10**, 1924, 121 pp.), includes a food plant catalogue.

Other Orders.—Students of Collembola will find the paper by J. R. Denis (*Ann. Soc. Entom. de France*, **93**, 211–60) on the Collembola of the Paris Museum of interest; many type species are described and several new species erected. H. E. Ewing (*U.S. Nat. Mus. Proc.*, **63**, 1924, Art. 20, 42 pp.) has a paper on the taxonomy and distribution of the biting lice, fam. Gyropidæ; three new sub-families, seven new genera, and twelve new species are described. The same author (*Proc. Ent. Soc. Washington*, **26**, 209–10) records five species of fleas from the island of Hawaii: these species are all of economic importance, having regard to the spread of flea-borne diseases, and have evidently been introduced to the island. An intracellular symbiont is recorded as present in the hog louse *Hæmatopinus suis* L. by L. Florence (*Amer. Jour. Trop. Med.*, **4**, 397–409): the symbiont is located in mycetocytes on the wall of the midgut and thought to be related to the physiology of digestion: there is a mycetome in each oviduct in the female which is transmitted through the egg. J. J. de Gryse and R. C. Treherne (*Canad. Entom.*, **56**, 177–82) deal with the male genital armature of the Thysanoptera, the results of which studies may afford evidence as to the phylogenetic relationship of the group.

R. C. Hartwell (*Ann. Entom. Soc. America*, **17**, 131–58) has investigated the olfactory sense of the termite *Reticulitermes flavipes* (Kollar) in order to analyse the assertion of McIndoo that the olfactory sense in insects is not located on the antennæ but on the legs and wings. Experiments made with different

substances show that termites do possess an olfactory sense, although not well developed, which appears to be connected with the antennæ, since response to odours does not occur if the antennæ are rendered functionless: definite conclusions cannot, however, be drawn from these limited experiments. It is nearly thirty years ago since Kirby published his work on the Dragon-flies of Ceylon, and the recent catalogue by F. F. Laidlaw (*Spolia Zeylanica*, Colombo, **12**, 335–74), based largely on collections made by E. E. Green, brings the records of the Dragon-fly fauna up to date. Thirty-three species of Ephemeroptera from Sunda Island and the Philippines are described by G. Ulmer (*Treubia*, Buitenzorg, **6**, 28–91), only two species having hitherto been recorded from the Philippines.

ARTICLES

THE ELECTRON AS A KEY TO ATOMIC STRUCTURE

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The fact that ionisation occurs when electrons with sufficient energy collide with atoms of a gas or vapour has been known for a considerable time, but it was not until the formulation by Bohr, in 1913, of his theory regarding the extra-nuclear electrons of atoms, that the possibilities of electron bombardment as a means of investigating the extra-nuclear structure, and of modifying it so as to produce atoms in abnornal conditions, came to be realised. In the hands of numerous experimenters the electron may be said to have proved as useful an agent for the investigation of the outer atomic structure as the α-particle is found to be in unravelling the constitution of atomic nuclei.

According to Bohr's theory, each of the planetary electrons of an atom revolves in an orbit (elliptical or circular) with the nucleus in one focus, each electron describing its own independent path. Contrary to the electromagnetic theory, no emission of radiation accompanies the revolution of the electrons in their orbits. In the normal state of an atom all the electrons are describing definite orbits distributed in accordance with certain assumptions, and the potential energy of the atomic system is a minimum. According to the theory, however, there are various other states in which the atom can exist temporarily, when one or more of the planetary electrons describe orbits of different dimensions and eccentricities from those described in the normal state. These abnormal states of the atom may be termed " excited " states, and in such states the energy of the atomic system is always greater than it is in the normal or permanent state. In general the atoms tend to revert from the excited states back to the normal state, either directly, or by passing through intermediate excited states, and an emission of radiation results from these changes from one state to the other. The ionised state of an atom is that particular state in which one of the planetary electrons

has been completely removed, leaving the residue with a positive charge.

Suppose A and B denote two successive states of the atomic system during a reversion to the normal state, the potential energy W_B of state B being less than the potential energy W_A of state A. Then, according to the theory, when the atom passes from state A to state B, the energy difference $W_A - W_B$ appears in the form of a monochromatic radiation of frequency ν given by the relation $W_A - W_B = h\nu$ where h is Planck's constant.

In an atom containing several planetary electrons these are bound to the nucleus with varying degrees of firmness, some being much more readily detached than others. Speaking generally, we may say that the planetary electrons can be divided into groups, such that approximately the same amount of energy is required for the removal of any electron from a particular group. The most firmly bound are known as the K electrons, the next as the L electrons, and so on. The displacements of one of the most loosely bound electrons to the various abnormal orbits, and the subsequent reversion of the atom to the normal state, give rise to radiations belonging to the *arc* spectrum of the element. Radiations belonging to the *spark* spectrum of the element are produced by similar transitions of a loosely bound electron in a positively charged atom, *i.e.* in one which has already had one or more of these electrons completely detached.

The importance of the electron as an instrument for investigating the relations between the normal state and the various excited states of the atom lies in the fact that experiment has shown that it is possible for a normal atom to be converted to an excited state, or for an abnormal atom to be converted to another state of greater excitation, by the transference to the atom of the kinetic energy of an electron which collides with it. The kinetic energy of a moving electron is connected with the potential difference V through which it has fallen by the relation $\frac{1}{2} m v^2 = e$ V, where e is the electronic charge. In order that an electron, at an encounter with an atomic system, may be able to transform it from a state in which the energy is W_1 to a state in which the energy is W_2, its kinetic energy $\frac{1}{2} m v^2$ must not be less than $W_2 - W_1$. Therefore, the smallest value of V, the potential difference, which will enable the atomic system to be transformed in this way, is given by $e\mathrm{V} = W_2 - W_1$. This value of V is thus a critical value, and is referred to as a *critical potential.*

The manipulation of the electron in investigating the possible excited states of an atom therefore resolves itself into the determination of the critical potentials which can give rise to the various abnormal atomic conditions.

Outline of Experimental Methods of determining Critical Potentials

The smallest critical potential of an atom will be that corresponding to the displacement of one of the outermost electrons to the first possible position of displacement from its normal orbit. In general the atom reverts at once from this condition to the normal state, giving out radiation in so doing, and for this reason the lowest critical potential is sometimes referred to as the "radiation" or "resonance" potential of the atom. The smallest critical potential for the production of ionisation corresponds to the complete removal of one of the most loosely bound electrons, and this potential is frequently spoken of as the "ionisation potential" of the atom. There are, of course, other ionisation potentials corresponding to the removal of more firmly bound electrons, but the term is commonly used in the restricted sense just mentioned.

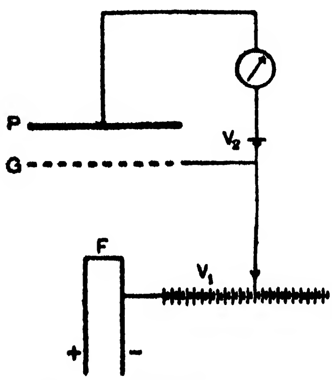

Fig. 1.—Diagrammatic representation of the arrangement of electrodes and electric fields in the "inelastic collision" method.

The experimental procedure for the determination of atomic critical potentials is simplest in the cases of the monatomic gases and vapours, for in these cases the encounters between electrons and atoms have been shown to be perfectly elastic except when a definite transformation of kinetic energy of the electron, to potential energy of the atom, takes place.

The methods by which the critical radiation potentials may be measured can be divided into two classes:

(*a*) Those in which a loss of kinetic energy of the bombarding electron is detected.

(*b*) Those in which the resulting radiation is detected.

Methods of class (*a*) are known as "inelastic collision" methods, and one of these has been largely employed, in the cases of the metallic vapours, by Tate, Foote, Meggers, and Mohler. This method is particularly valuable in the case of elements having very low critical potentials, and whose resulting radiation is not photoelectrically active.

The essential points of the method can be illustrated by reference to Fig. 1. Electrons from a glowing filament F are accelerated towards a metallic "grid" G by means of a potential difference V_1, of variable magnitude, applied between these electrodes. Those electrons which pass through the interstices

of the grid encounter a constant small retarding difference of potential V_2 applied between the grid and a plate P. The electrodes are enclosed in a glass containing vessel which can be filled with the gas or vapour under test, at a low pressure. For values of V_1 greater than V_2, the electrons which pass through the grid are able to reach P unless they lose some or all of their kinetic energy by encounters with gaseous atoms. If the current reaching P is measured—with an electrometer or sensitive galvanometer—for gradually increased values of V_1, and plotted against this potential difference, the stages at which the electrons acquire sufficient kinetic energy to produce excited states of the atoms with which they collide are indicated in the resulting curves by inflections towards the axis of potential difference. Owing to the finite velocities with which the electrons are emitted from the filament, and to contact differences of potential, etc., the value of V_1 at which the first inflection occurs in the curve is not in general exactly equal to the first excitation potential V. An accurate determination of V can be made from observations of the type described, by arranging so that for large values of V_1 the electrons make several collisions with gas atoms, at each of which they lose an amount of energy corresponding to a potential difference V, during their progress between the filament and the grid. This can be done by using the gas at such a pressure that the distance between these electrodes is several multiples of the mean free path of the electrons in the gas. When V_1 reaches, and passes, a critical stage the curve indicates a bend towards the potential difference axis, because many of the electrons are losing practically all their kinetic energy and are thus unable to reach P. As V_1 is increased, the amount of kinetic energy which each electron retains after collision ultimately becomes sufficient to carry it to this electrode, and the current increases again with increasing V_1 until a stage is reached at which an electron, after having made one collision, acquires, before reaching the grid, sufficient energy to undergo another inelastic collision, when, if the pressure conditions are properly adjusted, another inflection is obtained in the curve. The successive inflections in the curves will then be separated by potential difference intervals *exactly* equal to V, which is thus obtained independent of correction.

By employing pressures such that the distance between the filament and the grid is only of the order of the mean free path of an electron in the gas under test, the electrons can be prevented from making successive collisions in which they lose energy corresponding to the first critical potential. In these circumstances the current-potential difference curves will show inflections towards the potential difference axis for values of V_1 at which states of greater excitation of the bombarded atoms

are produced, and thus critical potentials higher than the first can be determined.

Various modifications of this method of inelastic impact have been employed, each introducing some refinement with a view to obtaining sharper inflections in the curves and an increased resolving power for critical potentials separated by very small voltage intervals. Space, however, does not permit any detailed description of these modifications in the present article.

The method we have been considering is not usually employed for the measurement of ionisation potentials because the interpretation of results is complicated by the fresh ions produced when ionisation occurs. A method of determining ionisation potentials, which has been very largely employed, can be considered simultaneously with the methods (*b*) of determining excitation potentials. This can be explained by reference to Fig. 2. The electrons from the glowing filament F acquire energy by falling through a variable difference of potential V_1 applied between F and a metal grid G. In general the distance between F and G is small, 2 mm. or so. Between G and B (a second similar metal grid) a constant potential difference V_2 is applied, opposing the motion of the electrons towards B, and larger than the maximum value of V_1 to be employed, so that no electrons from F ever reach and pass through B into the space beyond. Between B and a metal plate P, separated by a distance of only a few mm., a small difference of potential is applied such that P is positive to B. Now if the electrons from F produce ionisation of the gaseous atoms between G and B when they collide with them, the resulting positive ions will be urged towards the gauze B and some will pass through into the space beyond and, having acquired energy from the large potential difference V_2, between G and B, will reach P in spite of the small potential difference opposing their motion, thus causing an electrometer connected to P to register a positive current. If, however, the encounters between electrons and atoms in the space between G and B do not produce ionisation, but merely result in the production of excited atoms, then the return of these atoms to the normal condition yields radiation which is in many cases of such a frequency as to be photo-electrically active. With the electric fields as described the occurrence of such collisions will be indicated by the registration

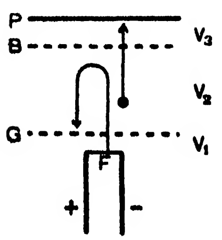

FIG. 2.—Diagrammatic representation of the arrangement of electrodes and electric fields in the Davis and Gouche method of measuring excitation and ionisation potentials.

of a *negative* current, for since B is negative with respect to P, photoelectrons will leave B and travel towards P. Hence if the current to P is measured as V_1 is increased, and the results plotted, the curve will be found to have the general form shown in Fig. 3, where A indicates the commencement of radiation and B the commencement of ionisation. By making the potential difference V_2 between the upper grid and the anode P opposite to, and greater than, V_2, the positive ions resulting from ionisation of the gas can be prevented from reaching P, and the critical potentials for the production of radiation can be examined alone. As the currents due to ionisation are in general very much larger than the photoelectric currents caused

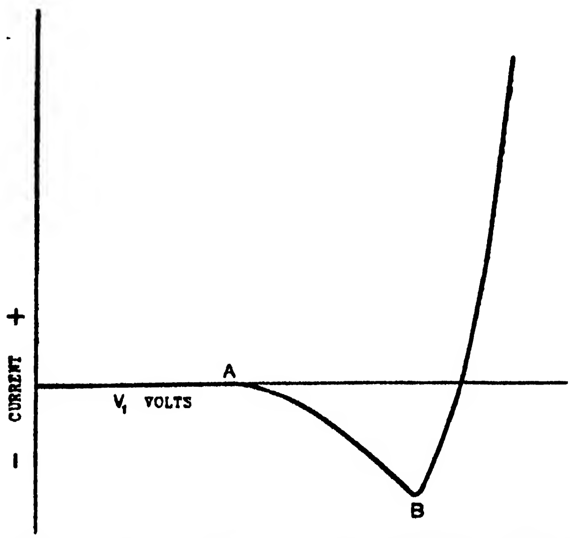

FIG. 3.—Typical current-voltage curve showing radiation beginning at A and the production of positive ions beginning at B.

by the radiation resulting from the return of excited atoms to their normal state, the arrangement of electric fields just described is especially useful for the investigation of critical excitation potentials above the first ionisation potential, *e.g.* the critical potentials for the displacement of a second electron after one has been removed.

All critical potentials measured by the method just described are subject to a correction to allow for velocity of emission of the electrons from the filament, etc. Elaborations of the method to yield the critical potentials independently of correction have been devised, but again space does not permit of their inclusion in this short account.

The photoelectric method, just described, for measuring

the critical potentials of gases and vapours, can be employed in the case of solids also. The method consists in bombarding, in the highest obtainable vacuum, a target of the solid, with electrons whose energy is variable, and in investigating how the photoelectric current from an electrode exposed to the action of any radiation which may result from the bombardment of the target varies with the potential difference V_1 applied to vary the energy of the bombarding electrons, suitable arrangements of electric fields being employed to prevent ions resulting from the bombardment reaching the detecting electrode. If the ratio of the radiation current to the total electron current is plotted against V_1 it is found that for any given target an approximately straight line relation is obtained, with abrupt changes of slope at various stages. These abrupt changes of slope indicate that some new type of atomic disturbance is occurring, the recovery from which causes additional radiations to be superposed on those already in existence. Radiation which is not characteristic of the particular target employed will, of course, result from the sudden stopping of the electrons by the target; but it can readily be ascertained whether any bend in the curve is associated with this general radiation, or with a radiation characteristic of the target, by testing with several targets in turn. If the bend occurs at the same voltage with all the targets, it can be attributed to the general radiation.

If the radiation which is produced at the first critical potential of a gas or vapour has a frequency falling in the visible region of the spectrum, we can locate this critical stage by watching for the first appearance of lines in the spectrum of the discharge as we gradually increase the energy of the electrons bombarding the gas. We may expect to obtain, first of all, a spectrum consisting of a single line, or a single doublet or triplet, and to get the complete arc spectrum when the ionisation potential is reached. Even if the radiation emitted at the first radiation potential does not fall in the visible part of the spectrum, the *ionisation* potential can be determined by watching for the appearance of lines in the visible region as the energy of the electrons is increased. A form of apparatus which has been found suitable for investigating critical potentials in this way is shown in Fig. 4. It contains two lime-coated platinum filaments F, F, which are employed one at a time. A short distance below the filaments is a gauze cylinder G open at the end remote from the filaments. Just inside the open end of the cylinder is a circular platinum plate A which serves as anode. These electrodes are sealed into a small glass vessel which just fits between the pole pieces N and S of an electromagnet. The energy of the bombarding electrons from the

filament is varied by varying the potential difference applied between the filament and G, and G and A are usually maintained at the same potential. The apparatus contains the gas or vapour at a low pressure, and when the discharge becomes luminous the magnetic field causes the luminosity to be concentrated into a bright central column of light between the grid and anode, and this column is observed through a wave-length spectrometer. When the various critical stages are reached the advent of new lines in the spectrum of this luminosity can be readily detected.

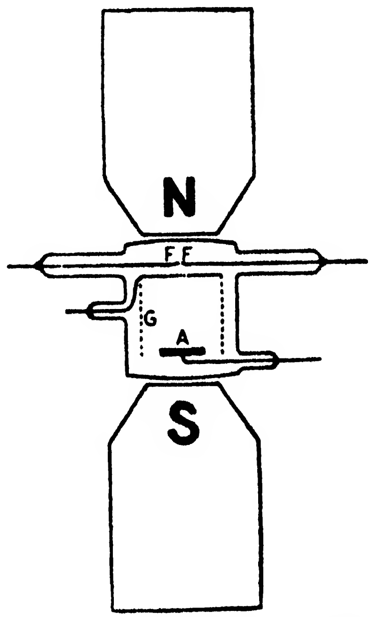

FIG. 4.—Apparatus for concentrating the luminous discharge by means of a magnetic field.

In determining the ionisation potentials of gases by methods similar to those described, it must be borne in mind that if the experimental conditions are such as to make it possible for an appreciable proportion of the atoms which are in excited states to be struck by other bombarding electrons, before they have reverted to the normal state, ionisation of the gas by cumulative effects will occur below the normal ionisation voltage. Such effects are, however, only likely to occur to anything but a negligible extent when very intense bombarding electron streams are employed, and when the gas pressure is of the order of 1 mm. or more.

Discussion of Critical Potentials in Relation to Spectra, etc.

We will now pass on to a general consideration of the results which have been obtained, and of their value in throwing light on the arrangement of the planetary electrons in atoms of different types. For every elementary substance whose atomic ionisation potential has been measured, indications have been obtained of the existence of lower critical potentials at which radiation is produced. This fact constitutes very definite evidence in support of the general idea of excited states of atoms upon which Bohr's theory is based. That excited states of atoms can be produced by electron bombardment enables a test

to be made of the explanation which the theory offers of the origin of spectrum lines. Suppose, for instance, that critical potential determinations, by electrical methods, have shown that four different stages of excitation of an atom of a particular element can be produced by electron bombardment below the ionising potential, and suppose the critical kinetic energies of the electrons producing these different types of excited atom are denoted by E_1, E_2, E_3, and E_4 respectively. Then, if we denote the energy of the normal condition of the atom in question by N, the energies corresponding to the various excited states are $(N + E_1)$, $(N + E_2)$, $(N + E_3)$, and $(N + E_4)$. Now if the energy of bombardment is adjusted to produce atoms in the state having energy $(N + E_4)$ we might expect to find evidence of the production of spectrum lines corresponding to any of the following transitions:

$$(N + E_4) \rightarrow (N + E_3),\ (N + E_4) \rightarrow (N + E_2),\ (N + E_4) \rightarrow (N + E_1),\ (N + E_4) \rightarrow N.$$
$$(N + E_3) \rightarrow (N + E_2),\ (N + E_3) \rightarrow (N + E_1),\ (N + E_3) \rightarrow N.$$
$$(N + E_2) \rightarrow (N + E_1),\ (N + E_2) \rightarrow N.$$
$$(N + E_1) \rightarrow N.$$

On the other hand, if the bombardment produces atoms having energy $(N + E_2)$ only, we could only expect to get the lines corresponding to the transitions—

$$(N + E_2) \rightarrow (N + E_1),\ (N + E_2) \rightarrow N,\ (N + E_1) \rightarrow N.$$

Similarly for the other stages of excitation, we should expect, in accordance with the theory, that certain lines could be produced and that others would be absent. As a matter of fact, the elaboration of the theory imposes certain restrictions, the so-called "selection principles," which exclude some of the transitions corresponding to those enumerated above from the list of possible inter-orbital jumps. This does not, however, affect the general argument we are considering at present, which is that, at each stage of excitation of the atom, before ionisation occurs, we should expect, in accordance with Bohr's theory, to find additional lines appearing in the spectrum of the bombarded gas. Several instances of the observation of such an effect have been obtained, among which may be mentioned the detection of stages in the excitation of the thallium arc spectrum by Mohler and Ruark, of the mercury arc spectrum by Pavlov, Hertz, and Eldridge, and the neon arc spectrum by Horton and Davies, and by Hertz. We may summarise the results of experiments on the stages in the excitation of different spectra by saying that though all the stages to be anticipated from theory have not been found in practice, this is probably to be attri-

buted to difficulties connected with the detection of faint lines, and that in all the stages which have been observed there is nothing in conflict with the theory. The large number of cases in which some of the arc spectrum lines have appeared before others gives definite support to Bohr's theory of the origin of spectrum lines.

If we accept Bohr's theory in its essential points, then in the case of elements whose spectra have been completely analysed into series, information with regard to the various possible energy states of an atom in which the most loosely attached electron has been displaced from its normal position is to hand in the *terms* of the arc spectrum. The frequency of every spectrum line in a series spectrum is expressible as the difference of two terms, *e.g.*, $1s - m\,p$, all the lines of one series having the same first term. The limit of the series is the frequency given by the first term. Now on Bohr's theory we get a set of lines related similarly to the lines of an optical series, if we group together lines corresponding to jumps from different initial orbits to the same final orbit, the limiting frequency of such a set of lines corresponding to the falling in of an electron from outside an ionised atom to the particular final orbit under consideration. Therefore, on Bohr's theory of spectra, the terms whose difference gives the frequency of any spectrum line must each correspond to some quantity characteristic of one of the orbits between which the jump occurs, the first term corresponding to the final orbit, and the second term to the initial orbit involved in the transition. Extending this idea, we see that each term is a quantity characteristic of some particular orbit. Since the second term in the expression for the frequency of any line is zero in the case of the limiting frequency of a series, *i.e.* when the atom is initially ionised, the quantity to which any term corresponds must be the frequency connected with the energy difference between the ionised state of the atom and the state associated with the term.

If the spectrum has been analysed into series the terms are known, and therefore most of the possible excited states of the atom, with its most loosely attached electron displaced, are known in relation to the ionised condition of the atom. If one of the terms of the arc spectrum corresponds to the normal state of the atom, then the possible excited states into which the atom can be put, by displacements of its most loosely attached electron, are known completely, and critical potential measurements can merely give confirmation without adding anything new to our knowledge in this respect. The question of deciding whether any term of the arc spectrum of an element, as ordinarily observed, does correspond to the normal state of the atom can be settled by reference to the absorption spec-

trum of the element. In general, it is found that certain series of lines occurring in the emission spectrum appear as series of absorption lines when white light is passed through the vapour of the element. When this is the case the series will be found to have one term in common. We have seen that emission of radiation is explained as a process occurring when an atom passes from one state of excitation to a state of less excitation, *i.e.* when an electron jumps from one abnormal orbit to another of smaller energy value, the amount of energy liberated in the form of radiation being equal to the energy difference of the two states of the atom. The converse process would be the abstraction of energy from the radiation and its transformation into potential energy of the atom in the state of greater excitation. Therefore, if white light is passed through a monatomic vapour, only certain wave-lengths will be suitable for absorption, namely those corresponding to the jumps from the normal orbit to orbits associated with excited conditions of the atom. Hence the term which is common to all lines of the absorption spectrum found under normal conditions must correspond to the normal state of the atom.

In the cases of the alkali metals, the first critical potential is found to correspond to the first line (a doublet) $1s - 2p_{1,2}$ of the principal series of the arc spectrum, and the ionisation potential corresponds to the limit of this series, namely to the term $1s$. In these cases the series of lines $1s - mp$ is the absorption spectrum of the element. Therefore in these instances the relative energy values of the various excited states of the atom with its most loosely attached electron displaced, are known completely from the spectrum terms.

In contrast to the alkali metals, we find in the rare gases helium, neon, and argon, examples of elements in which we cannot obtain all the information necessary for a complete knowledge of the possible orbits of a displaced electron from the terms of the ordinarily observed arc spectra of these elements. It has been explained that the terms of the arc spectrum enable us to arrive at the distribution of possible excited states of the atom in relation to the ionised state, and that the connection between the normal state and the excited states can be determined from the absorption spectrum of the element. In the case of the rare gases, none of the lines of the arc spectra appear as absorption lines when white light is passed through the gas. The link necessary for connecting the excited states with the normal state, simply from spectrum observations, is, therefore, missing in these cases. It has been obtained, however, from critical potential measurements, for since the ionisation potential gives the connection between the normal state and the ionised state, the connection between the normal

state and the excited states corresponding to the various spectrum terms follows directly. In the case of helium the ionisation potential has been found to be about twenty-five volts, and two radiation potentials have been found differing from the ionisation potential by voltages corresponding to the terms $2s$ and $2S$, these being respectively the highest terms of the two independent serial systems of helium, namely the doublet system and the singlet system, and thus corresponding to two of the states of least excitation of the atom. This led to the conclusion that at the first critical potential the electron is displaced to the $2s$ orbit, and, at the second, to the $2S$ orbit, and hence it located the normal state of the helium atom in relation to the various excited states.

A spectroscopic location of the normal energy level has since been made by Lyman, who found a series of lines in the extreme ultra-violet spectrum of helium in the region of 600 A.U., whose frequency differences seemed to indicate that they belonged to the series $N - mP$, where N denotes the term corresponding to the normal state of the atom, and $2P$, $3P$, $4P$, etc., are terms of the singlet serial system. The limiting frequency of the series was calculated by adding the term $2P$ to the frequency of the first line of the series $N - 2P$, and the position of the normal energy level, in relation to the other states, was found. The ionisation potential calculated from this limiting frequency did not agree exactly with the ionisation potential found from critical potential experiments. The difference has since been shown to be due to the experimental difficulties involved in determining the correction for velocity of emission of electrons from the filament, etc.

In the gases neon and argon, the extreme ultra-violet series have not been determined, and the results of critical potential measurements are the only sources of information connecting the normal conditions of the atoms with the excited states corresponding to the terms of the arc spectra. We see, therefore, that critical potential measurements are of particular interest in the case of elements whose arc spectra have not been analysed into series, or whose absorption spectra have not been found.

So far, in considering possible excited states of atoms, and the information in regard to these obtainable from critical potential measurements, we have confined our attention to those excited states in which only one electron—the most loosely attached one—has been displaced from its normal position. Other types of excited atom can, however, be produced without much difficulty, by bombarding the atoms with electrons having somewhat larger energies. For instance, after one electron has been completely removed, a second electron within

the ionised atom may be displaced to another orbit, or may even be completely removed from the atom, leaving it with a double positive charge. The gap created by the disturbance of the second electron is always filled first in such circumstances, and the transitions of the electron which eventually fills the gap give rise to lines of the spark spectrum of the element. Now experiment has shown that the state of affairs necessary for the emission of such spark spectrum lines can be brought about by the simultaneous displacement of both electrons at one impact or by the occurrence of a second impact on an already ionised atom. If the first process occurs, the bombarding electron must have kinetic energy sufficient to bring about the simple ionisation of the atom, and then its further excitation, whereas if the second process occurs any single bombarding electron need only possess the larger of these two amounts of energy, generally that required for the further excitation of an already ionised atom. The second process, however, will only occur when a very dense bombarding electron stream is employed, for it depends upon the occurrence of impacts with atoms ionised by previous collisions, before these ionised atoms have become neutralised.

The conditions of the excitation of a particular line in the spark spectrum of helium, namely, λ 4686, have been the subject of several investigations. The helium spark spectrum is of particular interest because in the positive helium atom we have a simple system consisting of a nucleus and one electron, such as is found in the hydrogen atom alone among electrically neutral systems, and for such systems Bohr's theory can make very definite predictions. The testing of these predictions is, therefore, of obvious interest as providing evidence for or against the theory. The lines of the spectrum of atomic hydrogen are given by the expression $\nu = N\left(\frac{1}{n^2} - \frac{1}{m^2}\right)$ where n and m are integers and N is a constant. According to Bohr's theory the spectrum of the positively charged helium atom should be given to a close approximation by the relation $\nu = 4N\left(\frac{1}{n^2} - \frac{1}{m^2}\right)$. The line 4686 fits the relation $\nu = 4N\left(\frac{1}{3^2} - \frac{1}{4^2}\right)$, and according to Bohr's theory it should be possible to produce this line by supplying to the positive helium atom energy equivalent to $4N\left(\frac{1}{1^2} - \frac{1}{4^2}\right)$, this being the amount of energy required to remove the remaining electron from its normal orbit, given by 4 N, to the initial orbit involved in the emission of the line. The theoretical value of this quantity corresponds to that acquired

by an electron in falling through a potential difference of 50·8 volts, and the line has actually been observed at 51 volts, but not for lower potential differences. The agreement between these two values is evidence in favour of the theory.

The critical potentials associated with the displacement of a second electron, and with the production of the spark spectrum lines connected with it, have been investigated in many cases besides that of helium, notably in the case of argon and the metals of the alkaline earths, so that in these cases information is to hand about the energy relations of the excited states of atoms when two electrons are displaced from their normal orbits. Spark spectra corresponding to yet higher excited states of atoms have been analysed in some cases, of which Al^{++} and Si^{+++} may be mentioned. Critical potential measurements corresponding to such processes have not yet been made, though the results of such experiments would be of value in providing the connecting link between the normal states of the various types of ionised atom and the higher stages of their excitation, transitions between which give the known lines of the spark spectrum in question.

There is another type of abnormal atom which can be produced by electron bombardment, which we must consider briefly, namely, excited atoms in which, instead of the most loosely attached electron having been displaced, an electron from an inner more firmly bound group has been disturbed and brought to the periphery of the atom. The creation of a gap in an inner group of electrons, and its subsequent filling by an electron from one of the groups outside the one in question, gives us the X-ray spectrum of the element, the lines of the K series corresponding to the filling up of a gap in the innermost group by an electron which may come from any of the outer levels, the L series corresponding to the filling of a gap in the second innermost group, and so on for the M, N, etc., series in the case of the heavier elements in which many groups exist. If the group in which a gap is created does not lie very far in the interior of the atom the series of X-rays which can result from its subsequent filling will not be very "hard," and for most elements there will be at least one such set of radiations with wave-lengths which are too great to be measured by crystal methods. It is in connection with the determination of the amount of energy required for the creation of a gap in an electron group, whose subsequent filling gives only these soft X-rays, that critical potential measurements are, therefore, of most interest, and several investigations of the critical potentials for the production of soft X-rays from solids have been made during the last three years. The absorption limits of the various X-ray series correspond to the process of re-

moving an electron from the group in question to the boundary of the atom without giving the displaced electron any kinetic energy, and they therefore correspond to the terms in the optical spectrum. It is, perhaps, permissible to refer to the potential difference connected by the quantum relation $eV = h\nu$ with the frequency of the absorption limit of any electron group, as the ionisation potential for that group.

The more recent developments of Bohr's theory attribute the long periods in the periodic classification of the elements, and certain outstanding features in the physical properties of some of the elements in these periods, to the fact that at certain stages in these periods, as we pass from element to element in the order of increasing atomic number, the electrons which are added go to the enlargement of an inner electron group instead of assisting towards the completion of the only partially filled outermost group. A change in the constitution of a group of electrons in the interior of the atom will, according to Bohr's theory, reveal itself in the Moseley curves for the absorption limit corresponding to the electronic group concerned, by a decrease in the inclination of the line to the axis of atomic number. To test the theory in regard to the further development of inner electronic groups throughout the long periods of the periodic classification of the elements, it is, therefore, desirable that the Moseley curves for the absorption limits corresponding to the electronic groups concerned should be constructed. Practically no data are available from X-ray experiments regarding absorption limits of such comparatively low frequencies as those for the special regions of the particular electronic groups concerned, and the critical potential method is one of the possible ways in which it can be supplied in the case of solids.

According to the theory, the M electronic group is undergoing development in this way between the elements scandium and zinc. The theory distinguishes three sub-groups among the electrons constituting the M group, but this fact does not affect the point at issue at the moment. Critical potential measurements for seven successive elements in this region, from chromium to zinc, have been carried out in the laboratory of the Royal Holloway College, and these have shown that the change of slope in the Moseley curves, predicted by Bohr, does occur in the M curves in the region of these elements. In these experiments critical potentials at which an increased photoelectric effect occurs are determined, and each such st marks the attainment by the bombarding electrons of energy requisite for the displacement to the periphery of the atom of an electron from an electronic group or sub-group yet more firmly attached to the nucleus than those from which

preceding displacements have occurred. The identification of any critical stage with the creation of a gap in a definite electronic group is effected by extrapolating the curves for the various absorption limits, from the region of high atomic number, where reliable data are available from other sources, down to the region of atomic number under consideration. The success of experiments on the higher critical potentials of solids depends to a very great extent on the maintenance of a very high vacuum in the apparatus, for the ionisation of even slight traces of residual gas is sufficient to swamp the photoelectric currents. Several investigators have carried out researches on a few isolated elements, but so far the field of higher critical potentials has not received nearly as much attention as the region of critical potentials corresponding to the disturbance of the most loosely attached electrons in atoms.

In conclusion, a passing reference may be made to the existence of what are known as "metastable" states. These are excited conditions of atoms from which the atoms do not spontaneously revert to the normal state. The possibility of such conditions follows directly from the selection principles by which Bohr's theory has been elaborated, and which impose restrictions which prevent the occurrence of direct transitions between certain pairs of the possible states except in the presence of very strong electric fields. An encounter between an atom and a bombarding electron of sufficient energy seems to come within the category of exceptions, and certain of the excluded transitions occur in the one direction as the result of electron bombardment, which leave the atom in a state from which it cannot spontaneously revert to the normal condition by the reverse transition. Encounters of these excited atoms in metastable states, with atoms of other substances, or even with normal or excited atoms of the same element, may enable the atom to get back to the normal state, because the encounter may result in the production of a different excited state, of lower, or even slightly higher, energy value, from which the atom can revert directly. If a lower state of excitation results the energy difference between the metastable state and this lower state is converted into kinetic energy of translation of one of the colliding atoms, and conversely if the intermediate excited state is one of higher energy, the excess is acquired at the expense of kinetic energy of translation.

Such experimental evidence as is available of the period of duration of an atom in an excited state from which a direct return to the normal state is possible, seems to indicate a value of about 10^{-7} sec. for this quantity. Since the mechanism by which metastable atoms ultimately become normal is entirely different, it seems probable that the average period of duration

of a metastable state is of quite a different order, and that it may be made to vary with certain controllable factors. Now in any excited condition of an atom in which one of the outermost electrons is displaced, we have a system in which one electron is describing an orbit of definitely larger dimensions than the other electrons, and the system becomes in certain respects analogous to the alkali metal group of elements. Hence it is conceivable that metastable atoms may enter into chemical combinations into which the normal atoms do not enter. The band spectrum of helium has been attributed to disturbances of helium molecules produced by the union of pairs of metastable helium atoms, and evidence has recently been obtained of the production of compounds of helium with atoms of other elements.

We see, therefore, that the bombardment of atoms by electrons of approximately uniform energy of controllable magnitude, besides being a means of obtaining evidence with regard to the various energy levels in different atoms, may also prove to be an important factor in the production of new chemical compounds.

ON THE FORM AND FUNCTION OF THE GOLGI APPARATUS

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THE presence of the Golgi apparatus has now been established in practically all vertebrate tissues and in all the invertebrate tissues examined up to now, and also in many plant tissues. It was first discovered in 1898 by Golgi, after whom it is named, in the ganglion cells of Vertebrata, and also by Ballowitz in the epithelial cells of Descemet's membrane in the eye. Independently of these two workers, Holmgren, in 1899, using entirely different methods, demonstrated a similar apparatus in various nerve-cells.

Work was devoted from now on mainly to discovering its mode of development and its form. Among pioneer workers on the subject may be mentioned Negri ('01), Golgi and his school, Kopsch ('02), Bergen ('04), Cajal ('15), and Deineka ('10). These workers, using various animals and various tissues, demonstrated the presence of a Golgi apparatus in all vertebrate tissues.

The extent of these studies depended, and still depends, practically entirely upon two properties exhibited by the material of the Golgi apparatus, (1) That of reducing osmic acid to metallic osmium and becoming strongly blackened thereby. This action was discovered by Kopsch ('02), and usually requires impregnation in 2 per cent osmic acid for anything up to fourteen days and even more, some workers impregnating for a month. Kopsch's original method was simply to place small pieces of the material to be fixed in 2 per cent osmic acid in a glass-stoppered tube or bottle and to leave them in the dark for from eight to ten days, after which he washed them thoroughly for twenty-four hours in running tap-water, dehydrated them in the usual way, and, finally, sectioned them in paraffin. The sections he found could be mounted unstained, but often required some differentiation, the cytoplasm being blackened as well as the apparatus. This differentiation he carried out in turpentine, which decolorises the cytoplasm, fat, yolk, etc., but leaves the Golgi apparatus

blackened. The method has been modified by the addition of a preliminary fixation before osmication, in some fluid which may or may not contain osmic acid, but which is designed to obviate the shrinkage caused by the osmic acid. Chief among these modifications may be mentioned those of Weigl, Sjövall, and Kolatschev.

The second property of the Golgi apparatus material is that of becoming impregnated with various silver salts. This was the method utilised by Golgi when he first observed the apparatus. His original method was that of fixing for from 24 to 48 hours in a mixture of 8 parts of 2 to 2·5 per cent potassium bichromate, and 2 parts of 1 per cent osmic acid. Then the pieces were transferred directly to a ·75 per cent solution of silver nitrate and left for from 24 to 48 hours or more. After this they were placed in 80 to 90 per cent alcohol, which was changed until it remained clear. They were dehydrated in the usual way and mounted without a coverslip in thick xylol-damar.

This technique, which was originally designed for nerve-fibres, etc., has been modified considerably by workers such as Cajal and Da Fano. Da Fano's method for the Golgi apparatus consists of fixation in a mixture containing formalin and cobalt nitrate, for from 6 to 8 hours, and impregnation in 1·5 per cent silver nitrate for 24 or 48 hours. The object is then reduced in Cajal's hydroquinone mixture. It is dehydrated, embedded in paraffin, and sectioned. The sections are mounted on the slide and toned in acid gold chloride; excess silver is washed out in a solution of sodium hyposulphite, and they are finally mounted in balsam with a coverslip. The method requires practice and is somewhat capricious, but gives excellent results on the whole.

A brief summary of the chemical properties of the Golgi apparatus material may serve for comparison with those of other cell inclusions, such as mitochondria, with which it might be confused.

—	Golgi Apparatus.	Mitochondria.	Fat and Yolk.
Silver methods	Blackens	May be present not blackened	Dissolved away
Osmication methods	Blackens. Does decolorise easily in turps	Rarely blacken; if they do, may decolorise or may not in turps	Blacken, easily decolorised in turps
Mitochondrial methods	Does not stain	Stain	Yellow, brown or black
Non-mitochondrial fixatives	Destroyed	Destroyed	—

In addition, the Golgi apparatus can very rarely be stained or observed in the living cell, whereas the mitochondria may be stained by Janus Green, and have been observed moving about in the cell by various means. Hence one sees that the Gogi apparatus, by chemical methods may, with care, be distinguished fairly definitely from the other cell inclusions.

Morphologically the apparatus varies considerably in the different tissues and groups of animals in which it has been described. The typical form in vertebrate cells is that of a network of anastomosing threads and fibres, usually compact, or fairly so, and often definitely orientated in the cell. It is quite often found in close association with the archoplasm surrounding the centrosome, and several theories as to its function have been based upon this association. The actual form of the network varies in individual cells in a tissue, but the average form in any one tissue may be said to be constant and to differ in various details from the average form of any other tissue, and quite often to resemble quite closely the form of the network in the corresponding tissues of other species. This network has been described in all vertebrate gland cells, in most epithelial cells, in nerve cells, in bone marrow cells, and in other tissues, and consequently may be termed the general typical form. In the germ cells it is considerably modified and will be dealt with later.

When considering this form of the Golgi apparatus, and indeed all forms, it is as well to remember that the apparatus has rarely been observed in the living cell. The static picture obtained by present techniques is only the result of fixation of a fluid medium of varying consistency whose component parts are kept in position by various tensions. Hence considerable caution should be exercised in formulating any theories as to the relations between the varied forms exhibited by the apparatus in different tissues. Indeed a number of sceptics consider that the whole thing is nothing but an artefact. However, this view needs a great deal of substantiation, and is rather improbable in face of the facts of the almost universal occurrence of the body, its general conformity to two forms, occurring in vertebrates and invertebrates respectively, and also in face of its occurrence and general similarity under a variety of conditions of fixation.

The other form which is taken on by the Golgi apparatus is found chiefly in the Invertebrata ; and here it is almost as universal in its distribution as is the network in the Vertebrata. It consists of small, disperse, lamellar, vesicular or rod-like elements, often spread haphazard all over the cytoplasm of the cell in question. They have, however, been recorded by several authors as orientating themselves at various periods in the

history of the cell, and sometimes fusing together to form one single mass. There is some considerable dispute as to what is the true form of these small elements; some workers state that the little rods and scales found in most osmic preparations are the true state, others that the vesicles seen after many silver impregnations represent the apparatus, and that the rodlets are caused by the inferior fixation produced by osmic methods. However, this again is a problem that can only be directly solved by observation of the apparatus in vivo.

As stated above, the apparatus was first discovered by Golgi in various nerve cells, and he named it the "apparato reticulare interno." According to his description it is a closed network of fine fibres occupying the intermediate zone of the cell cytoplasm and separated by a distinct interval from the nucleus and from the surface of the cell. Towards the nucleus the net sends out fine fibres. Golgi expressed himself with great reserve concerning its function and nature, but was very positive that it had nothing to do with the neurofibrils, and that it was entirely intracellular. In a very few years this pioneer and his school demonstrated the apparatus in the tissues of all the more important organs and structures in the vertebrate body; in nerve, gland, kidney, liver, cartilage, striated muscle, epithelial cells, leucocytes, connective tissue, interstitial cells of the testis, etc. Meanwhile Kopsch ('02) had worked out his osmication method, and much work confirming the silver impregnation results was carried out by his school.

Apart from these two discoveries, Holmgren, and Nelis to a lesser degree, in 1899 had found in nerve cells fixed by various methods such as picric-sublimate, an endocellular net of juice canals, which remained clear in stained preparations. The cells were found to contain moderately fine canals of fairly uniform calibre, forming a dense network which extended round the nucleus and here and there communicated with the exterior by definite canals. From then on he published several accounts of the connection of these canals with processes penetrating the nerve cells from the capsular cells surrounding them. He claimed that this canalicular system, or "trophospongium," corresponded with the Golgi apparatus; and certainly a comparison of the superficial forms of the two organoids leaves little room for doubt. However, little credit has been given to the theory, chiefly on the grounds that none of the network fibres of the Golgi apparatus reach the surface of the cell. The other is regarded by many workers as an artefact. However, Bensley, Guillermond, and Mangenot have demonstrated in plant cells a similar canalicular system which develops into the vacuolar apparatus of the mature plant cell, so that one is forced to give very careful consideration to the problem.

During the first ten years at least, our knowledge as to the function of the internal reticular apparatus remained at a standstill. Several theories were proposed, such as that of Ballowitz, who suggested that it formed a framework for the centrosphere, and invented the name of " Zentrophormium " for it. It was, however, shown that the connection between it and the centrosphere was by no means permanent, and the theory was discredited. Most workers were very circumspect, and refused to commit themselves to any definite statement on the matter.

A wider study of the occurrence revealed, as has already been stated, that this net form was not universal. In the cells of invertebrates the Golgi apparatus occurred as small elements scattered throughout the cytoplasm, and not as a condensed, localised network. Finally Sjövall and Bowen have demonstrated another form of Golgi apparatus occurring in the male germ cells of insects and of many mammals, at least. Here the apparatus takes the form of a closed or open capsule whose walls impregnate strongly with osmic acid.

That these three forms represent one and the same category of organoid has been fairly conclusively shown by Hirschler in the development of *Limnæa stagnalis*. Here he traced the apparatus from the egg to the adult, and he found the following very interesting sequence. In the egg and in the blastomeres up to the gastrula stage the apparatus takes the form of little scales and closed vesicles—the diffuse stage. During the larval stages, however, this diffuse stage becomes transformed into the complex, localised type. In ganglion-, yolk-, and mesoderm-cells it takes the form of a closed or open capsule with an osmiophil membrane. In epithelial cells it is present as a group of little elements which gradually becomes transformed into a network, and so reaches the typical vertebrate appearance. Confirmatory evidence is afforded in cell division in many vertebrates, where the network breaks up into small vesicular elements, and these pass into the daughter cells, and there fuse together again and form a new network.

In recent years some considerable amount of direct and indirect evidence has been brought to show a strong connection between the Golgi apparatus and secretory activity on the part of the cells of various tissues. Dealing first with the indirect evidence : in 1915 Cajal had observed that during embryonal development the Golgi apparatus of the ectoderm cell always lies between the nucleus and the exterior, and even maintains this position when the ectoderm is reversed, as in the formation of the optic vesicles. It had also for some time been known that in cells of fixed secretory polarity, such as the acinus cells of the pancreas and salivary gland cells, the Golgi apparatus

always lies between the nucleus and the discharging pole of the cell, that is between the nucleus and the lumen of the gland. In 1922, however, Cowdry found that in the thyroid gland the apparatus normally migrates from one end of the cell to the other at times, so that it does not always lie between the nucleus and the lumen. At certain periods it migrates towards the lumen, but at others it flows round the nucleus and takes up a position at the far end of the cell with the nucleus between it and the follicular cavity. This migration may take place in whole follicles or only in single cells. A somewhat similar situation had already been described by Biondi (1911) in the choroid plexus, and by Golgi (1909) in mucus-secreting intestinal epithelial cells. Now we have reason to believe that the thyroid, unlike the pancreas and such similar glands, can, under certain obscure conditions, pour its secretion into the vascular network on its periphery, and hence, Cowdry has suggested that in the migration of the Golgi apparatus we have an indication of physiological reversals of polarity. This suggestion is supported by the work of Masson (1922) on the centrioles in malignant thyroid tumour cells. The centrioles, like the Golgi apparatus, lie usually between the nucleus and the lumen of the gland, but in the tumour cells they migrate to the other end of the cell, and lie between the nucleus and the peripheral vascular network, with an accompanying redistribution of colloid.

Important work in connection with the researches of Cowdry and Masson has been performed by Reiss (1922) on the anterior lobe cells of the cat's hypophysis. There are known to be three related types of cell in this organ, which, from being chromophobe, become basophile, then acidophile. Reiss investigated the position of the Golgi apparatus in these three types, by counterstaining the cytoplasm with various dyes after silver impregnation. He found that in the chromophobe cells it lies without any special orientation, thus coinciding with the theory of this cell being in a resting condition. In the basophil cell the apparatus always lies between the nucleus and the periphery of the cluster, and in the acidophil cell between the nucleus and the central area. Reiss claims to have seen all transitional stages between these three conditions, and interprets his observations as indicating a mechanism whereby the cells can first pour out a secretion towards the periphery and then turn about and pour a second, different secretion into the centre of the cluster.

Recent work by Da Fano has shown that in the mammary gland cells during pregnancy and lactation, the Golgi apparatus enlarges considerably and becomes more complicated, whereas, during the resting period between such physiological phenomena, the apparatus is quite small and simple. This same worker,

with many others, has shown also a general derangement of the normal conditions of the Golgi apparatus in various tumour cells and in various unhealthy tissues, leading often to a great hypertrophy of the network.

All these observations point at least to a definite relation between the Golgi apparatus and secretion ; but whether the apparatus takes an active part in actual secretory metabolism cannot be postulated on this evidence alone. However, more direct evidence has been collected recently, notably by Nassonov and Bowen, working independently ; Bowen's work, however, was evidently inspired by that of Nassonov.

Nassonov (1923) working on the pancreatic and interstitial testicular cells in a mouse, was able, by using Kolatschev's modification of the osmication method, to demonstrate a close relationship in position between the strands of the Golgi reticulum and the smallest secretory droplets demonstrable. These granules then lose their connection with reticulum and move towards the secretory pole of the cell, growing as they go ; and this, at first sight, would point to there being no physiological relation of the apparatus to secretion. On closer observation he found that each secretion granule carries with it a small osmiophil particle, which Nassonov interprets as a piece of the apparatus. He suggests three alternative theories to explain these phenomena : (1) Direct transformation of the apparatus itself into secretory products ; (2) That the Golgi reticulum plays the rôle of an intermediary between the ultimate secretory products and some other source which furnishes the raw materials and which may be the mitochondria ; (3) The secretion droplets may come from undifferentiated cytoplasm, out of which, in immediate contact with the Golgi reticulum, and under its influence, the droplets are synthesised. There he leaves the matter, except to state that the first theory is not supported by the facts observed.

Bowen in 1924 published work which was an elaboration of the recent researches of Nassonov, and which confirmed his results to a remarkable degree. Working also with Kolatschev's technique on the pancreatic and intestinal epithelial cells of Amphibia, he attempted to elucidate four main points which will be dealt with consecutively. They were firstly, the structure of the Golgi reticulum, and here he decided that nothing definite could be stated, the structure observed depending very largely upon the intensity of the impregnation. Secondly, he had in view the morphological changes occurring to the Golgi reticulum, which might be connected with the secretory activity of the cell. He found that the apparatus, from being a few irregular thin strands in the resting cell, increased greatly in size and became quite a large anastomosing

network, like a tubular sieve or basket, surrounding vast crowds of secretory droplets. As the goblet cell—for they were goblet cells on which he was working at the time—neared completion, the basket contracted, first at its basal end and then throughout its whole length, driving the droplets into the goblet portion of the cell and thence into the intestine. The basket was left behind in situ, and there was no evidence that the process did not repeat itself.

His third question for investigation was the relation of the secretory granules to the Golgi apparatus. He found that small vesicles which he could not identify categorically, but which were probably the smallest visible secretory droplets, were embedded on the strands of the reticulum, and that later they broke away. This original intimate connection between the two suggests that the secretory granules are a product of the network, and not bodies of foreign origin which have since come into relation with it. Hence he seems to favour Nassonov's first hypothesis of direct transformation of the Golgi-material.

Finally, he investigated the nature of the osmiophil granules observed by Nassonov, accompanying the free secretory granules, and, more particularly in pancreatic cells, he found each secretion droplet accompanied by a smaller osmiophil granule. Noticeably in the smaller granules, but also in the larger ones, the osmiophil granule was separated from the secretion droplet by a clear space. In heavily impregnated preparations the granule appeared as a crescent or cap over one side of the droplet, and could even be seen adhering to droplets that had not yet separated from the Golgi reticulum.

Here, before discussing Bowen's interpretation of these results, it will be necessary to outline his work previous to this. During 1922 he published an excellent series of papers on the behaviour of the cytoplasmic inclusions in the spermatogenesis of insects. As a result of his investigations, which covered a wide range of insects, and also some Amphibia, he has drawn up a very complete history of the Golgi apparatus in this type of cell. The apparatus commences as the diffuse type, rodlets and vesicles scattered throughout the cytoplasm. In the spermatocyte these vesicles commence to fuse together into one large body. This occurs almost universally, but the group Lepidoptera may be mentioned as a notable exception in which the droplets remain separate. The body formed by this general process of fusion consists of an inner sphere of material which impregnates lightly with osmium, and an outer rim which impregnates very strongly and which may be a closed hollow sphere, or which may be open at one point. The whole structure comes to lie just behind the nucleus in the angle between the latter and the "nebenkern," a body formed by

the fusion of the mitochondria. Then a vesicle or granule appears on the side of the sphere nearest to the nucleus, often pressed against the nuclear membrane. It grows in size until it is quite large, and then one of two things happens—either the vesicle and sphere move together to the head of the cell, or else the vesicle moves thither by itself. In either case sooner or later the two bodies part company, and the Golgi remnant passes back into the cytoplasm of the cell, and is sloughed off at metamorphosis. The fate of the vesicle is interesting from more points than one, but only one of these bears on the question at issue. The vesicle forms the acrosome of the spermatozoon, which is a body of still doubtful function, but which undoubtedly plays an important rôle in the process of fertilisation.

At first sight the function of the Golgi apparatus in the male germ cell seems to be specialised for a particular purpose, and to have no bearing upon the question of the relation of the reticulum to secretion. But if one accepts Bowen's theory, the phenomena exhibited by the apparatus in this latter type of cell bring strong support to the idea of this relation. Bowen, in concluding his 1924 paper on secretion, points out that attempts to trace the origin of secretory products have been frustrated because of the large numbers of granules present in the secretion cells examined. If one could find a cell producing only one or two secretory droplets, matters would be comparatively easy. Such a cell, he suggests, is the spermatozoon, and the secretion droplet the acrosomal vesicle.

Applying this to the gland cell, the secretory products arise on or within the Golgi apparatus ; their mode of origin is obscure, but probably resembles that of the acrosome. The granules become separated from the Golgi material and pass towards the secretory pole of the cell, their further growth being provided for by the contribution to each granule of a portion of the Golgi apparatus. The clear zone observed between secretory and Golgi granules may be interpreted as the light inner portion—or idiosome material—of the sperm Golgi apparatus, from which the acrosome develops. The casting off by the acrosome of the Golgi remnant may be paralleled in the gland cells by the phenomena observed by Heidenhain, who published a record of the throwing off by secretory granule of the " kapuze " or osmiophil granule.

Hence an overwhelming amount of very varied evidence is brought to prove very conclusively a physiological and morphological relation between the Golgi apparatus and the process of secretion. On the origin and evolution of the apparatus very little work has been done, but a second recent paper by Nassonov deserves consideration at some length. Working with

various fresh-water Protozoa, and with the latest Golgi apparatus techniques, principally that of Kolatschev, he found that the contractile vacuole of all the forms examined was picked out very clearly, its walls being impregnated very strongly with osmium, and so blackened. Other properties exhibited by the organ were: it is destroyed by the action of the so-called non-mitochondrial fixatives; does not stain selectively after mitochondrial fixatives; and it is invisible in the living cell owing to the possession of a refractive index very close to that of protoplasm. All these facts, he says, point to a lipoid constitution.

Morphologically the contractile vacuole or excretory apparatus varies considerably, from a simple, osmiophil-walled vesicle in lower forms, to a complicated system of vesicles and canals in the higher forms, such as Paramœcium. He proceeds to try and homologise the excretory apparatus of the Protozoa with the Golgi apparatus of the metazoan cell, pointing out their similar chemical compositions and emphasising their reaction to osmic acid. Morphologically he bases his argument on the similarity in form between the simple contractile vacuole of the lower forms in the Protozoa and the Invertebrate Golgi body. An important stage connecting the two is found in the Choanoflagellata and the choanocytes of Sponges. The Choanoflagellata possess a contractile vacuole, but the choanocytes do not, or at least one has only rarely been observed. On the other hand Hirschler (1914) has demonstrated a small Golgi vesicle towards the collar of the choanocyte in Spongilla, so that this may very conceivably be a bridge between the two structures. Functionally also there is a marked resemblance between these two organs in the Protozoa and Metazoa, for the secretory function of the Golgi apparatus is practically established, as is the excretory function of the contractile vacuole of the Protozoa. He postulates that the osmiophil wall of these bodies acts as a semipermeable membrane and also secretes inwards into the lumen.

On these grounds he suggests a primitive, one-celled ancestor from which the present-day Protozoa and Metazoa have evolved; and this ancestor possessed a lipoid-bearing excretory apparatus similar to that of the fresh-water Protozoa, and from which the excretory apparatus of Protozoa and the Golgi apparatus of Metozoa have been evolved. His work was concerned entirely with free-living, fresh-water Protozoa, and he suggests that the solution of the problem lies in the study of those forms where a contractile vacuole is not generally known to exist, and whose osmotic conditions of life are very similar, to wit, the salt-water and parasitic Protozoa. In view of this suggestion, it is interesting to note that we now have at least three records of a perfectly normal Golgi apparatus

in some five or six parasitic Protozoa; by Hirschler in 1914, by Gatenby and King in 1923, and by Joyet Lavergne in 1924, in each case the apparatus being of the disperse type.

Gatenby and King have objected to Nassonov's theory on the grounds that the similar chemical compositions of the organs in question is no criterion, and that the true Golgi apparatus of metazoan cells is usually associated with the centrosome and centrosphere, whereas no similar association has been observed in the Protozoa up to now. Also they point out that a normal Golgi apparatus has been found in several forms, as for instance, Adelea, Coccidium, Aggregata, Adelina, and Haplosporidium. However, since these are all parasitic forms and possess no contractile vacuole, this argument loses its force. If a Golgi apparatus and also a contractile vacuole had been demonstrated side by side in the same form, then this reasoning would have held, but as it stands it seems rather weak.

Our knowledge as to the function of the Golgi apparatus has made enormous strides during even the last two or three years, and it can now be stated practically with certainty that the function of this cell organ is of a secretory nature. How this secretion is carried out depends to a very large extent upon the form of the apparatus, and here Nassonov's recent work is useful in indicating a possible solution of the problem of its form. But considerable caution must be exercised in considering this theory, particularly because very little is known of the form of the apparatus in the living cell. It has been stated that the mitochondria have been observed moving freely through the region occupied by the Golgi network in the fixed cell, and that hence this network must be in a fluid condition. But a fluid network, if it could be conceived, could only be held in its position in a relatively mobile medium, such as the protoplasm is known to be, by an enormously complicated system of forces. The theory which suggests itself is that, what we are really dealing with is a system of vesicles each surrounded by a fluid membrane, which upon fixation breaks down and, lying closely apposed, as would necessarily be the case, to other similar membranes, it commences to run into them during the period of disturbance which must precede fixation. Particles of osmium or silver adsorbed on to the surfaces of these drops would naturally tend to collect in the angles between two adjacent droplets, and a very complicated network could quite easily be seen in the finished sections. Such a theory is supported by the fact of the evolution of the network from the simple vesicles of the lower Invertebrata and the Protozoa, and would certainly render Nassonov's suggestion as to the method of secretion into the lumen of the apparatus and thence into the cytoplasm more easy of acceptance.

THE FIELD FOR RESEARCH IN THE FLOUR-MILLING INDUSTRY

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The field for research in the milling industry is astonishingly wide and is by no means a purely chemical field : for its adequate exploration will be required the united efforts of the chemist, physicist, biochemist, plant and animal physiologist and geneticist, mathematician, chemical engineer, and the practical miller and baker. These are not arranged in any " order of merit," for I am by no means convinced that the chemist is the most useful of the series, while I should be the last to suggest that the miller is the least important. All are essential to the work of exploration, and all should work in the closest possible collaboration. As Dr. A. E. Humphries has remarked : " Please do not think for one moment that research means a chemist and nothing but a chemist. It goes very much further and very much better than that. Certainly, chemistry is important, but the chief value of a chemist to a flour-miller is that he has a trained scientific mind and approaches problems not merely with chemicals, but with the whole range of the application of science to flour-milling." " The idea (of inaugurating a Research Association for the Flour-milling Industry) has been that we should have a nucleus staff, properly trained. . . . But, in addition to that, we should have power to call in the aid of any person on any problem that may be presented to us, who may be the most suitable man for that particular job. We shall have the benefit of the best scientific men to apply their knowledge to any problem that may arise. This is the whole range of the application of science in the widest sense of the word."

All very true, and requiring neither comment nor further elaboration. And yet it may be asked : Why is it that the chemist plays such a predominant part in almost all scientific industrial research ?

The answer is not simply that there is a much larger supply of chemists than of other scientific workers to meet the demand of industrial concerns for such workers. There is a large and

constantly increasing demand for chemists as such in industry —a demand out of all proportion to that for other scientific workers such as physicists, biologists, and mathematicians. Indeed the number of scientific workers, other than chemists, employed in the laboratories of private firms in this country is practically negligible. The reason is, I think, that chemistry is that branch of science that deals with the properties and characteristics of things, substances, bodies—call them what you will—and the main, almost the sole, concern of most industries is the manufacture and distribution on a large scale of these same things, substances, bodies. In many instances it was the chemist alone who first discovered industrially useful things and worked out methods for their production and manufacture—first in the laboratory, later, on a larger scale, in the factory. Many of the more technical industries, such as the dye, chemical, incandescent lamp, and rare earth trades, owed their rapid development and even their localisation to the development of new processes and products and methods by individual chemists.

The engineering industries, on the other hand, belong to a different category : a machine or a motor-car is not a merely passive production like a dye, a suit of clothes, or an incandescent gas-mantle. The products of the engineering industries —dynamos, motor-cars, railway engines, and so forth—are essentially instruments for transforming one kind of energy into another, and the study of energy transformations is essentially the proper field of work of the physicist. An engineer *is* a physicist, and an engineering laboratory is a laboratory of physicists whose investigations have a direct bearing on the development of industry—not only on the engineering industries, but on all other industries in which machinery is employed—that is to say, upon *all* industries. In engineering works, the chemist—the works chemist—although important and indispensable, is very far behind the physicist in importance : his function here, mainly, is to exercise analytical control over the raw materials, the metals, paints, lubricating oils, etc., upon which the engineer works. In most other industries the works chemist exercises analytical control not merely over the raw materials but also over the intermediate and the finished products and over the by-products, while the physicist plays but a secondary rôle in this branch of work.

In all manufacturing concerns difficulties regarding materials, both raw materials and finished products, are constantly arising, —are in fact of almost hourly occurrence. Difficulties, too, in methods, and processes occur, taking an industry as a whole, in an unending stream, and such difficulties tend to hinder, if not actually to prevent, smooth working.

To overcome these difficulties some investigation is necessary. There are two ways of doing this: either empirically, by applying rule-of-thumb methods—a course that frequently leads to some temporary improvement, but that just as frequently allows of fresh outbreaks of similar trouble in the same or some other form. Or the investigation can be done scientifically, *i.e.* thoroughly, a method that involves an analysis of the causes of the trouble and hence leads, not to a merely temporary alleviation, but to an actual elimination of the trouble. The extent to which it is necessary, and even more the extent to which it is possible, to apply science to this end depends of course on the nature of the materials and the complexity of the manufacturing processes, but in all industries there is usually ample scope for a scientific staff and laboratory facilities to deal with such troubles.

Closely connected with this type of research, and indeed an essential part of it, is investigation into analytical methods and processes, the checking and improvement of old ones and the introduction of new and accurate ones, more suitable for the particular industrial conditions under consideration. In this connection I need only point out the present primitive methods of estimating the gluten content of flour; a reasonably accurate method of estimating starch in any kind of starchy product is still awaiting discovery, while even so apparently simple a matter as a moisture determination contains elements of uncertainty that call loudly for investigation.

Provision is being made in the Research Association's laboratories towards making a commencement in this type of research work. It is not intended that the Research Association should undertake ordinary routine analyses for its members: such work falls naturally within the sphere of activity of consulting and analytical chemists. But it is hoped that members will turn to the Research Association for help when they are up against difficulties or complaints respecting their processes or the quality of their raw materials or products. In such cases samples will be analysed and, when necessary, baking or other tests carried out and reports based upon such tests issued to members. In this way my colleagues and I hope not only to be helpful to you but at the same time to be helpful to ourselves. Because it is only by coping with the daily difficulties and urgent immediate needs of the practical man in the mill that we can get to grips, as it were, with your industry, and so find out for ourselves of what your problems really consist.

Apart from the study of experimental analytical methods, in the chemical sense, a good deal of investigation is required, and in some industries has been carried out, with the object of producing standard specifications connected with the purchase of

raw materials, as well as with the sale of finished products. In this kind of work America is well ahead of us: much work of this nature has been done there in other industries—little I believe has been done anywhere in the milling industry—and this work, mainly in the engineering trades, where a high degree of standardisation is possible and has indeed been achieved, has been made very largely of common value through the channels afforded by the American Society for Testing Materials. This society has had an enormous influence in American industry by determining the best methods of testing and the correct specifications to use for the purchase of materials. The membership of the society comprises not only many of the most prominent engineers and chemists, but also the leading manufacturing firms. It is, of course, sufficiently obvious that such work must be made of common value: secrecy in such a case would defeat the very object for which the work was undertaken.

Very little work along these lines has been carried out in the milling industry in this country, although the grading and standardising of wheats and flour is of obvious importance. To quote Dr. Humphries again, this time from the Report on the Quality of Red Fife Wheat grown successively in England for twenty-one years: "The appearance of a wheat is not a correct index of its quality as disclosed by baking tests, but it is the only test which can be applied readily by buyers in the stress of marketing." An experienced buyer was asked to appraise the relative merits of the Red Fife Wheat, grown at eleven different centres in England in 1922, upon their appearance only. His reports, arranged on a numerical basis by Dr. Humphries, showed no relation to the gluten content, nor to the baking quality of the flour as measured by the size of the loaf produced.

Here we have a whole field of research opened up, as it were, at a glance, and waiting only for the cultivator with sufficient ability, resource, patience, and perseverance to yield a considerable and a profitable harvest. But this problem is further complicated by the fact that it does not belong exclusively to the branch of work I am at present considering, *viz.* the grading, testing, and standardising of materials: its real solution depends on the study of quality in wheat and flour. And quality belongs to another section of the field which will be dealt with later.

Another illustration of this type of work may be of interest. Hard spring and winter wheats, *i.e.* those often called "flinty," are generally considered to be higher in protein content than soft, *i.e.* starchy, wheats. There is considerable evidence of a somewhat general nature to support this contention, which is widely accepted and indeed has been made considerable use of in the commercial grading of wheat, as well as in the

improvement of wheat by selection, because a high protein content was generally desired and hardness was thought to be a good and quick means of discrimination. A closer examination of the facts, however, casts some doubt on this generalisation. If the two characters, of hardness and high protein content, are genetically connected, there must be what the statistician calls a high *correlation coefficient* between them : if one character is the cause of the other, or if both are effects of one and the same cause and there are no disturbing factors, the correlation co-efficient should be theoretically equal to unity ; if there is no connection between the two factors, then the coefficient should theoretically be zero. These two values are never reached in any correlation between natural factors, but very high and very low values for the coefficient are frequently obtained. In the case under consideration, Roberts in 1923, working with 500 grains of each of 94 pure strains of wheat (each strain being the product of a single head), found the correlation between protein content and hardness (as measured by the weight required to crush the seed), to be for all practical purposes nil, *viz.* $0{\cdot}02 \pm 0{\cdot}003$. This conclusion runs counter to the generally accepted opinion and requires further investigation. A number of questions spring to mind. Is "crushability" a real test of hardness ? Is it not really a test for brittleness ? If so, what, if any, is the relation between brittleness and hardness, between "condition" and hardness ? To say the least, there is ample scope for standardisation here.

The part of the field so far outlined is pre-eminently a chemist's field, although as already indicated he very frequently has to use methods that are not strictly chemical in character. The importance of this type of work—or at least of analytical control over materials and products—is widely admitted and most works of any magnitude are equipped with analytical laboratories, although all too frequently both staff and equipment are on too modest a scale to admit of any real research work being carried out : too often the work done is of a purely routine character.

The type of work dealt with so far has been mainly in connection with the elimination of existing manufacturing troubles. During the course of such work many problems suggest themselves for investigation that do not fall precisely within the portion of the field so far described. This leads us to consider a second and larger part of the field of research : investigations connected with improvements both in products and processes—work that tends to lessen cost of production by effecting real improvements rather than by merely eliminating manufacturing troubles—and the discovery and exploitation of new products, or of new uses for old products and by-

products; for it is an old and true saying that the waste products of one industry are the raw materials of another.

This kind of work involves an intelligent appreciation of the trend of development of manufacture, and of the possible application of a product, together with a close study of the scientific features underlying the industry itself. It is not the kind of work that can as a rule be undertaken by ordinary analytical or "works" laboratories; it is altogether on a much broader basis, and involves fundamental development of the whole subject in which the industry is interested. A fundamental theory has to be worked out; the scientific principles underlying the processes of manufacture have to be sought for by means of slow and laborious investigations. The scope of this portion of the field is very wide: any single problem in it may require for its complete investigation the united efforts of the chemist, physicist, mathematician, biologist, and miller, and involve a great deal of work on pure theory and in the fundamental sciences associated with the industry. For the adequate direction of such work a man has to stand, as it were, with one foot in the factory and the other in the University, and it is difficult, as it is certainly undesirable, to draw a hard and fast line between such development work and so-called pure academic scientific research. Such work is necessarily slow and is full of disappointments and exasperations. Too often, perhaps, it has to be its own reward. Frequently the practical result is small or negligible. Just occasionally the result is so great as to more than pay for the ninety and nine investigations that came to nothing—in a practical sense. And one has to remember that if those ninety and nine had never been tried, the hundredth, and commercially successful one, would probably have shared the same fate.

The experience of the General Electric Company in America is interesting in this connection. They started a research department about 1903 at a cost of £1,000 a year. They now spend £100,000 a year, and employ a staff of 150 workers, and as a result of twenty years' experience of this laboratory they have come to the conclusion that "investigations in pure science lead almost inevitably, *in course of time*, to some commercial application, and increasing attention is therefore being paid to this phase of research work." The general policy of this company has been to employ highly trained investigators and particularly men of breadth of view, who are sufficiently visionary and, above all, appreciative of the enormous industrial possibilities latent in scientific discoveries. It is not without interest or significance that the results of much of the research work carried out in this company's laboratories, particularly that bearing the character of pure science, are widely published in the scientific press.

It is impossible in a short review such as this to do more than merely indicate some of the more typical groups of problems that await investigation in this vast region of a vast field of research in an industry of world-wide extent. The difficulty of drawing a hard and fast line between the kind of developmental research I am now considering and analytical control work on the one hand, and purely academic scientific research on the other, has already been pointed out. It is even more difficult to discriminate between problems that belong exclusively to the milling industry and those that do not. The milling industry is a kind of buffer industry: it is, on the one hand, a natural sequel to agriculture, and on the other a kind of prologue to the baking industry; many of the problems are of vital interest and importance to all three industries; many others cannot be dealt with at all adequately by either industry separately. In this connection I need only remind you of the twenty years of invaluable work of the Home Grown Wheat Committee of this Association. In 1900 the National Association circularised the most prominent agricultural societies throughout the country, pointing out that since 1890 "the tendency in many parts of the country has been towards a further deterioration in the milling quality of home-grown wheat, and we attribute this to the fact that farmers and seed-raisers pay more attention to the large quantity of straw and wheat produced by some of the newer varieties, whilst overlooking the fact that many of these are singularly destitute of gluten, and of other characteristics which are of the utmost value for milling purposes.

"Whilst advocating improvement in the quality of the grain, this Association does not lose sight of the fact that from the grower's point of view the question of the quantities of wheat and straw produced per acre is apparently more important than the quality of the grain, and that, therefore, if any attempt in the improvement in quality is to be made, it is essential that everything which tends to make up the grower's profits should receive full consideration."

The twenty years' work that followed—carried out in collaboration with Cambridge University, the Rothamsted Experimental Station, and many other institutions, and in some ways with such striking and unexpected success—is too familiar to all of you to require any further mention; but I would point out that it was to a large extent botanical and agricultural in character. Whilst of the greatest value to the millers—who indeed initiated and organised the work—the miller played a part not very dissimilar to that of the analytical control of products to which I have already referred. In this work the two industries were working together, each with a full consciousness of the needs and peculiar problems of the other. Work

such as this—wheat breeding for strength, yield, and disease-resistance, the study of certain cereal diseases, the study of the progressive development of the wheat-plant and the wheat-berry, should be the joint possession of the two industries, and success in this line of work the proud heritage of both.

In the study of the strength of flour the baker has vital interests, while it is unlikely that he alone, even in collaboration with the chemist, will solve such problems as the staling of bread, or control "ropiness" and other diseases without the help of the plant physiologist, the bacteriologist, the agriculturist, and the biochemist, working on the same or similar problems in the milling industry. Why the Research Association of British Flour-millers does not also include British Bakers is not easy to see; that it should do so can admit of as little doubt as that it should at least include all millers.

The staling of bread is an important technical and economic problem in the baking industry, but its solution lies in part at least with the miller. Jago maintains that "the character rather than the quantity of the gluten content" governs the quality of the bread, whilst Whymper quite reasonably insists that quality in bread should include keeping quality, and that undoubtedly the addition of extra gluten to flour-doughs results in bread of better keeping quality.

Viewed from one angle only, flour, dough, and bread are extraordinarily complicated colloidal systems, and to unravel that tangle will always be among the most urgent and pressing of our problems. When we clearly understand the real nature of the physico-chemical-colloidal system present in a flour, in a dough, and in a loaf, respectively, we shall have gone a long way towards learning the nature of strength in flour and quality in wheat. That time is a long way off. Colloid chemistry is still in early infancy—in very early infancy—and much work remains to be done in developing the infant science itself before its principles can be applied to the study of such complex systems as flours and doughs and breads.

Most of the work done so far has been in connection with materials and products. A no less important part of the field of work concerns itself with the actual manufacturing processes. A wise forethought on the part of the millers interested has resulted in the Research Association being run in conjunction with a full-sized demonstration mill—an existing mill, run on ordinary commercial lines for profit, but which will always be available for study and for large-scale experiments by the Research Association Staff. The mill is the real integrating factor in our work. It is there for our use and, even more important, for our inspiration; it will serve as a reminder of our common aims and objects, of our unity of outlook and of effort. But, above

all, it is there to be used, to afford us facilities for studying the technical processes for converting wheat into flour. Broadly speaking, these processes are apparently simple: storing, separating, washing, and conditioning the grain, crushing and grinding, and separating the various grades of products by means of sieves and air-currents. But are these processes really as simple as they appear? Do we really understand them? Do you millers understand them? Do the milling engineers understand them? Certainly there is much about them that the scientific worker, the chemical engineer, would like to know. There is much that he can learn from their study, and perhaps at least something he may be able to tell you—later on. That he has not been able to help you before has been due to lack of opportunity rather than unwillingness: you cannot study milling processes in technical colleges or university departments, while the practical miller has to run his mill as a means of livelihood rather than as a scientific study. If he wants to study at all, the market is probably of greater urgency to him than the plant.

I am loth to deal other than tentatively with this branch of the subject in the presence of practically all the millers in the country, and of some eminent milling engineers, particularly in view of my own slender connection with the industry. But on the scientific side I have been interested in other industries for over twelve years, and it frequently happens that experience gained in one industry is found to be directly applicable to another. Certainly in agriculture and the textile trades, the scientific study of *processes*, as distinct from *products*, has hardly begun. After more than a century of research in agriculture—and at the present day in no other industry is research carried out on so extensive, and so intensive, a scale—the scientific study of the *art* of *cultivation* has not really commenced. Not even a theory of the plough has been worked out: different types of ploughs have been, shall I say, evolved to suit different soils and farming conditions. Milling machinery, wonderfully efficient as much of it is, has been evolved in a similar manner since the inception of roller plant in the 'sixties and 'seventies. I do not think I am going too far in suggesting that milling machinery has not been *designed* on the basis of any real, intimate, and fundamental knowledge of the work it is called upon to perform, but rather just *built*. Improvements have invariably arisen through large-scale experimentation involving expensive changes in construction: rolls were first introduced—of different sizes and speeds, then differential speeds were tried; then again flutings of various shapes and sizes, at first parallel to, and then at various angles to, the axis of revolution—and so on, and so on.

All these were tried and those that " worked " most satisfactorily were retained. This of course is a crude and in many ways inaccurate, even unfair, account of the real state of affairs: in so far as well-known engineering principles are concerned, milling machinery may be highly efficient. I, at any rate, am not competent to criticise machinery as such. But I am trying to paint a picture in rough outline only, to emphasise a point of view, leaving it to the discussion that follows to fill in the finer details and to make the readjustments necessary for a correct perspective.

Consider, as a first illustration of what I mean, the simple storing of wheat, in large quantities, in bins and elevators. It is well known that such storage involves certain difficulties, particularly as regards the regulation of the moisture content and temperature of the wheat and the admission of moisture to the bins. If the wheat is damp, or moisture is not excluded, " heating " occurs in the bins and serious damage to the grain may result. How many millers and milling engineers know the cause of this heating, and, more pertinent still, how would, or how could, milling practice in this connection be altered if they did know? Here is a large group of problems that may bear directly on milling practice, and it is interesting that the cause of heating in damp vegetable matter (for the same problems occur in the storing of damp hay, and to some extent in the making of ensilage) is essentially a problem for the plant physiologist and the biochemist. Originally it was thought to be due to fermentation caused by bacteria. It has now been found to be due to the normal respiration of the grain. Oxygen is used up in the combustion of some of the ingredients of the grain, carbon dioxide is produced, and heat is given out in the process. Grain itself is a poor conductor of heat, a mass of loose grain a worse one, and the heat generated cannot get away—hence the practical problem of the marked increase in temperature when the grain is moist. Similar changes take place during the curing of tobacco and the ripening of fruits, and much of the work done on these problems is directly applicable to wheat. It is probable that the heating effect is due to the action of oxidising enzymes on carbohydrates (probably reducing sugars such as dextrose) in the presence of air and moisture at a suitable temperature, and it is considered that the main centre of the action is in the germ or embryo. Moisture above about 14 per cent greatly accelerates the change: thus an alteration in moisture content from 14 per cent to 17 per cent multiplies the effect 25 times, and it is interesting that soft white winter wheat appears to be affected very much more than hard spring wheat. The heat produced accelerates the evil: raising the temperature from 75° F. to 130° F.

hastens the process by more than sixty times. The rate falls again, almost as rapidly, above this temperature; but it is when these higher temperatures are attained that the mischief is done. Many practical points may be cleared up, and difficulties avoided, when more is known about this process. The shrinkage of grain when in transit by railway or by sea is due to the same cause and not, as with wool and cotton, merely to changes of "condition."

Consider another example from this field: the crushing of the wheat in the breaks.[1] Instead of experimenting with the plant to find out which type, or the conditions under which any given type, gives the best results, something may be learned by studying the *process* involved, analysing it scientifically into its underlying factors, finding out the conditions that must be fulfilled for the efficient carrying out of this process, and then seeing whether the plant fulfils those conditions. If it does not, there is room for improvement since the plant cannot be working at maximum efficiency.

There are two ways of dividing a mass up into smaller particles:

1. A force may be applied to it greater than its breaking strength, so that it is crushed or split; or,

2. The mass may be cut or torn apart.

The former is the method used in the smooth reduction rollers, the latter is the main operation performed by the breaks. An important practical point is the energy used in the operation of crushing, as this is causally connected with the magnitude of the fuel bill. Now Rittinger maintains that the work done in crushing a given weight of material, *e.g.* coal, from pieces two inches to pieces one inch in diameter, is half that required for converting the same weight of one-inch pieces into half-inch pieces, or, in general terms, the work done is proportional to the amount of fresh surface produced. Kick, on the other hand, insists that there is a logarithmic relation between the two factors—work done, and surface produced—in which case the same amount of work would be done in breaking up from two-inch to one-inch pieces as from one-inch to half-inch pieces. Neither of these relationships is correct, even for coal, and they are even more incorrect when applied to wheat. Probably Kick's law is less incorrect than Rittinger's when applied to the reductions, while in the breaks the reverse may be the case. Both laws are more incorrect for

[1] The *breaks* are the fluted rollers used in the preliminary crushing of the wheat whereby the endosperm is separated from the branny part of the wheat-berry. Further separation is effected by sieves and air-currents. The separated endosperm, known technically as semolina and middlings, is finally ground into flour by smooth *reduction rolls*.

coarse grinding than for fine, but this may possibly be due to the presence of incipient cracks in the fine pieces, caused by earlier grindings. Then again, different wheats will show strikingly different variations from these laws, the more so in the breaks than in the reductions. For the break system is not a mere crushing or grinding of a material homogeneous throughout ; the result to be attained is a separation of endosperm from bran, and it has been aptly said (I believe the simile is due to Dr. Humphries) that whereas the endosperm of some wheats will separate like nuts from their shells, in other cases it is more like separating an orange from its skin. The relation between work done and the result obtained will not be the same in the two cases. It is a difficult matter to analyse such complicated cases, but the attempt should be made. Much fundamental work will have to be done in explaining modern practice in such ways, for modern practice is so very complicated, and one has always to remember that until we fully understand what we are doing and why, we cannot satisfactorily effect real improvements.

One other example of this kind of analytical work I should like to mention. After going through the breaks, a partial separation of bran from endosperm is effected by sieves and/or air-currents, and there seems to be a general impression among millers that separation by air-currents is due to a difference in specific gravity between the various particles. Kozmin, in his book on Flour Milling, adopts the same explanation, but I am convinced that it is incorrect. The difference in specific gravity between endosperm and bran is not great, and its effect on the separation must be negligible in comparison with that due to the difference in surface area between the particles. It is the latter that is the determining factor in this type of separation : the velocity of a particle falling through the air is given by the expression

$$V = K\sqrt{\frac{D.S}{s}}$$

where D is the diameter of the particle, and S and *s* are the specific gravities of the particle and of the air, respectively. From this it is obvious that size rather than specific gravity must be the determining factor. Separation by specific gravity is used on a large scale in some industries, *e.g.* in separating powdered minerals from one another by sedimentation through water; but it is not a good method and only works at all when the particles are of nearly the same size.

The types of problem to be attacked in this section of the field are of almost infinite variety. I have cited only a few, purely as illustrative examples, and have deliberately chosen

some of the less prominent ones. Many more could be discussed if time allowed, and no doubt many more will be brought forward in the discussion that is to follow. Many in connection with conditioning were discussed at last year's Convention and are doubtless too fresh in your minds to require more than this passing reference. The theory of sifting, the reduction of middlings, over-milling—all supply their problems and difficulties. The bleaching of flour and the use of improvers belong both to this section of the field and to the more academic one I have already discussed.

One more branch of the field of research remains to be discussed—a branch the importance of which can hardly be over-estimated, although, in point of fact, it is very rarely appreciated at its true value. When a scientific worker takes up the investigation of any particular problem, he must read all that has been written in the scientific and technical press that bears on the problem. This involves a systematic search of an immense literature—a piece of research in itself, that takes much time and labour—unless, which is very rarely the case, a highly organised information service is at his disposal.

Such an information service is an indispensable requirement of modern industrial research, especially in a field such as ours, which embraces so many branches of scientific and technical knowledge.

As time goes on, the quantity of scientific matter published becomes greater and greater. Information bearing upon our work may appear at any time in one or more of a large number of periodicals and journals (both technical and purely scientific), in the extensive field of Patent Literature, in Reports and Pamphlets, and in other ways.

The fact that a discovery, or piece of information, has appeared in the literature is of no value in itself. It has happened more than once in the past that matter of great importance to scientific progress has been overlooked—buried, as it were, in more or less obscure periodicals. Since this has occurred with quite important matters, it may very easily happen in the case of minor facts and out-of-the-way statements, which, though apparently unimportant, may at any time have a vital bearing on subsequent work.

There is only one way of overcoming this difficulty, and that is, thorough and systematic abstracting and indexing.

The subject of the indexing of technical information is one of some complexity. The aim is to make every point which has appeared in the literature, or is otherwise known, instantly available in any connection in which it may be of use. Such a scheme must depend for success on methods which have been carefully planned and carried out, and a scheme of

this nature is being developed in the Research Association's Library.

A mere enumeration of the more obvious groups of problems is almost bewildering: we have the origin and classification of wheat; wheat-breeding for strength, crop-yield, and disease resistance; the progressive development of the plant and of the grain; the study of certain cereal diseases; the effect of premature freezing and of sprouting on the milling and baking qualities of the resulting grain; grain storage, especially with regard to respiration; the grading and standardising of grain and cereal products; the conditioning of wheat; the chemistry and physics of the actual processes of flour-milling; the bleaching of flour; the use of improvers; the study of enzyme phenomena, especially those occasioned by zymase, diastase, phytase, and the proteases; the biological and biochemical study of the yeasts; the chemical, physical, and biological changes involved in the various processes of doughing and baking; the diseases and the staling of bread; the consideration of cereals and cereal products in human and animal nutrition, including the study of vitamines.

Truly a bewildering array, and sufficient in itself to disabuse the lay mind of the idea that milling is a simple process of extracting flour from wheat. Things have changed since the historic miller, who lived beside the Dee, spent his days in song, whilst the mill-stone helped by the river did his work for him. The modern up-to-date miller no longer stands, as one writer puts it, "with one hand in the hopper, and the other in the bag." He really is a manufacturer. Flour is essentially a manufactured article, and its production involves problems and difficulties that must be dealt with and overcome. In the time at my disposal, I have been able to give only a very slender sketch of the field for research in your industry.

The preparation of this paper has left me with one dominant impression—an impression that is outstanding above all others, and one that must, I think, occur to anyone taking even a casual glance at the field for research in your industry. It is an impression, moreover, that is bound to colour and to dominate —or at any rate, should dominate—all our research.

It is this: I have been greatly struck by the smallness of the field for research that belongs exclusively to the millers, in comparison with the huge extent of the field that the milling industry shares with agriculture, with baking, and with the physical and biological sciences.

You millers know better than I do the problems you are up against in your mills. I leave it to your imagination, aided a trifle perhaps by this slight sketch, to give you an idea of the magnitude of the whole estate.

THOMAS BEDDOES, M.D.: A NEGLECTED CHEMIST

BY T. W. JONES, B.Sc.

IN John Davy's Life of his brother there is a description of a Doctor Thomas Beddoes. By profession a doctor, and a chemist and scholar by inclination, Beddoes had founded at Clifton a "Medical Pneumatic Institution," where he investigated the medicinal value of the newly discovered "pneumatic fluids." Humphry Davy was the first medical superintendent of this Institution, and it is from his first letter home that we take our description of the doctor. "Dr. Beddoes is . . . uncommonly short and fat, with little elegance of manner and nothing characteristic *externally* of genius or science." The lack of flattery was probably occasioned by Beddoes' manner. This was notoriously abrupt upon first acquaintance, but could be very charming to those he really liked, and that he speedily showed this side to Davy is evident from a later letter. "I am quite naturalised into the family, and I love them the more I know them," he writes to his mother after a few weeks.

In the main, Davy's first impression was true. Beddoes, though possessed of a considerable originality, an originality that often bordered on the eccentric, was not a genius. He belonged, rather, to that order of men, that is to be found in every age, who hover somewhere between the great and the average. Acquiring in their own times if not fame, at least something akin—a sort of general recognition by their contemporaries as possessing unusual talents and ideas in advance of accepted thought—their names are gradually lost to common knowledge, posterity usually fails to ratify the contemporary verdict, and their reputation seldom survives them by many years. In the first decade of the last century Beddoes had a fame as a chemist and physician that extended beyond his own profession and neighbourhood. He was acquainted with a large and comprehensive amount of the knowledge of his day, and himself accomplished a prodigious amount of work, introduced Mayow, Scheele, Bergman, and Davy to the English scientific public, and yet failed to produce any work of first-

THOMAS BEDDOES, M.D., 1760–1808.

Ætat 36.

rate importance to familiarise his name to the modern student. The only facts generally remembered of him to-day are his connection with Davy, and that his son, Thomas Lovell Beddoes, left behind some fragmentary verse of unusual merit.

Born of middle-class parents, at Shifnal in Shropshire, in 1760, he was educated at Bridgnorth Grammar School, and entered Pembroke College, Oxford, in 1776. Here, in addition to his classical studies for the B.A., which he took after his first three years' residence, he taught himself French, Italian, and German: modern languages that formed no part of the classic curriculum. Attracted to chemistry, botany, and mineralogy by their connection with the practice of medicine, the profession he had already decided to enter, he soon gained more than a smattering of each. By the time he took his B.A. he had acquired considerable manipulative skill in pneumatic chemistry and was perfectly conversant with the work of Priestley, Cavendish, Lavoisier, and Scheele. An amount of study surely prodigious for a graduate of 19 to have accomplished in three years. In 1779 he took up the study of medicine proper, and went in 1781 to study anatomy in London under John Sheldon, the illustrious pupil of William Hunter, and at that time lecturing and carrying on research in a private theatre in Great Queen Street. About this time Beddoes commenced his first literary work, the translation of the "Dissertations" of Spallanzani, whose advances in surgical method were at that time unknown in England. In 1783 he took his M.A., Oxford, and then studied at the Medical School in Edinburgh until the summer of 1787, visiting Oxford in December 1786 to take his M.D.

In the autumn of 1787 he made a short tour of the Continent and formed a friendship with Guyton de Morveau and visited, among other chemists of note, Lavoisier, with whom it is said he corresponded on his return. The writer, however, has been unable to trace these letters. In January 1788 he was offered and accepted the post of Reader in Chemistry at Oxford (Oxford had no Chair of Chemistry until some years later), a post that at that time carried with it no salary other than was obtainable from the fees of students who voluntarily attended the lectures. That this state of affairs was finally altered was largely the work of Beddoes, whose representations to the authorities resulted in a change for the better, although not until some time after he had left the University. He himself can, however, have had no cause to complain, for it is reported that his lectures were crowded, and such was the enthusiasm with which he infected his students that philosophical research was undertaken on a scale hitherto unprecedented at Oxford. Science as a whole was unusually popular during his term of office as

Reader; in addition to Thomson's lectures on anatomy and mineralogy, John Sibthorp, Sherardian professor of Botany and a founder of the Linnæan Society, was lecturing during the interval between his Greek expeditions, and writing his Flora Oxoniensis.

Well liked by his colleagues, his natural cheerfulness and good humour found him many friends among his students, among whom was his life-long friend Davies Giddy, afterwards Sir Davies Gilbert, who succeeded Davy in the Presidential Chair of the Royal Society, and in whose guardianship he left his son Thomas Lovell Beddoes. His friendships outside the University increased during these few years and included men of considerable scientific achievement, such as James Keir, with whom he regularly stayed during the vacations, James Watt, Boulton, the Wedgewoods and Erasmus Darwin. They all found a common ground in their interest in chemistry and mineralogy; and with these friends and those at Oxford Beddoes passed the happiest years of his life. The tranquillity of a University suited his habits and temperament better than the hurly-burly of medical practice. In choosing the medical profession he had mistaken his vocation; his true bent was rather to pure science than medicine, for it is significant that his natural skill in manipulative experiment should have taken the chemical rather than the anatomical turn, seeing that in Sheldon he had a master second to none of his contemporaries, save perhaps the Hunters, in anatomical preparations. It would therefore have been more than easy for him to have taken up anatomy rather than chemistry, had he wished. He was the born savant and scholar rather than the doctor. He might, and in all probability would, have found his true vocation at Oxford, and possibly have contributed something of lasting importance to chemistry, had he not dabbled in politics.

Political events in France at this period had repercussions in England, the violence of which it is difficult to realise at this distance. In the early months of the Revolution, whilst theory still largely outweighed practice, English converts to the magic of the slogan "Liberté, Egalité, Fraternité," were not few, and Beddoes, though able to weigh dispassionately the claims of a chemical theory, could not so examine one having a sentimental appeal. Always impetuous, he rushed into a course that he afterwards repented, and during the Long Vacation of 1792 circulated in his home neighbourhood a leaflet in which he expressed his opinions with more force than discretion. Its circulation though small was effective, and on his return for the Michaelmas Term he found himself decidedly cold-shouldered by his colleagues. This, coupled with a touch of that "wander-

lust " that afterwards cursed his son, decided him to put in his resignation, and he made plans to revisit the Continent.

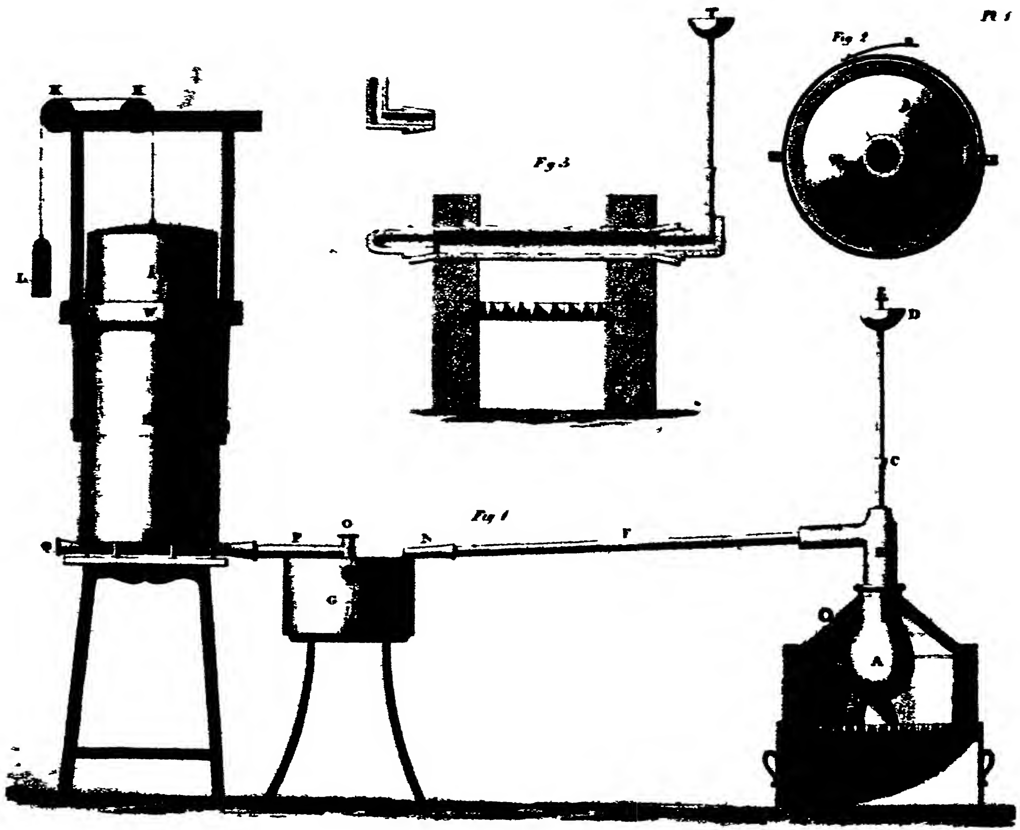
By the time he left the University at Christmas 1792 he had published several chemical and medical works. In addition to the translation of Spallanzani's " Dissertations " already mentioned, he translated Bergman's Elective Attractions and Scheele's Chemical Essays, besides editing and annotating Cullen's Translation of Bergman's Physical and Chemical Essays. (His translation of Scheele was republished complete in 1901 by a well-known publishing house, with an additional biographical appreciation of Scheele, a matter already covered by Beddoes' preface, which with his notes was left intact. No notice of any kind was made of Beddoes.) In 1790 he published a digest of Mayow's chemical work and opinions under the title of *Chemical Experiments and Opinions extracted from a Work published in the Last Century.* These had been entirely forgotten until Beddoes published this work, and it created no little surprise in the chemical world that the new theories of Lavoisier had been stated so long previously. Black, it is said, " lifted up his hands in complete astonishment " ; Fourcroy set about belittling Beddoes' work, and there are several papers on the subject in *Nicholson's Journal*, both from Beddoes' pen and those of others. This year marks Beddoes' conversion from an ardent phlogistonist to the school of Lavoisier. In his translations of Scheele and Bergman it is obvious from his notes that he then accepted the doctrine preached by Stahl ; in the preface to Scheele's Essays (1786) he says : " I know not whether it can now be necessary to put any reader on his guard against the notions concerning the composition of *heat* which run through these essays. . . . Yet erroneous as the theory certainly is, it required for its formation not common talents." In the preface to Mayow (1790) he says, " He (Mayow) has clearly presented the notion of phlogiston, which rendered the name of Stahl so celebrated." This was in February 1790, and by November 1791 he alludes in a letter to Keir to his phlogistic faith as a thing already forgotten. His later chemical writings, which we shall notice in their order, though more valuable than these, are very small and have, possibly on that account, been given much less notice by writers.

Of his other chemical activities at Oxford we have only the barest allusions in later writings ; no notes of lectures or experiments have been preserved. It is, however, certain that during 1792 he had conceived the scheme of investigating the medicinal values of the several newly discovered gases. There was considerable speculation about at the time concerning their possible value in such diseases as phthisis, and to determine the

facts seemed to Beddoes a valuable work for humanity. That others were of his opinion is shown by the fact that, before setting up in practice at Clifton, he and three friends had subscribed two hundred pounds each to the purchase of apparatus. The opposition aroused in Clifton when it was learnt that noxious gases would be let loose by the new doctor was considerable, and only quietened by the influential intervention of R. L. Edgeworth, the father of the Irish novelist, Maria Edgeworth, who had met Beddoes previously and was spending one of his numerous visits to England at Clifton in the summer of 1793.

With this backing, for Edgeworth possessed considerable influence although generally disliked for his self-conceit, matters became much easer, and Beddoes built up a large and fashionable practice in a very short time. We find him writing to Darwin as soon as 1795 that his literary pursuits for that morning "were interrupted by a concourse of patients," for Beddoes could no more keep from writing than a duck from swimming. His first medical work attracted medical attention, and as it was published in January 1793 his fame was known at Clifton before he arrived, and probably aided his rapid climb. This work, *Observations on the Nature and Cure of Calculus, Sea-Scurvy, Catarrh and Fever,* though to-day of little value, deserves notice for the soundness of some of his views on scurvy. Among other courses he recommends the eating of fresh raw meat. Those who have read Stefannson's *Friendly Arctic* will recollect that during his exploration work upon the ice he and his companions kept perfectly free from scurvy upon a diet of fresh seal meat alone, and on one occasion he cured his two companions of the disease (brought on by foolishly eating biscuits and tinned foods), by feeding them on fresh animal meat. Beddoes' observations on catarrh and fever were novel and in some places far-fetched ; his advocacy of the use of ice in fever is to-day a matter of common prescription, though at that time it was considered a dangerous innovation.

Besides the work of his growing practice, the doctor continued his pneumatic experiments and literary activities. The generating apparatus used proved unsatisfactory, and James Watt designed and built one of better design and capacity. The results of this work appeared in 1794 under the title of, *Considerations on the Medicinal Use of Factitious Airs and the Manner of Obtaining them in large Quantities* ; this was in two parts, the first by Beddoes and the second by James Watt. In the preface Beddoes developed his idea of a small trial of the gases into a more ambitious scheme for the establishment of a Medical Pneumatic Institution. In this the "airs" would be administered by a salaried medical superintendent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under the supervision of a physician. Subscriptions were invited for a sum of £3,000—£4,000, " a small sum compared with the possible benefit to humanity at large." On the other hand, if the results proved negative, the public would have the satisfaction that a possible weapon against disease had not been neglected. Part I contained details of experiments already made by Beddoes upon animals and patients, and a few carried out by others ; for he was not the only practitioner who had formed great hopes of the medical value of Priestley's and Lavoisier's gases. Part II contained experimental directions and drawings of the apparatus " for the benefit of others who might wish to make a trial of the airs." The apparatus was well designed, and, except for the coal-fire furnace for heating the solids, compares favourably with modern workmanship : it consists of retorts of various shapes (for the different methods of preparing the gases) resting upon the bars of the furnaces, and connecting to a cold water cooling device, through which the evolved gases passed, on their way to the gas holder. This gas-holder was designed on the same principle as the modern gasometer, namely, an iron cylinder closed at one end and counter-poised by weight and pulley ; the open end of the cylinder was sealed with a water or mercury seal. The gases were usually allowed to stand in the holder for some time before use to allow any acid or other particles to settle. Their use may well be given in Watt's own words. " In regard to the manner of breathing these medicinal airs, I think it will be done best from bags of some flexible and light substance, such as very thin leather waxed, or oiled silk (the identical bags mentioned by Davy in his *Researches upon Nitrous Oxide*). If a small tube be inserted into the mouth of the bag, the air may be pressed out opposite the patient's mouth, in cases where they are too weak to make extraordinary exertions of the lungs, or rooms may be filled with the proper mixture of airs." That some invalids might be nauseated by the smell of the oiled silk he recognised, and advised that before the bags were made up the cut silk should be laid in alternate layers with finely powdered charcoal, which would be found effectively to absorb the smell.

The gases used were, oxygen, prepared by heating powdered pyrolusite or pyrolusite and concentrated vitriol, hydrogen from water dropped on red-hot iron, carbon dioxide, or " fixed air " from chalk, " hydrocarbonate " or water gas made by dropping water upon red-hot charcoal. The experiments upon animals were made in the usual manner adopted by Priestley and others. Priestley's contention that hydrogen was as lethal as carbon dioxide Beddoes early disproved, and he quotes that " Scheele could make twenty inspirations of it without inconvenience."

Many cases are cited in which a percentage of hydrogen in the atmosphere to "modify" it gave great relief in breathing to phthisical patients. The value of carbon dioxide as a modifier of the atmosphere is stressed, and instances of its beneficial use cited. Watt's speculations on its effects are curious. "As fixed air is a saturated solution of charcoal in oxygen, it is not possible that the lungs can decompose it; we should therefore only look to its effects as an antiseptic: it may have some effect by merely excluding the oxygen of the atmosphere. . . . I think it will have most beneficial effects in cases of putrescent tendency." The effects of these gases upon animal organs were fully examined after asphyxiation by Beddoes and others, and the results compared with the appearances of the same organs in controls of the same litter. Diluted hydrogen was found useful in relieving asthma, and "hydrocarbonate" in phthisis! Numerous cases are given with the fullest detail.

During the visit of Edgeworth and his family, already mentioned, Beddoes became strongly attached to Maria's sister Anna, who from all accounts was a very charming and witty woman, with an optimistic cheerfulness that matched his own. When the end came to their holiday in England, Maria writes that their preparations for departure "seemed to particularly grieve and distress Dr. Beddoes." The parting was accordingly delayed that the two might know each other better, and in April 1794 they were married at Edgeworthstown, Ireland. From what letters there are, it is evident that they were very happy together; Mrs. Beddoes charmed all she met, and it was largely due to her that the house at Clifton became the meeting-place of such men as Southey, Coleridge, the Barbaulds and others of literary note, besides Beddoes' scientific friends. It was in fact a spot where the sciences and literature met without the discordance that too often mars their meeting. Among scholars Beddoes could easily hold his own, and among poets had no need to take a back seat. While at Oxford he had produced a long narrative poem (of doubtful merit) upon Alexander's Expedition to the Indian Ocean, and although this were best forgotten, some of his later work contains "purple patches" that pleasantly anticipate the delightful fragments left by his son.

During the next few years Beddoes' scientific writings continued in a rapid stream; collections of opinions and experimental details of the use of gases in medicine were also made and published under his editorship, the most important being the third, fourth, and fifth parts of the *Considerations* . . ., of which several editions were called for. In 1798 the Medical Pneumatic Institute was opened through the generosity of

Thomas Wedgewood, and Davy, introduced to Beddoes by Davies Giddy, was appointed Medical Superintendent. Almost immediately after the opening appeared a collection of scientific papers by various hands under the title *Contributions to Medical and Physical Knowledge from the West of England*, and edited by Beddoes. This deserves some notice, for not only does it contain Davy's first published work, some immature speculations upon heat and light (which, by the way, Davy afterwards confessed he could never see, but he blushed for his early ignorance), but also a classification of chemical substances according to their principles, by Beddoes.

This scheme of classification was first printed in the spring of 1798 to illustrate a course of popular chemical lectures given by the doctor in Clifton. In it, bodies are divided into four classes according to their "principles" or rather their reactions towards oxygen. Beddoes, always impetuous, had been badly bitten by Davy's "phosoxygen" theory without stopping to inquire too closely into it, and he accordingly devoted the first two classes of the four to light and oxygen respectively. All other bodies then fall into the other two classes. In the first are "Philoxygena," those able to combine with oxygen. In this are placed such elements as hydrogen, nitrogen, carbon, sulphur, phosphorus, and the metals. Class IV, the "Misoxygena" contain those that cannot be made to combine with oxygen; the alkalies and alkaline earths together with silex or silica. He appears to have been the first to recognise the similarity between the alkalies and alkaline earths, for he classifies them together under a general heading of alkalies, an advance upon Black's theory. Another theory that he puts clearly, although only as a speculation, is that the alkalies may be unable to combine with oxygen because of "some peculiarity of the union of their elements or because they *are already combined with oxygen*." This was certainly a speculation in advance of contemporary thought and one that foreshadowed his pupil's (Davy) future decomposition of the alkalies. Another theory yet more advanced is (p. 223), "The existence of iron in such variety of plants and animals; and of manganese in some plants, suggests an opinion that these metals are compounded by the organic powers."

It is high time we returned to the fortunes of the "Pneumatic Institution." It was no part of Beddoes' policy that the pneumatic remedies should be applied indiscriminately, their use was to be strictly limited to the few cases that obviously could not be alleviated or cured by ordinary medicines, which were to be applied wherever possible. Accordingly Davy had plenty of time in which to experiment with gases as yet untried, and his researches at the Institute fill more than one volume of

his collected works. Further experiments were undertaken on the gases already mentioned, experiments so thorough that it is remarkable that he should have survived them and left a collected edition at all. On one occasion he inhaled pure water gas. Luckily he had sufficient strength left to disengage the mouthpiece before losing consciousness.

By May 1799 Davy and Beddoes had made several experiments upon Priestley's "Dephlogisticated nitrous air" (nitrous oxide), a gas which a Dr. Mitchell had defined as the "principle of contagion, oxide of septon," and therefore an attractive material to anyone investigating the physiological action of gases, especially one who, like Beddoes, preferred to make his own physiological theories rather than to accept those of others. The gas was first prepared in the early part of March 1799, from the interaction of nitric acid and zinc. By April 11, when Davy first breathed the pure gas, work had progressed so far that this method had been discarded for the preparation from heated ammonium nitrate. On April 16, 1799, Davy for the first time inhaled sufficient of the gas to experience its peculiar properties. From that date the chemical and physiological investigation of the gas proceeded apace, and in June 1800 Davy published his complete work upon it under the title *Researches, Chemical and Philosophical, chiefly concerning Nitrous Oxide*, which includes all the chemical work. A large proportion of the physiological investigation is to be found in Beddoes' *Notice of some Observations made at the Pneumatic Institution*, 1799. In this he announces with some complacency these first-fruits of an institution "for pursuing the connection between the properties of elastic fluids and the conditions of life," and hopes that with the various gases already known "we should now have at our disposal an infinite series of powers" to combat disease. Although he never for an instant lost sight of the physiological aim of the work, and although he noted that sufficient of the gas could induce coma, and Davy that it could relieve toothache, yet curiously enough neither suspected its properties as an anæsthetic.

The novel effects of the gas excited a good deal of attention: all Beddoes' friends flocked to experience them, and in the "Notice" above mentioned there are details of its effects upon Coleridge, Southey, Mrs. Barbauld, the Wedgewoods, Boulton, Watt, and some of the Edgeworth family.

In addition to this work, experiments were carried out upon the chemical and physiological effects of galvanism, a subject that had greatly occupied Beddoes' thoughts since 1792. Davy, under Beddoes' direction constructed a large galvanic pile to supplement the effects of those already constructed by the doctor, and several papers on the subject were contributed by

Davy to *Nicholson's Journal* whilst he was at Clifton. For all this work, including that upon nitrous oxide, little credit has been given to Beddoes, who after all appears, from sundry hints in Davy's papers and elsewhere, to have acted the part of the modern director of a laboratory who, to-day, claims his due share of the honours attracted by the researches of his pupils. Beddoes' wide familiarity with the subject of pneumatic chemistry, both practical and bibliographical, must have been the guide and impetus to Davy's work. The actual work of experiment, it is true, was probably largely left to the pupil's discretion, for—what with a large practice and his literary work, beside the superintendence of a second section of the Institution which he had put in the charge of a Mr. King, who there investigated the purely physiological properties of gases and medicines,—Beddoes must have had his hands and thoughts fully occupied. Nevertheless, to anyone who reads Beddoes' and Davy's writings of this period, it must appear that more is due to Beddoes than has been rendered him. The raw Cornish lad of nineteen could not have gained in the short space of eighteen months the familiarity with chemical literature and method that is so evident in Davy's writings, and also at the same time carry out the mass of experimental work that he did, had not Beddoes not only guided, but planned and in part executed, the work.

The work on nitrous oxide carried Davy to the Royal Institution, and after his departure in the middle of February 1801 the nature of the work at the Medical Pneumatic Institution gradually changed, and by the summer of 1803 the "pneumatic practice" had been abandoned and the building was used as a clinic for preventive medicine, and continued to be so used after Beddoes' death in 1808. "Preventive" medicine Beddoes had ever regarded as the only sane policy for a progressive society, and he did all in his power to foster its growth; both services and medicines he freely placed at the disposal of the neighbouring poor. In addition he wrote numerous books and pamphlets for general reading in which he gave simple elementary rules to preserve health as well as limning the obvious symptoms of the more prevalent diseases and the appropriate treatment. Of these, possibly the best known is the series of essays published under the title *Hygëia*. Of all diseases he considered consumption the deadliest and most insidious, and to combat it did all in his power. The treatment for consumption ascribed to him by Mr. Lytton Strachey in his *Last of the Elisabethans*, was not originated by the doctor, although he fully believed in its efficacy, and employed it in all cases where the patient would submit to it. This was to put the patient on a suitably screened and elevated platform in a cowhouse.

Here the invalid slept and had his or her being until cured, and usually during the whole of the winter months. The advantages were an even temperature, the ammoniacal odours from the fæces of the cows and the carbon dioxide from their respiration. Beddoes was not the only practitioner who believed in and prescribed this treatment. Several cures were reported to have resulted from it. Needless to say, such a prescription but heightened the reports circulated of his eccentricity, and are still produced as evidence for it, as in Mr. Lytton Strachey's *Last of the Elisabethans*.

Beddoes possessed an immense capacity for hard work, and a talent, rare in his day, for collecting and sifting data, realising their logical implications and initiating the necessary complementary experimental work. To carry the investigation to a triumphant conclusion was not in him, and this is his failure. His incapability to achieve work of the first water can only be ascribed to the multiplicity of his interests, coupled with an infirmity of purpose that divided his energies. Jack of many pursuits, a family lack of mental discipline prevented him from applying himself sufficiently to any one to master it. Like his son Lovell and his own grandfather, he could follow a subject so far and then, within measurable distance of final success, would drop it to follow some newer star that beckoned, only to be lost again. The only light that he followed with any pertinacity was a cure for consumption, and even in this he allowed himself to be continually side-tracked. Bypaths, however, can be made to yield successes, and from them Beddoes made contributions of value to the science of his day and indirectly therefore to modern thought. His greatest discovery was undoubtedly that of Davy, whose researches he directed to such good purpose that the master is forgotten while the pupil alone is remembered.

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N.B.—This list is not exhaustive.

THE BRITISH MUSEUM EAST AFRICA EXPEDITION, 1924

By L. S. B. LEAKEY
University, Cambridge

In 1907 a German naturalist reported to the Berlin Museum authorities that he had found an exposed bed containing fossil bones at Tendaguru, near Lindi, in the south of German East Africa. An expedition was equipped and sent out in 1909, and it was soon established that the beds were of a period round about the Cretaceous and Jurassic, and that the remains were those of gigantic fossil reptiles.

By 1914 some 120,000 bones, including several nearly complete skeletons, and also some gigantic individual bones, (one humerus measured 82 inches) had been taken to Berlin Museum. Owing, however, to the outbreak of hostilities the work was stopped, and in due course German East Africa was given to Great Britain as a Mandated Territory under the name of Tanganyika Territory.

Sir Arthur Smith Woodward, then Keeper of Geology at the British Museum, at once urged the trustees that an expedition should be sent out to continue the work, and to obtain specimens from this remarkable find for the National Museum. It was not until 1924, however, that an expedition could be arranged, and then only on a small scale as funds were absolutely inadequate.

In February 1924, Mr. W. E. Cutler of Manitoba University, who is recognised as one of the leading palæontological collectors of the day, sailed for East Africa as leader of the British Museum East Africa Expedition, and I myself, though only an undergraduate, had the great luck to be chosen as his assistant, for I had been born and brought up in East Africa, and could speak two of the native languages.

Owing to numerous difficulties connected with the transport of equipment at the proposed site, it was not until the beginning of June that the actual work of excavating could be commenced, and even then work was considerably restricted by the density of the vegetation covering the whole area. In fact, the first few sites worked on were only found by the help

FIVE ENORMOUS BONES.
The scapula at the back measures 74 inches.

SPECIALLY SELECTED TRAINED NATIVES AT WORK UNCOVERING AND CLEANING BONES AFTER THE ROUGH GANG HAVE DUG DOWN TO BONE LEVEL.

of local natives who led Mr. Cutler through elephant grass, often reaching well above his head, to spots where they remembered having seen exposed bones when the bush-fires had done their work at the end of previous dry seasons. Once a bone exposure had been located, however, work could be started, and before long we began to discover bones in ever-increasing numbers.

Meanwhile Mr. Cutler had spent a good deal of time down dry creek bottoms, making a collection of the invertebrate fossils and corals, without which the more sensational finds of bones would be of little scientific value, for it is mainly to the invertebrates that geologists look for information as to the geological age of any bed.

But to return to the bones: when an area had been selected, a gang of natives (Angoni from the west formed the main labour supply as the local tribe, the Wamwera, were neither numerous nor hard-working) was set to work to dig down until "bone-level" was found, and they then proceeded to open up the area all around to the same level. Specially selected and trained natives next set to work to uncover all bones partially, and then the hard small bones were taken up and packed in brown paper, while the larger ones were plastered. Any bone which was "brash," *i.e.* crumbly, was of course treated with shellac. Amongst some 600 bones found between June and November were a number of truly remarkable size, as will be seen in the accompanying illustrations. It was a strange fact that teeth, usually so plentiful in similar beds, were very rare indeed, while skull fragments were hardly ever found. These two facts give very strong support to the theory that the Tendaguru beds form the site of the old delta of some great river. This river carried down the bodies of these dinosaurs and other reptiles which lived and died along its banks, and deposited them in the silt of its mouth. As decomposition set in the head would be the first part to drop off, thus seldom being carried as far down as the rest of the body.

The work of the expedition during 1924 has been simply confined to one very small portion of a great area that is covered with promising sites for excavation; not only that, but bone-beds are now reported from other parts of East Africa, even from the shores of Lake Nyassa, and doubtless as interest increases, reports will continue to come in. And yet, owing to lack of financial support from those who are interested in science and discovery, the Museum has only one man at Tendaguru, where there is work for ten or more.

The Director of the British Museum issued an appeal at the end of last year for funds to enable the work to be carried on; but, so far, the response has been small. Are we going to hold

back and let America have all the glory of great discoveries in this branch of scientific research? The American Palæontologists are doing a great work in Mongolia. Shall we let England lose her opportunity of making greater discoveries in Africa?

For further articles and photos on the first year's work of the British Museum East Africa Expedition see *The Times* of December 19, 1924, and *The Sphere* and *Illustrated London News* of January 17, 1925.

A GIGANTIC FEMUR IN ONE OF THE EXCAVATIONS.
It has been partly cleared and is awaiting plastering. A bone such as this, when plastered, will take as many as eight men to carry it up to camp.

GIGANTIC HUMERUS (HALF-PLASTERED) AND ANOTHER BONE (BEHIND IT) COMPLETELY PLASTERED.

POPULAR SCIENCE

ON FEEDING IN PUBLIC SCHOOLS

By G. E. FRIEND, M.A., L.R.C.P., M.R.C.S.

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THE purpose of this article is to review in as non-technical a manner as possible those various aspects of institutional feeding, particularly as they apply to the modern public school, which have become familiar to me in some twenty years' experience and supervision of dietetics in schools and in hospitals.

I would say at once that nothing I may write in a critical spirit is to be taken as applying to any particular institution. The faults that exist, and they do still exist, are general to a great extent, and they are, probably always, due to a lack of appreciation of essentials on the part of those responsible. Many influences are at work to diminish this lack of understanding, and the problems of institutional feeding are now receiving much more attention than they have until quite recently. For myself, I have always found that any suggestions of alteration or improvement I have had to make have been sympathetically received, and put in practice whenever circumstances have reasonably allowed.

The subject of School Diet has again been much in the public eye since 1919, as it had previously been from time to time during the past forty years, and probably even before that. It is also evident that a good deal of uninstructed criticism as well as much sound common sense have been allowed to appear in print, in the lay press.

In 1891 Dr. Clement Dukes first published his book on *School Diet*, and as regards the better-class public and preparatory school this book is still a standard of reference.

Though recent research has modified some of his conclusions, the bulk of what he then wrote is still of general application and will well repay continual study on the part of those to whom it was addressed.

Much public attention has been directed to the accessory food factors (vitamines), and there is the possible danger

that these constituents of diet may be given too prominent a place in the lay mind, to the detriment of the integer of the diet—the calorie.

In this country, given an adequately mixed diet and given that this diet is properly cooked and properly served, I venture to think that if you take care of the calories the vitamines will take care of themselves. In saying this I am not to be taken as in any way disregarding their essential importance, but as merely stressing the point that they must not be allowed to displace the calorie when the basic value of any diet scheme is under consideration. Though our knowledge of these factors is still elementary, their importance is fully proved. There is a very clear account in non-technical language of these factors and the particular foodstuffs which do or do not contain them in the pamphlet by Dr. Culverwell, and I do not propose to consider them in any detail in this paper.

We learnt many valuable lessons in the practice of dietetics from 1917 to 1920 and after; and early in the War, when considering in advance the possible effects of a shortage of essential foodstuffs on a school population, it occurred to me that the following "equation" would make the essential conditions to be aimed at clearer to the lay mind than a long physiological dissertation:

$$\left.\begin{array}{c}\text{Amount of food (eaten)}\\ +\\ \text{Amount of sleep}\end{array}\right\}\ \begin{array}{c}\text{should equal or}\\ \text{slightly exceed}\end{array}\ \left\{\begin{array}{c}\text{Amount of work}\\ +\\ \text{Amount of play}\\ +\\ \text{Amount of growth.}\end{array}\right.$$

The effect of sunlight and fresh air is to be taken into account on both sides of the equation. And it is essential to note that the balance will be affected by illness, weather, temperature. The indices which will most readily show variation from the normal will be gain or loss of weight, illness, fatigue.

The Amount of Food Required

From a study of the old diet scales of several schools of fifty years ago or thereabouts, it seems probable that the amounts of food used were arrived at empirically by providing in the first instance just so much of a plain and moderately mixed diet as sufficed to keep the consumers more or less satisfied and free from any actual sensation of hunger. The basis of the diet was also apparently founded empirically on the experience of years, combined with the prevailing fashion of the period in regard to dishes, combined also with a strict eye to economy of cost. In the better-class school, where the then normal high fee was charged, there could be no justification for such a method, and while many schools and house-masters more than

fulfilled their obligation to their pupils, in a good many instances the consumer did not get anything like the value that was being paid for. That this was too frequently the practice will be evident to any careful reader of Dr. Dukes's book. After the publication of *School Diet*, a good deal more attention was undoubtedly paid to the scientific construction of school diets, or at least the amounts supplied were much improved, but there is reason to believe that there still remain far too many schools where the feeding system remains on a purely empirical basis.

In the present state of our knowledge of feeding, a large element of empiricism is not only inevitable, but is possibly even to be advocated so long as it is intelligently and sympathetically applied. But empiricism must be kept sharply distinct from any suggestion of cheeseparing.

Even in those institutions where the supply of food is a "luxus" one, pure Empiricism is both economically and physiologically wrong, and is as much to be condemned as it is in those institutions when the supply is near or even under the actual margin of safety.

The first factor to be determined in arriving at an adequate dietary is the AVERAGE AGE of the CONSUMERS, and the same principles apply whether the number is 50 or 500.

Following the first publication of Dr. Dukes's book on Diet, many schools reconstructed their diet tables on scientific bases, and the average value of the diets arrived at then would appear to have been about the average caloric value of 2,700 per day per boy, taking the average age of the school to be between 14 and 15 years. According to the schemes recommended, such a diet would contain approximately:

Proteids	125 gms. or	4·44 oz.
Fats	100 „	3·55 „
Carbohydrates	352 „	11·54 „

From present knowledge this is a very poor diet. It is deficient in all three essential constituents, and for growing children is dangerously low in both proteid and fat. No doubt in many schools this diet was unofficially supplemented by the irregular consumption of tuck, and by jam, etc., provided by the parent.

Practical experience has more than borne out Dr. Dukes's dictum that "In the feeding of the young, from infancy to adult age, it is, therefore, important to remember this fact, that excess of albuminous or nitrogenous food is imperative—at the order of Nature—so that they may not be stinted in what is necessary to prevent them being stunted."

Such a scale as the above means practically that there is

only one good meal a day, and that breakfast and tea consist of bread and butter, or bread and margarine, with occasionally a little jam or marmalade or porridge. This is quite wrong. The first meal of the day must be adequate, and should always include either meat, fish, or porridge and jam (marmalade), in addition to unlimited bread and *butter*.

In many schools this need has gradually become recognised, and some addition has been made to the breakfast on all or most mornings of the week.

It is indefensible to make any boy either start work on an empty stomach or to go through a morning's work in school (often with a period of physical training included), unless he has had an adequate breakfast. Equally inexcusable is the practice of allowing boys to supplement the school dietary with jam or other extras provided by the parent.

Where the dietary has been drawn up on a scientific reckoning, it is now generally estimated that the average daily value of the diet per child where the average age of the school is 14 to 15 years should be about 3,000 calories.

I would go further and say that if the average age is over 14·0 years, the average daily value per child should be round about 3,200 calories. I am prepared to admit that if the conditions are ideal, this value may prove to be needlessly high. But as things are at present in many schools, to judge from information that appears to be reliable, I do not consider 3,200 calories too high a value to be aimed at in order to keep near the line of efficiency indicated by the "equation" set down above.

The Dietary

This is best divided between three meals per day.

Breakfast	7.45 or 8 a.m.
Dinner	1 or 1.15 p.m.
Tea	6 p.m.

Supper at 8 p.m. may be required as an extra for those boys only who stay up for late Prep.

I believe if the diet be liberal, varied, and properly served, in other words, adequate, that three meals are enough.

Supper for the whole school is unnecessary, and even undesirable on physiological grounds.

For the elder pupils who are working late a light snack should be provided—two to three ounces of biscuit or four ounces of bread, with half an ounce of cheese or butter per head is quite sufficient, with a small glass of water if a drink is required. Hot milk or cocoa are not needed at night, except possibly in

winter, and more food than the above tends to keep the child awake or otherwise annoy his normal balance.

The practice of giving tea at 4.30 and supper at 7 or 7.30 I believe to be unnecessary and liable to overheat the organism. With three meals a day of a total average calorie value of 3,000 to 3,200, made up approximately of :

Proteid	150 gms. or 5·33 oz.	taking the average age of the school to be about 14·5 years,
Fat	150 gms. or 5·33 oz.	
Carbohydrate ..	450 gms. or 16 oz.	

it should be possible to provide three thoroughly well balanced, attractive, and nourishing meals at a reasonable economic cost.

Breakfast.—Cocoa or coffee (with tea as an occasional variant) with plenty of milk should be the rule. The meal should include either meat, fish, or porridge and jam or marmalade, in addition to unlimited bread and butter. The latter should be given every day at both breakfast and tea.

The use of margarine or margarine mixtures is quite indefensible in view of the importance of vitamines and in view of the fees charged by the majority of schools. Dr. Culverwell's suggestion that a school dietary which contains an ample allowance of milk, and in which much of the fat is given in the form of margarine and dripping, would therefore seem preferable to one in which butter is freely used, but which contains relatively little milk, is no doubt scientifically correct, seeing that butter contains little calcium, and that Vitamine A is dependent for its proper action on an adequate supply of calcium salts. But this suggestion should not be allowed to be used as an argument for the retention of margarine in any school dietary (except for cooking), unless the economic factor is overwhelming. There are other foodstuffs which furnish a supply of calcium in a well-mixed diet, especially eggs and vegetables, while the growing organism needs all the available Vitamine A that it can get, and the action of that amount contained in the butter will presumably be assisted by the calcium in the other items of the diet. The exclusion of butter would also have to be justified by a full ration of milk to ensure sufficient vitamine supply—a condition of things which is the exception rather than the rule in most dietaries.

The Lunch Bit.—If twenty minutes' physical training is included, as it should be, in the middle of morning school, a couple of wheatmeal or ginger biscuits should be provided for each pupil and time allowed for changing clothes before and after the drill. Properly organised, this only entails a break of thirty minutes, and it has been my experience that this routine is rendered well worth while by the increased freshness

of the work during the second half of morning school. That this is so has been reluctantly admitted by some of the most inveterate scholastic diehards. The more important effects of physical training are now so sufficiently recognised that they do not need emphasising here.

Dinner.—As Dr. Dukes wrote, this is and always will be the best meal of the day. There should always be a choice of meats, or at least a frequent daily variation of kind, which should be fresh cooked for each meal. Twice-cooked meat should never be served if it can possibly be avoided. "Cold meat is better than re-cooked scraps." "If meat must be re-cooked, mince it and make cottage-pie." "Re-heat; never re-cook."

With good management it is as economical and nearly as easy to provide once-cooked as twice-cooked meat, and it must be remembered that the latter has most if not all of its growth-producing value destroyed by the additional processes to which it has been subjected. Stews are unfortunately necessary at times to avoid waste, but they should be limited in frequency and more carefully made than is often the case. Cottage-pie is better value than stew and has the advantage of being a popular dish.

A second vegetable, preferably of green stuff, should be served not less than four days a week. Beetroot is not a full substitute for greens.

When soup is given instead of meat, it should be made mainly from vegetable stock freshly prepared, and should not be an ultimate extract of the ox.

Where it is the practice to provide cheese in place of a pudding, it should be the rule that a substantial duff or suet pudding be served on soup days. Cheese should never be given at the same meal as soup, and soup days should never be allowed to coincide with competitive football matches.

Milk puddings are probably more popular with the cook and the caterer than with the consumer. They are easy and comparatively economical to make, but I doubt if they are as good value for children of school age as they are usually assumed to be. I would rather see the milk allowance issued as milk with porridge, tea, coffee, or cocoa, and as *milk* to the smaller and delicate child, than have it perfunctorily transformed into a sloppy and unappetising mess, as is too often the case. A good milk pudding is a very pleasant dish to many, but not to the majority at this age; but even so it can be served too frequently, while a badly made one is enough to destroy the most robust appetite. Two a week with stewed fruit are admirable; more than this often tend to defeat their object.

Tea should consist of unlimited bread and butter (never

margarine), with either cake, jam, or fresh green food as an extra, and not as a substitute for butter.

When considering the amounts of food to be provided for any diet scheme, it has to be remembered that in catering for *growing* children it is essential that a surplus of proteid and fat be provided. The supply of meat per day should, according to Dr. Dukes, be not less than nine and a half ounces uncooked weight per head, while Dr. Hutchison says that at least one-third of the total daily proteid should be given in the form of meat. These statements are approximately in agreement.

The supply of bread should be unlimited, and sufficient jam and butter provided to ensure that enough bread is eaten. As regards the question of wholemeal versus whitemeal bread, a bread made from a flour containing not less than 80 per cent of the whole wheat grain is preferable, with the occasional addition of some bread made of a 90 per cent flour (a stronger flour than this will not be tolerated by the modern child's digestive organs without a process of desensitisation). But in the better-class school, where presumably a fully adequate diet is supplied, the value of the wholemeal as against the white bread is probably not of that vital importance as it is where economic pressure is more of a factor. In the case of poorer institutions or families, a wholemeal bread and flour is undoubtedly both more economical and more efficient as a staple food. On purely physiological grounds also the 80 per cent wholemeal bread and flour is to be preferred.

Beverages.—Tea and coffee have by now become established as staple beverages. It should, however, be remembered that their food value is practically nil, for children their stimulant properties are undesirable, and as aids to appetite they should be unnecessary. They serve as useful (*sic*) vehicles to ensure the consumption of part of the milk ration. Cocoa has fewer disadvantages and perhaps some advantages, but its food value is generally overestimated. Cocoa when provided should be disregarded in estimating the caloric value of a diet. For several reasons it is a pity that the practice of providing a small quantity of a light malted ale at at least one meal of the day in school dietaries has been given up.

To attempt to make anything like an adequate list of the various articles of food which can be used in their seasons to ensure a sufficient variety in the diet scheme would take up far too much space. The matter is fully and admirably dealt with in Dr. Dukes's *School Diet*, while much useful information in regard to the vitamine aspects of many foods is clearly and non-technically given in Dr. Culverwell's pamphlet. A full account of the sum of our present knowledge of vitamines is

contained in Professor H. E. Armstrong's Cantor Lectures for 1919.

Common Faults in Institutional Feeding

1. *The Basis of Diet Tables is too often purely Empirical.*—In many schools the food supplied is quite adequate in amount, and even excessive, but yet the diet as a whole is badly balanced and may be grossly lacking in some essential constituent. It is said on good authority that in many schools margarine or margarine mixtures are still used in place of butter. The supply of milk is often too small. In either case it will probably be found that the general health is below par, boils and other chronic skin infections are frequent and take longer to heal than they should, or the average mental condition is slack. In other words, there is fatigue.

2. *Too Short a Time is allowed for Meals.*—This is the case in many schools, especially in respect of dinner. Dinner should never last less than forty minutes. Too often thirty minutes is considered ample, and the fashion is to hurry over the food, which is bolted instead of being properly masticated, the younger children having to keep pace with the older, in order that they may all be off to some game instead of being made to take sufficient time to properly dispose of the main meal of the day. The younger the child the more time he needs to masticate a full meal, and it is the duty of the proper authority to see that this time is provided.

Incidentally, no strenuous nor any organised game or exercise should be permitted to begin for at least one hour after dinner. Reading books at meals is bad for several reasons and should never be permitted.

3. *Early Morning School.*—It cannot be too strongly insisted on that it is entirely wrong to make boys do any work (mental or other) on an empty stomach. From practical experience since 1917 I am sure that once school before breakfast has been given up and the effect on the pupils carefully noted in any school, it is unlikely to be resumed. A small mug of cocoa or a couple of biscuits is not a meal, and to give this at 7 a.m. and then extract an hour's work before the real breakfast is as wrong as it is to exact the work on an empty stomach. For it is calling the digestive apparatus into activity to deal with an inadequate mass, and then when the initial stimulus is beginning to slow off, to suddenly present the apparatus with a full load to be dealt with before the organs have had time to recover their poise. In this, as in the habit of "snacks between meals," lies the origin of much digestive trouble to be developed later on. To be condemned equally with early school is the practice of holding morning chapel before break-

fast. This may be permissible in summer in warm weather, but it is definitely harmful in winter or bad weather. If the time-table is the obstacle to having chapel between breakfast and morning school, it would be better to lengthen the interval between breakfast and dinner to five and a half hours, than send the pupils to chapel on an empty stomach.

4. *Lack of Variety in the Daily Menu.*—In far too many schools it is the practice to draw up a weekly menu which does duty without any alteration week by week throughout the term, if not for longer, except only when the butcher fails to deliver some particular order to time. Then " system " becomes worse confounded, and the hapless consumers for that one meal are fed on some hastily contrived substitute. Such a system of menu can only be described as the acme of bad management.

A menu for each meal should be drawn up, giving in detail every meal for every day of each week of the term. And it is especially important that the day on which any particular table or mess will have each particular dish served should remain an unknown quantity to the consumers beforehand. There is nothing so likely to interfere with appetite than to know that every Friday you will have stew for dinner, porridge for breakfast, and plum jam for tea. Variety is one of the chief natural appetisers, and everything possible to provide plenty of variety should be insisted on. It is really only a question of good or bad management.

5. *Proper Service of Meals.*—This is nearly as important as sufficient variety. Proper service includes punctuality and regularity in the time of meals. Hot dishes should be *hot*. Meat should be properly carved and decently plated. The table appurtenances should be clean and kept in proper repair. As it is the duty of the providing authority to procure an adequate dietary, so it is his duty to see that it is properly, decently, and appetisingly served at table. On so-called economic grounds the number of servers is often insufficient, carving is badly done, food is not hot or is sent in untidily or dirtily served or dished. Anything that tends to diminish appetite is bad both economically and physiologically. Properly served food goes further, does more good to the recipient, and so saves money both directly and indirectly to the provider.

6. *Bad Cooking.*—The above remarks apply equally to the kitchen. Far too often food is wasted and spoiled, or has part of its value destroyed by bad cooking. The food is good enough when bought and plentiful in amount, but its value may be diminished by as much as 50 per cent by indifference, carelessness, or bad management in the kitchen.

7. *The Diet Scheme should be capable of adaptation* to meet any great or sudden change in weather or school routine. In

hot weather less meat and less fat will be required, and a more frequent supply of fresh green food, such as watercress, radishes, lettuce, etc., and fresh fruit be called for.

Fresh fruit and greens should be much more frequent items in the diet in most schools, and the same remarks apply to green vegetables.

8. It is not enough that sufficient food be bought, supplied to the consumer in an appetising form, and properly and punctually served. It is also the duty of the immediate supervisor of the meal to see that while all this is done, no pupil is detained from getting to the meal at the proper time, for any reason, and that when there is assured of getting enough time to eat it. It is essential to see that the small pupil is not crowded out or passed over. This in the very nature of things is most likely to happen unless there is constant supervision. The smaller child requires pro rata as much food as the bigger, and he also requires more time to eat it. He is often served last and has to finish at the same time as his bigger fellow who was served before him. This is quite wrong. It does not happen at home and it should not be allowed at school. Given a properly balanced and adequate dietary, I am convinced that a great increase in efficiency as regards value with considerable economic saving can be obtained if due regard is given to the questions of variety, service and cooking, and the æsthetics of the table. I do not think that sufficient attention is paid to these points in the majority of institutions. From a fairly intimate knowledge of the feeding systems now in force in over a dozen schools, including public and preparatory, boy and girl, I an sure that increased attention to these aspects would pay for itself over again. These matters will seem of small importance to many, while others will no doubt consider that they are already giving them as much attention as is worth while. I venture to differ. Should such criticism be offered, I would say to the critic that if he or she possessed the necessary elementary knowledge of physiological principles that underlie the science of dietetics, they would more fully appreciate the value of good work in these directions.

It is almost a national habit to grumble about the food we have served us. He who pays directly for his food is less critical than those who are catered for. No institution is free from such critics. They are sometimes more or less justified in their criticism. Usually less than more, in my experience. Criticism has probably always been expended on school feeding. Occasionally it is no doubt justified, but I think, on the whole, conditions to-day in the majority of schools are much better than the popular idea would lead one to suppose. That there are faults is to be admitted, but I do not believe they are

fundamental in the majority of schools. Where they exist, they are mostly on the lines indicated above and are not to any great extent incapable of solution.

There are two systems of school feeding in force:

1. Where all meals are taken in a central hall, the food being provided and cooked by the school.

2. The boarding system, where the pupils are fed in houses by the house-master.

1. If the school, say of 400 to 500 pupils, is fed in a central hall and the meals are provided by the school from a central kitchen, stocks are got in wholesale at wholesale prices. Their issue is in the hands of a trained steward or housekeeper, who is also responsible for the daily menu and the service. Results in this case depend on the managing capacity of the person in charge and the problem is capable of a fairly easy solution. I believe the solution to be the employment of a trained lady superintendent with direct responsibility to the ultimate authority.

2. Suppose, however, the school lives and feeds in houses of forty to fifty pupils. Each house is separately run by the house-master, who is responsible for the catering. He probably delegates the feeding arrangements to his wife or a housekeeper, who may or may not be trained to the work. In any event, his and his wife's knowledge is most probably amateur and empirical. Neither of them is a trained buyer, house-keeper, or cook. They start and they continue of necessity at a disadvantage in their dealings with the traders, whose business it is to get the better in any bargain. There may be eight, ten, or more houses in the school being run on these lines. Some suffer, others may not. All buy separately, and all cook and serve separately. Probably the servant problem has to be considered, and they dare not offend the cook or ask the maids to do any more work. They do not realise that if they could all buy collectively, and obtain collective supervision of their own household services, they would at once double their efficiency and save one-third of their outlay, possibly more. This could be done by the joint employment of a trained lady superintendent, who would do the whole buying, arrange the dietaries, supervise the staffs and service, and in addition look after linen and domestic arrangements generally. Such a superintendent, being trained to the work, would within a few months effect such a saving as would suffice, after paying her a good salary, to provide a margin, part of which could be used, if necessary, to assuage the feelings of the cooks and maids. In my own experience in three institutions it is quite possible to introduce such a system not only without friction, but with considerable increase of comfort and efficiency to all concerned.

The same suggestion was recently advocated in a letter to *The Times* from Miss Jean Muldrew of Ottawa, published in the issue of December 22 last.

I have purposely refrained from discussing in any detail the actual proportions or the basic construction of a diet scheme. I take it for granted that the essential constituents are known to all interested in the subject. Further, as the data necessary for such a purpose are set out so broadly by Dr. Dukes in his book on *School Diet* and so clearly and in such detail in Dr. Hutchison's book on *Food and the Principles of Dietetics*, it is an unnecessary as it would seem presumptuous for me to attempt to do more than allude to this aspect of the subject. I have rather tried to picture the problem as it affects the administrator, and to point out certain of the more obvious defects in so far as I have been made cognisant of them by my own institutional experience.

I wish to express my indebtedness to those authorities to whose work I have had recourse for information both for this article and for my own previous instruction, and for greater convenience in reference I have added a list of the books and papers consulted instead of giving individual references in the text of the article.

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NOTES

George Downing Liveing, Emeritus Professor of Chemistry, Cambridge University (Sir Arthur E. Shipley, G.B.E., F.R.S., Master of Christ's College).

THE Mathematical Tripos was founded in 1748, and for more than three-quarters of a century those students at Cambridge who took an honours degree had to take this Tripos, but in 1824 the Classical Tripos was established and in 1851 the Moral Sciences Tripos and the Natural Sciences Tripos were inaugurated. In the latter the first man in the first class was the late Professor Liveing. He was followed by Professor Hort, afterward Lady Margaret Professor of Divinity, and throughout his long life a keen botanist. Altogether there were only six candidates, whose number was halved the following year. In 1854 Liveing was an examiner. He must only just have taken his M.A. degree. For a while he worked in Germany, and returned to Cambridge to become a Fellow and Lecturer at St. John's College. His Fellowship, however, was vacated on his marriage in 1860, and in that year he became Professor of Chemistry at the Military College at Sandhurst, though he continued to teach at St. John's. The year following his marriage he was elected to the Professorship of Chemistry at Cambridge, and he held this post until the year 1908.

At this time there was little or no provision in the University for practical work. In the 'fifties the University had acquired the site of the old Botanic Garden between the back of Mr. Mortlock's Bank on the north and Downing Street on the south. But things do not move very rapidly in the older Universities, and although plans were prepared which included rooms for the Mathematical Professors and the Jacksonian and for the Professors of Botany, Chemistry, and Mineralogy, nothing came of them. It was anticipated by the Committee which was considering the matter that at least two of these seven Professors might lecture during two terms and the remaining five during one term! Assuming this, they thought that three lecture-rooms would suffice for all the Professors. But there was no money, and the scheme fell through.

However, when I came up in 1880 a Laboratory had been

built, though not a very adequate one. It stood on the site of the now gaping wound in Corn Exchange Street between the buildings of Pathology and of Zoology, and one can still see on the northern wall the painted dado of the upper chamber. Here and in an adjoining room where Sir James Dewar worked, Liveing started our great School of Chemistry.

But at first his activities were by no means limited to his own subject. At this time, owing to the fact that there was no Cavendish Professor of Experimental physics, Liveing had to do for ten years a good deal of physical teaching. Many years later it was due to his energy and initiation that the School of Agriculture was started, and, if Chemistry had a small beginning, Agriculture had a still smaller one, for it was housed at first in a couple of small rooms in the basement of the Chemical Laboratory, which had by 1889 been built on the site of the old Perse Almshouses.

Liveing spared no pains in drawing up the plans of his new Laboratory, even down to the minutest details. He visited many continental Institutions, and it is satisfactory to read that when at last in May 1899 the Laboratory was finished the Professor of Chemistry and the Jacksonian Professor both recorded, "We are now in possession of the whole building, and we are glad to be able to state that it fully answers our expectations." These Laboratories have since been increased and increased. At the time of Professor Liveing's retirement there were working there two hundred and fifty students and researchers; but at the time of his death this number had been increased threefold.

Liveing was a simple man, with all the strength of simplicity. He never sought fame, and if anything rather effaced himself. For instance, he was by no means fond of taking part in the discussions in the Senate House. He was physically very strong. As an undergraduate, he rowed in the Lady Margaret Boat, and he always, even to the latter years of his life, worked in his garden. At the age of ninety-five he was found cutting faggots. In his garden he took a keen interest, and he had a considerable knowledge, as a landowner, of agriculture as well as of horticulture. His wife was for many years an invalid, and it was one of the familiar sights of the town, when I was a student, to see the Professor wheeling her out every afternoon in a bath-chair—rather a pathetic sight. He was extraordinarily natural, and you always knew more or less how he would take a thing, for he was always true to the high type that was in him. He was very rarely ill—a fact which he attributed to his always wearing flannel next to his skin. But he had at least two long and serious illnesses which he bore without complaining and with the utmost patience; and indeed with a certain silent

cheerfulness that seemed little removed from indifference. As a magistrate he was constant in attendance on the Bench, and it is on record that his series of demonstrations on spectroscopy which lasted two hours were given on five days of the week only because on the sixth he had to attend the Court, for, as he said, without his " sanction no man could be put on bread and water." " Men of science, my Sandra, are always the humanest," as Laura says in *Villoria*. The following anecdote is contributed by my friend Mr. J. P. Millington, who knew Liveing intimately for many years.

" Of his magisterial office he was proud. I remember one occasion when two of his assistant demonstrators had been charged by the Cambridge Police for riding bicycles on the footpath, were duly summoned to appear before the Bench. Jointly they went to Liveing, admitted their guilt and asked to be excused from attendance. Liveing naturally wished for an excuse, which was at once forthcoming—' Professor ! If we attend the Court we shall not be able to demonstrate in your Laboratory.'

" ' Good ! but I shall fine you, all the same.' He did."

Mr. Millington records the Professor's inveterate dislike of tobacco, a dislike shared by many a man of his age. And again :

" Liveing had a great objection to being interrupted when at work in his private room in the Laboratory. On one day I was asked by a man whether he could see the Professor and where was his private room. My reply was that the Professor did not see people except by appointment, but—there was his room. The man explained that he had come especially about the insurance of the Laboratory Buildings and Contents. I could do no more than point to the door behind which the Professor worked—and wait.

" I heard three taps on the door, a voice ' Come in,' a louder voice ' Go out,' a shutting of the door. I retreated with discretion."

He was indeed extraordinarily terse in his conversation ; not exactly abrupt or curt, but sometimes very monosyllabic. Like Captain Cuttle, he did not waste " language as some do." On the other hand, he could be a very entertaining companion, and one of the most remarkable efforts I remember was his delivering an address after the annual dinner of the Philosophical Society at Sidney Sussex College, which lasted the better part of an hour, at a time when he must have been over ninety. He was at times rather irascible, and in fact when I was a student he used to be called " Red Precipitate." But he had a kind heart, although he was the most logical of men, and fearlessly traced effects to their causes.

Mr. Millington further writes:

"When I had the chance of taking up some industrial work I naturally asked him if he would allow me to refer my prospective employers to him. This was done by letter. On the same day I was told that the Professor wished to see me at his house ('The Pightle'). Of course I went, and was shown into his study. The old man was sitting before a fire with one leg resting on a gout-stool. I ventured to sympathise with him on his rheumatism. 'I have got no rheumatism! Went up to London two days ago to a meeting of the Royal Society. Took a hansom cab! Slipped on the step! Rheumatism? Never had it in my life. I will tell you the secret. Thick flannel next to my skin!' He hitched up the leg of a trouser and displayed the underwear. He spoke truly.

"Another foible of the old man was a hatred of draughts. In my time each laboratory was fitted with swinging half-doors. On one occasion I let the doors of the first-floor swing and bang, left this floor and went to the third floor. There, well inside, I was greeted thus: 'You slammed the doors! You created a draught! Do you not know that my life has been one long crusade against draughts? Don't do it again!'"

The management of so large an institution as the Chemical Laboratory inevitably took up much of the Professor's time. His lectures were most carefully prepared, and his experiments were most striking. To all those who had sufficient mathematics to appreciate his leaning in that direction they were very stimulating. Every day he went round the classes and spoke and criticised the work of the students. He could never tolerate slovenly work. In spite of the many cares of the Laboratory administration he found time to carry out, in conjunction with Sir James Dewar, some remarkable researches in spectroscopy which were later republished in a volume and now form a standard work on the subject. Even during the last few years of his life, in spite of his great age, he was occupied in carrying on a long and difficult experimental research on Radiation in the Metallurgical Laboratory, and indeed it was on his way to that building to continue his research that he met his accident which ended fatally.

He laid down rules for the management of the workers in the Laboratory which had to be most strictly adhered to. The Laboratory was closed early in the evening, and no one was allowed to enter it till the next morning, and no work was ever permitted on Sundays. He was a very generous man, and he objected to his accounts being audited, as he did not wish the University to know how large a sum he spent out of his own pocket on the upkeep of the Laboratory. Many a rising young chemist was only enabled to pursue his career owing to the

financial assistance the Professor gave him. But no one knew of these things.

Liveing was extraordinarily helpful to the poor; but he never let his left hand learn what his right hand gave away. He took particular interest in the prisoners in the County Jail, visiting them weekly and helping them to regain their self-respect when they were set free. No better epitaph could be written of him than the lines Fitzgerald adapted from Crabbe:

> "Friend of the Poor—the Wretched—the Betrayed
> They cannot pay thee—but thou shalt be paid."

Sir James Mackenzie (R. McNair Wilson, M.D.)

Sir James Mackenzie, who died of angina pectoris on January 27 last, was one of those outstanding personalities who leave an indelible mark on the thought of the world. And, like most pioneers, he suffered from the fact that, to a great extent, his ideas were misunderstood. The very people who loudly acclaimed his discoveries were often the last to comprehend their meaning.

For this reason it is necessary to direct attention to the "last phase" of his life rather than to the earlier periods, to which full justice has already been done. Mackenzie was fortunate, from a worldly point of view, in making the discovery that a tambour placed over the carotid sheath in the neck affords a tracing or graph in which a wave is shown corresponding to the reflux of blood occurring in the jugular vein when the auricles contract, and in which, also, the arterial wave in the carotid vessel is displayed. This so-called "venous pulse" makes it possible to examine not only the general condition of cardiac activity, but also the separate activities of the auricles and ventricles. Thus the discovery led to a differentiation of the various forms of arrhythmia, and directly or indirectly afforded a new knowledge of the heart's action.

The medical world, apparently believing that Mackenzie had set out to make the discoveries he actually achieved, at once hailed him as a great cardiologist. His ink polygraph, and later the electro-cardiograph, were eagerly employed, and his conclusions speedily verified and accepted. A "new school of heart medicine" sprang into being, and it was generally supposed that Mackenzie would rest on his laurels.

When, therefore, the "great cardiologist," established in a lucrative Harley Street practice, suddenly closed his doors and returned, of his free choice, to general practice there was widespread amazement. Had he not "risen" from general practice to the "status" of a consultant? Had he not made discoveries which had opened up a new vista? And so on.

What had possessed him, that he should "throw it all away" for the dull and "inferior" work of a "G.P."?

The people who spoke in this fashion—and there were hundreds of them—simply had no conception of the task which this great doctor had set himself. They did not know—they did not even dream—that the discoveries made in connection with the heart seemed to the discoverer himself no more than "pebbles picked up by the wayside." At best they were illustrations of the rewards in store for any man who followed the same method. As an end in themselves they simply had no place in Mackenzie's mind.

And that was the fundamental difference between the leader and his disciples—the difference so few of the disciples even recognised. They were eager to call themselves "heart specialists"; he disliked the title, and escaped from it at the first possible opportunity. The title which he really coveted was that of "physician—student of the science of medicine."

The words "science of medicine" in his mouth, however, had a special meaning. Medicine, as he understood it, was a complete and separate branch of human activity. It was the λόγος of the living man; not that of the chemistry of his tissues or the physical laws governing his relationship to his environment. It was neither biology nor pathology nor physiology nor psychology, but something bigger, if less precise, than any of these.

He had his first glimpse of this "Science of Medicine" by the bedside of his earliest patients in the Lancashire town in which he set up practice in the first instance. He recounts in one of his books how he observed that a shock or injury would cause one patient to faint, and another to grow red in the face, and more active in the body. Where one man suffered pain, another appeared to go scathless. These immense differences of behaviour gripped his interest. He asked himself the question: "*What is the basis, the determining factor, of all the innumerable symptoms which a human body can display?*"

That question haunted his mind for nearly half a century, until the moment when his last seizure began, and mental activity was finished for ever. He was seeking for an answer to it when he discovered the venous pulse and the nature of cardiac arrhythmia, he was still seeking for an answer when he sat down among the general practitioners of St. Andrews, and bade them treat him merely as one of themselves.

It is impossible here to trace the history of that strange quest. Mackenzie encountered immense difficulty with magnificent courage, so much so that some of the passages in his writings recall Defoe's descriptions of Robinson Crusoe on his island. Everything he needed in his work had first to be

created by his own hands. There were no predecents. The conclusions he reached, however, must be set down, for much will be heard of them in days to come.

He saw that the "symptoms of disease" are merely an expression of the manner in which the body is reacting or responding—*not to disease* (*the common fallacy*)—*but to its environment.* There is an immense and fundamental distinction here between the medicine of the schools and the medicine which the practitioner of Burnley evolved. An illustration must suffice. The pain of a sprained ankle is a symptom. Most of us would call it "a symptom of a sprained ankle." Mackenzie knew better: he called it a symptom of inadequate response to life. Thus, he looked beyond the ankle to the demands made by life on it, and to the methods by which these demands are normally met. When, one day, he noticed that a passing attack of influenza caused a recently sprained ankle to swell up and appear to be "restrained" he was not surprised—as most doctors would have been. Another factor was now interfering with the responses to life (the poison of the influenza) and hence the symptoms of inadequate response had been exaggerated. The ankle had not been "restrained"; what had happened was that a further handicap had been imposed on its power to make complete responses.

Recognition of this led him to see that all living tissue is in a continuous and perpetual state of reaction to a stream of stimuli which is never dammed up. We never escape from our environment or from the need of responding to it. Thus, the air of the room surrounding the sprained ankle stimulates it by virtue of heat or cold, wetness or dryness, movement or stagnation, etc. If each of these minute stimulations meets with its perfect response no symptoms will be observed. But if any response is altered or prevented "symptoms," *e.g.* pain, sense of heat, swelling, and so forth will show themselves.

Two men are seated in the same warm room. One "feels" well; the other shivers. This symptom, shivering, indicates an alteration in the normal response to warm air—not necessarily the cause of that alteration. For the shivering man may be a condemned criminal awaiting the step of the hangman at the door of his cell, he may be a man in the cold stage of malaria, or again he may be a patient, recently run over in the street, and now suffering from shock.

In truth, we do not react to disease at all. We react only to life—to the stimuli which environment imposes. Disease may operate (*a*) by changing the nature or intensity of such stimuli, or (*b*) by hindering or altering our responses. *It can evoke no responses of its own.* The condemned criminal shivers because intense and terrible stimuli are flowing along his nerves

to which full and complete—and so comfortable—reactions are impossible. The patient with malaria is suffering no such excessive stimulation; he shivers because the poison of the disease is either exaggerating the excitability of his nervous system to such an extent that even "room temperature" is as powerful a stimulant as a chill wind or is preventing a normal reaction. The patient with shock, finally, cannot react by reason—perhaps—of the previous exhaustion of his reacting mechanism.

A quick pulse, tachycardia, may therefore be due to exaggeration of the normal stimuli of life, since it is an "exaggerated response." This can be brought about by the action of poisons on the nerves or by changes in the sense organs (for example, a slap on a raw skin surface exercises a very different effect from a slap on a normal skin surface). Thus the cure must lie far from the heart itself.

Mackenzie was working on these lines at the time of his death. It does not need much imagination to perceive that they foreshadow "a new medicine," remote from the work of the dead-house or even the laboratory.

Science and the Public (R. R.)

Nature of the 14th February last published an able obituary of Dr. Oliver Heaviside (who died on the 3rd February) written by Dr. A. Russell, F.R.S., who says, "He has probably saved the Government of every large civilised country in the world millions of pounds in the costs of their telephone schemes." Yet, we gather, he has been, for more than twenty years, the recipient of a Civil List Pension. These pensions are practically charitable annuities of a few hundreds a year or less, given by this rich but grateful country, after a personal suit to a Prime Minister, for services in science or art or other mean subjects. We infer, then, that for saving millions of pounds to the tax-payers of the world, some of them, with noble and rare generosity, gave Heaviside a few hundred or thousand pounds.

We commented in our previous number (page 498) on the manner in which one of the most important and brilliant discoveries ever made, that of the carriage of the plague bacillus by the rat-flea, has been accepted by the world with little thanks and no reward for those who made it. One is amazed both at the meanness and at the folly of such ingratitude. I once heard a philanthropic peer exclaim about payments proposed for another discovery, "Rewards! Rewards! It would cost the world untold millions to reward such an important discovery adequately. It is best therefore to give the discoverer nothing at all, but to assure him of our respect." Perhaps the discoverer would value the respect at its money's worth. Another politician said in the House of Commons on July 20, 1921, that "the difficulty of apportioning merit for even the greatest of discoveries is often overwhelming; monetary rewards would lead to jealousy instead of co-operation among research workers" (Science Progress, 1921, page 286). What a convenient attitude for the Exchequer! Lastly, the Royal Commission on Awards to Inventors excluded all medical discovery from any pecuniary awards because (it argued) doctors had always been noble enough to do such public work for nothing! It is easy to find excuses for refusing to pay just debts, and the baseness of doing so is not apparent to certain minds. But

ingratitude for benefits received is also as foolish as it is base, because it automatically discourages benefactors; and possibly no more pernicious judgments than those mentioned above have ever been uttered in any country pretending to be civilised.

The Typical Discoverer (R. R.)

The history of Columbus is perhaps the type-history of all true discoverers in any line, and it may be worth while in this connection to remind the reader of it. Columbus was a poor native of Genoa, who had led a wandering maritime life until, at the age of about forty (according to the estimate of his friend Bernaldes—that is, about 1475), he began to conceive the mad idea that if the world is spherical we might be able to reach Japan and China by sailing due west from Europe instead of by travelling east, as Marco Polo had done two centuries previously. Until 1492 he vainly appealed to the able sovereigns of Portugal and of Spain for ships to enable him to make the attempt—his suggestion was scoffed at and finally rejected by two learned councils of scholastics. In that year, however, the noble Queen Isabella of Castile was converted by the eloquence of his friends, and he set forth with three crazy little ships, which were all that Spain would give him. The story of that voyage is perhaps the greatest tale in history; and in seven and a half months he returned with the discovery of a New World. But on his return he suffered the usual experience of discoverers—Pinzon, the captain of one of his caravels, had left him in the hope of reaching Spain before him and claiming the discovery for himself, and was foiled only by arriving too late. Columbus repeated his explorations in a second and a third voyage; but novelty had worn off for the fickle people of Spain, and on the third voyage he was sent home in chains by the infamous Bobadilla, who had superseded him in command owing to the machinations of his enemies at home. On his fourth voyage he was left marooned on an island for many months by Bobadilla's successor, Ovando, and then finally returned to Spain, where he died worn out and in poverty in 1506. Isabella had died previously, and the compact which he had made with her and Ferdinand before his discovery was never carried out by the latter, who, however, ordered a monument to be erected to his memory! He was evidently a man of far greater intelligence than those who surrounded him, and was patient, devout, punctilious, and good to a degree; but he was always persecuted by jealous fools—especially by the scheming Fonseca, who, having been put in charge of "Indian affairs" at home, was constantly able to thwart him.

History shows that such is almost the common story of all those great adventurers who add to human science by daring to cross uncharted oceans, as compared with the petty navigators who creep from cape to cape along the shores of knowledge. Since then many a Columbus has enlarged the potentialities of mankind; but the intelligence of the masses remains in the same condition to-day as that of the Spaniards who enchained him and who starved Cervantes, of the Greeks who poisoned Socrates, the Jews who crucified Christ, and the Italians who persecuted Galileo. Men have learnt many things, but not yet either the importance or the psychology of the true discoverer; and the result is that true discovery remains fortuitous and rare.

Columbus, Bobadilla, and Ovando (R. R.)

In the last two numbers of SCIENCE PROGRESS I have described in some detail my own conclusions, formed during many years, regarding the best methods for encouraging real advances in medical science. In this country, the United States, and elsewhere, considerable sums are now being spent in the

employment of many enthusiastic prospectors for the gold of science under the direction of equally enthusiastic gentlemen few of whom have themselves ever succeeded in finding any or much of that precious metal. They are all doing their best, and it would be churlish and, therefore, impossible to criticise them or their efforts in detail. But all this is an expensive procedure, and is not the best way to secure the best results. The only real method, or at least the principal real method, is to encourage and employ by every means in our power the men who have already obtained the most notable results in the past; and this is just what the world is not doing at present. In medical lines, at least, it throws away its tried and proved prospectors and mostly employs those whose principal qualification is their ambition. Columbus is superseded by Bobadilla and Ovando, and progress ceases.

Workers on the Theory of Evolution since Darwin

Students of science who are not biologists must often be confused by the constant discussions of evolution which now distract attention rather than define truth, and they will, therefore, be duly grateful to Prof. L. W. MacBride, F.R.S., for his very capable summary of present knowledge and theory, under the heading given above (*Nature*, January 10 and 19, 1925, pages 52 and 89). Incidentally, we note with some surprise, in view of the large number of biologists who have lived since the *Origin of Species* was published in 1859, how very few of them are mentioned in this outline—only Darwin, Spencer, Lamarck, Huxley, Meckel, Haeckel, Weismann, Romanes, Goldschmidt, Mendel, Correns, Johannsen, Bateson, Morgan, Jennings, Agar, De Vries, Willis, Einer, Cope, Driesch, Hertwig, Kammerer, Durkhen, Harrison, McDougall, Tornier, and Jansen: that is twenty-eight names altogether. Doubtless more names would be entered in a fuller treatment of the subject; but in any case the paucity of effective workers on evolution compared with the vast totals of the entire human population over the same period supports my thesis of the great rarity of really fruitful work in science (SCIENCE PROGRESS, October 1924 and January 1925). Studies of the figures regarding almost any of the main branches of scientific progress—such as the Calculus, Gravitation, Relativity, Parasitology—will yield similar results. Obviously, effective advances are almost aways made only by men who happen to be specially endowed by nature for the work, and must, therefore, be usually very rare.

The Tsetse Fly in Africa.

The dinner given by the African Society on January 28 to the Hon. W. G. A. Ormsby-Gore, M.P., Under-Secretary of State for the Colonies, Major A. J. Church, D.S.O., M.C., and Mr. F. C. Linfield, the three members of the East African Parliamentary Commission, is fully reported in our excellent new contemporary *East Africa* of February 5. Mr. Ormsby-Gore spoke for an hour on a series of interesting topics. Especially regarding the tsetse fly he said, "Between Tabora and Mwanza we witnessed one of the most dramatic things in our tour, the fight against the tsetse. Half Tanganyika—which is bigger than Nigeria, the biggest dependency under the Colonial Office—half of that vast area is under or is threatened by the tsetse. I cannot too much emphasise that fact. It spells death to cattle and horses and to all forms of animal transport, and death in a way to the native; for, apart from the sleeping sickness which is carried by some forms of tsetse, by bringing death to cattle, with which the whole social organisation of the Bantu races is bound up, that social system disappears. The fight against the tsetse is a tremendous thing." Then Major Church, who followed, said: "Medical research is just as essential as other research. Tsetse is encroaching every-

where. Except for a few parts of the highlands of Kenya, practically the whole of East Africa is threatened with fly domination, which is spreading daily from the Sudan to Southern Rhodesia. There is no backing for those who are trying to combat it. They can only touch the fringe of the problem with their small funds. Swynnerton, near Shunyanga, gathered under Makwaya, a most enlightened chief, ten thousand natives, and said to them, 'You are now waging a battle against a more insidious enemy than man. Bring out your men, women, and children; we are going to exterminate this pest. It attacks and kills the cattle.' Where cattle die, the people leave. In one spot a population of 30,000 has decreased in four years to 3,000 because of the loss of cattle. It is the vicious circle. We cannot allow these territories to be dominated by an insect; £200,000 at least is needed to equip a first-class scientific expedition to go to East Africa to work on the spot, study the problem, not to exterminate the fly within two years, but to lay the foundation of a campaign to exterminate the fly in five years. What Ronald Ross and Patrick Manson did in their sphere can be done against the tsetse. We want the best men the nation can produce."

The Royal College of Surgeons of England (Sidney C. Lawrence, M.R.C.S.Eng., M.B., D.P.H., Hon. Sec. of Society of Members of R.C.S. Eng.)

I WILLINGLY take the opportunity offered me to record my impressions of the proceedings at the annual meeting of Fellows and Members at the College, Lincoln's Inn Fields, London, W.C., on Thursday, November 20, 1924.

There was a good number of Fellows and Members present in the theatre —sixty-seven, to be exact—to receive the report of the Council for the year ending on July 31, 1924. Accompanying the President, Sir J. Bland-Sutton, were six other Fellows, members of the College Council: that is, seven attended out of twenty-four Councillors! This is not a good record, but this is the only meeting for which no fee is paid. Besides, a Councillor gets bored by hearing detailed annually the grievances of Members, which he does not wish to remove or set right.

On this occasion the annual report was not actually submitted to the meeting, although a few copies were found reposing on the benches. The President did not even (as he did in 1923) move that " it be taken as read." It has been customary for former Presidents to spend twenty to thirty minutes skimming through its pages, making (more or less) elucidatory comments, as he sped by. However, Sir John Bland-Sutton, after passing through an ordeal of questions, called on Dr. W. E. A. Worley, Hon. Sec. of the City Division of the B.M.A., to propose the following resolution, on behalf of the Society of Members:

" That this thirty-sixth annual meeting of Fellows and Members again affirms the desirability of admitting Members to *direct* representation upon the Council of the College; moreover, this meeting requests that the President shall hereupon reply in detail to the two resolutions passed at the last annual meeting, and that he shall also state whether his Council has seriously considered any means whatever whereby the collective views of Members of the College may be ascertained on matters affecting their interest as Members."

This special request had been some time before sent to the College by me, as Hon. Sec. of the Society. It was laid before the Council on November 13, *i.e.* a week before this annual meeting; but no reply was returned. However, my note made the Council fully aware of what our demands at the annual meeting were going to be.

Our resolution was carried with only one dissentient.

The reply of the President was most disappointing. From a person with

his democratic upbringing, the Society had expected a more considerate and conciliatory attitude: Sir J. Bland-Sutton proved no better than his aristocratic (and autocratic) predecessors.

The chief points of his reply were five in number:

(1) That he had failed to notice the production of any *new* arguments.

(2) That a somewhat similar resolution had been replied to by himself last year (November 1923) and by several of his predecessors in years past, such replies having been published in each calendar of his Council. (This can be obtained from the Secretary of the College for 1*s*., postage 9*d*.)

(3) That the Society of Members had always declined to disclose the exact number of its adherents.

(4) That, judging from the small number of Members attending these annual meetings, there was either a lack of interest in the affairs of the College, or that Members had a great confidence in the management of College affairs by the Council of Fellows.

(5) That there appeared to him no need for reform, as to-day the College was in an excellent position, both numerically and financially.

N.B.—As the President's speech in answer to the Society's resolution is never made until the end of the meeting, no immediate reply from his audience is permitted or possible; so that for a twelve months at least his statements cannot be effectually refuted. They often could be, if opportunity were given at the end of the annual meeting.

However—by your Editor's indulgence and sense of fair play—I may just append a few remarks of my own on the President's five points, or excuses for his obduracy. The numbers correspond with the previous ones.

(1) Some new and convincing arguments were produced by our orators, but it is not their fault that Sir John failed to notice them. Indeed it is not really necessary to produce new evidence, until the old has been refuted or disproved. *It never has!*

(2) The replies (so called) of Sir John and his predecessors have never been satisfactory, but ever evasive, or contemptuous, or both. No President yet has ever given a thorough and complete answer to the arguments of my Society's spokesmen. Indeed, in November 1923 Sir John entered the theatre with his reply ready written in his hand—that is, *before* he had listened to the speeches of our orators at all.

(3) In my opinion, it would be a gross tactical error of a general to give the enemy's general exact information as to the number and position of the forces opposed to him. However, I can assure the President that the number of our supporters (both Fellows and Members) is much greater than he, his Council, or the Secretary of the College, imagine it to be. Indeed, a minority of the Council itself is sympathetic with our views.

(4) The number of the Members attending the last annual meeting was more truly representative than that of the Fellows. Take the total number of Fellows at 1,700 and that of Members at 16,000. Then each Fellow present represented 340 absent surgeons, whilst each Member represented only about 290. Therefore, if Members are slack in visiting their College once a year, the Fellows are relatively worse! I admit that there is amongst many Members a deplorable apathy about College affairs, but that is due to the long-continued neglect of them by the Council. Neglect and apathy are interchangeable!

N.B.—I have already noted that less than one-third of the Councillors were present: seven out of twenty-four! By a recent by-law, Councillors are not counted in making a quorum.

(5) The President himself, in reply to Dr. Redmond Roche, President of our Society, gave figures showing that, whilst in 1920 there were 484 students, in 1922 there were only 240 entering for the Conjoint Diplomas.

That shows a fall of more than 50 per cent. in two years!

The post-war rush is over, and the President must be prepared for a very considerable reduction in the number of those students seeking the diploma of M.R.C.S.Eng. in future years.

106 Richmond Park Road,
Bournemouth.

A Mathematical Parable

Miss Hilda P. Hudson, O.B.E., Sc.D., in an article on Mathematics and Eternity published in the *Mathematical Gazette*, of which we have received a reprint of unstated date (we cannot expect dates from mathematicians), writes the following interesting passage:

"There is yet to be written a companion volume to Mrs. Gatty's *Parables from Nature*, of parables from mathematics. The convergence of infinite series makes a regular Pilgrim's Progress. The whole series is a man's life, and the terms are just terms, or years, or any other periods. The first few tell you very little about him, he is still a child; as the terms pass by, the law of the series becomes apparent, and character is formed. Sooner or later he has to face the critical event of his career, to pass his convergency test, and according to how he emerges from that, so is his fate when, like all flesh, he passes to the limit. For only three score and ten or so of the terms are ever actually written down, there is no escaping the great leap to infinity. And then, if he is convergent, he reaches the sum towards which Providence has all along been guiding him by a direct or oscillating course; the remainder term, which is the amount by which he falls short of that perfection, his sin in fact, vanishes and is blotted out. But for the divergent fellow, who strays further and further away from any appointed goal, there is the piling up of the remainder, the fearful looking for the wrath to come, and no final resting-place. And, since the condition of convergence need not hold till after any finite number of terms, there is always the possibility of a deathbed repentance."

Institute of Plant Industry, Indore (A. Howard)

A new Research Institute for the improvement of crops, at which special attention will be paid to cotton and to the fundamental problems underlying the production of this crop in India, was formally inaugurated at Indore in Central India on November 24, 1924. The foundation of this new Institute has been rendered possible by the provision of a valuable site of 300 acres by the Indore Durbar, by a grant of 2 lakhs of rupees (about £15,000) for capital expenditure by the Indian Central Cotton Committee, and by an annual contribution of 120,800 rupees a year (a little more than £9,000) for current expenses in addition to the income derived from the land at the disposal of the Institute. This annual grant has been provided jointly by the Indian Central Cotton Committee and by seven of the Central India States (Indore, Dhar, Jaora, Datia, Rutlam, Dewas Senior Branch, Narsingharh and Sitamau). The control of the Institute has been vested in a Governing Body of six members with the Agent to the Governor-General in Central India as President. Three members of the Board of Governors are nominated by the Indian Central Cotton Committee, one by Indore Durbar, and two by the rest of the contributing States. The Director of the Institute will act as Agricultural Adviser to the States, and will in this way come into direct touch with the Malwa plateau, one of the most important cotton tracts in India. The experimental area, which has been leased by the Indore Durbar to the Institute for ninety-nine years at a nominal rent of £20 a year, embraces all the types of black cotton soil met with in India, and is very favourably situated for research work on crops. It is close to the city of Indore, now rapidly growing in importance as a commercial, manufacturing, and educational centre, and to the cotton mills.

The maintenance of an up-to-date library on crop-production and the training of post-graduate students, selected by the Indian Central Cotton Committee, will be features of the Institute. Mr. Albert Howard, C.I.E. (formerly Imperial Economic Botanist at the Agricultural Research Institute, Pusa) has been appointed Director of the Institute and Agricultural Adviser to States in Central India, and Mrs. Howard (formerly Second Imperial Economic Botanist at Pusa) will be employed as Physiological Botanist at Indore.

Australopithecus africanus

Remains of this important man-ape have recently been found at Taungs, eighty miles north of Kimberley in South Africa, and has been ably described by Professor Raymond A. Dart, University of Witwatersrand, Johannesburg. The discussion of the matter is not advanced far enough for us to deal with it in a quarterly publication, but an excellent statement by Professor Dart will be found in *Nature* of February 7, with comments by others in later numbers.

Notes and News

THE New Year honours list was short but unprecedented. Responsible Authority included in it a reasonable proportion of awards to scientific workers. Sir Ernest Rutherford and Sir James G. Frazer were appointed to the *Order of Merit*; knighthoods were conferred on Prof. John Adams, Professor of Education, University of London; Prof. Rowland H. Biffen, Professor of Agricultural Botany, Cambridge University; Mr. William B. Hardy, Secretary of the Royal Society; Prof. F. Gowland Hopkins, Professor of Biochemistry, University of Cambridge; Principal J. C. Irvine. Principal and Vice-Chancellor of the University of St. Andrews; Dr. T. M. Legge, Senior Medical Inspector of Factories; Mr. E. W. Petter, President of the British Engineers' Association; Dr. H. J. Waring, Vice-Chancellor of the University of London, 1922–4. Miss L. B. Aldrich-Blake, Dean of the London School of Medicine for Women, was appointed Dame of the *Order of the British Empire*.

The list of candidates selected by the Council of the Royal Society for election to the Fellowship of the Society is as follows: Dr. W. R. G. Atkins, Head of the Department of General Physiology, Marine Biological Laboratory, Plymouth; Dr. C. Lovatt Evans, Research Biologist, National Institute for Medical Research, Hampstead; Mr. R. H. Fowler, University Lecturer in Mathematics, Cambridge; Dr. F. A. Freeth, Director of Research, Messrs. Brunner, Mond & Co.; Dr. W. Gibson, Assistant Director in charge of H.M. Geological Survey work in Scotland; Dr. H. Jeffreys, Secretary, Geophysical Committee, Royal Astronomical Society; Dr. F. W. Jones, Professor of Anatomy, University of Adelaide; Dr. J. Kenner, Lecturer on Organic Chemistry, University of Sheffield; Dr. E. Mellanby, Professor of Pharmacology, University of Sheffield; Mr. J. A. Murray, Director, Imperial Cancer Research Fund; Dr. J. Proudman, Professor of Applied Mathematics, University of Liverpool; Mr. R. V. Southwell, Superintendent of the Aerodynamics Department of the National Physical Laboratory; Dr. L. J. Spencer, Assistant Keeper, Mineral Department, British Museum; Dr. R. J. Tillyard, Biologist to the Cawthron Institute, Nelson, New Zealand; Dr. R. Whiddington, Professor of Physics, University of Leeds.

Prof. C. Vernon Boys has been awarded the second Duddell Memorial Medal by the Council of the Physical Society of London.

Prof. A. S. Eddington has been awarded the Bruce Medal of the Astronomical Society of the Pacific for his distinguished service to astronomy.

The Council of the Röntgen Society has presented the Röntgen Award for the session 1923–4 to Mr. L. H. Clark, of the Physics Department, Middlesex Hospital.

The Swedish Academy of Sciences has postponed the award of the Nobel Prizes for physics and chemistry for 1924. The prize for medicine has been given to Prof. W. Einthoven, of the University of Leyden.

Mr. C. T. R. Wilson has been elected to the Jacksonian professorship of natural philosophy at Cambridge.

The prize awards of the Paris Academy of Sciences announced on December 22 last included the following: Poncelet prize for mathematics to Ernest Vessiot; La Caze prize for physics to Paul Langevin; Lavoisier Medal to J. A. Le Bel for his work in chemistry.

The following have been elected Presidents of the several sections of the British Association to be held at Southampton from August 26 to September 2: A, Dr. G. C. Simpson; B, Prof. C. H. Desch; C (Geology), Dr. W. G. Miller; D (Zoology), Mr. C. Tate Regan; E (Geography), Mr. A. R. Hinks; G (Engineering), Sir A. Denny; H (Anthropology), Dr. T. Ashby; I (Physiology), Prof. A. V. Hill; K (Botany), Prof. J. Lloyd Williams; M (Agriculture), Dr. J. B. Orr.

Among the names of the scientific workers whose death has been announced during the past quarter were: Prof. J. Bergonié, of Bordeaux, physical physiologist; Sir Maurice Fitzmaurice, engineer; Sir William Garstin, engineer; Prof. J. I. Hunter, anatomist; Dr. E. König, photochemist; Mr. A. H. Savage Landor, explorer; Dr. G. D. Liveing, chemist; Mr. R. I. Lynch, botanist; Sir James Mackenzie, heart specialist; Sir Guilford L. Molesworth, engineer; Abbé Rousselot, phonetician; Dr. J. F. W. Tatham, statistician; Prof. E. Warburg, of Stuttgart, physicist; Mr. J. A. Wheldon, botanist; Mr. W. Whitaker, geologist.

Leeds University marked the twenty-first year of its foundation and the jubilee of its parent, the Yorkshire College of Science, by series of ceremonies extending over the whole of the third week in December. The outstanding events were the sermon preached by the Archbishop of York, Lord Balfour's speech at the University dinner, Sir Michael Sadler's review of the history of the University, and Sir Berkeley Moynihan's address dealing with surgical work in Leeds. The organisation of the various functions was most successful, and the representatives of the various universities, societies, and professional bodies who had been invited had a most interesting and enjoyable experience.

The inaugural address delivered by Senatore Marconi to the Society of Arts dealt with the development of short-wave radio-telegraphy. Senatore Marconi commenced his systematic study of short-wave communication in 1916. Experiments over comparatively short distances in 1920–21 with a 100-metre wave showed that, under favourable conditions, good reception was obtainable with very low power transmitters. Further tests between land stations and the yacht *Elettra* in 1923 showed that very considerable distances could be covered and that the nature of the intervening ground does not materially affect the range. In the spring and summer of 1924 the short-wave station at Poldhu maintained successful communication with places as distant as Buenos Ayres and Sydney, using only 20 kilowatts, provided that the whole, or at least the greater part, of the track was in darkness. In August last a further series of tests were made from the *Elettra* with two aims in view: (*a*) to ascertain the advantage gained by using a reflector at the transmitting station, (*b*) to find means of maintaining contact during daylight hours. As a result it was found that the reflector functioned in the manner indicated by previous calculations and that the range increased very rapidly as the wave-length was reduced. In October transmission experiments were made on a 32-metre wave between Poldhu and a number of stations in both northern and southern hemispheres, *e.g.* Montreal, Rio, and Sydney. Only 12 kilowatts were used, and communication was maintained throughout the whole 24 hours with the two first-named stations and for

$23\frac{1}{2}$ out of the 24 hours with Sydney. In December similar success was attained with stations in India and South Africa. One notable advantage of short-wave transmission is its freedom from interference by atmospheric disturbances.

The most interesting feature of the Physical Society Exhibition held at the Imperial College last January was the demonstration of the De Forest phonofilm given by Mr. C. F. Ewell. In this process the sound record appears as a series of fine lines at the edge of the ordinary cinema film and is thus adapted for use with a standard projector. The original sounds are received by a microphone; this controls the light given by a specially designed helium lamp which illuminates the edge of the undeveloped film through a fine slit. On reproduction light passing through this edge falls on a thalofide screen, the electrical resistance of the thalofide varying with the intensity of its illumination. This variation of resistance produces a corresponding variation of the current which, after several stages of valve amplification, actuates a loud speaker. The reproduction is quite good and the process would seem to be sufficiently perfect for exhibition in the cinema hall.

A pleasant feature of the exhibition was the marked change in the attitude of the demonstrators in charge of the exhibits; courteous explanations were readily obtainable at all the stands which the writer visited. The unpleasant feature—a very minor one—was provided by the firm exhibiting a wireless set attached to a loud speaker made by another firm—the combination producing a loud noise from which neither can have gained any benefit.

The second edition of Prof. R. A. Millikan's book, entitled *The Electron* (University of Chicago Press, price $1.75), contains a very clear account of the phenomenon known as the Compton effect. Accepting Einstein's hypothesis of light quanta, Compton supposes that when such a light-quantum meets a *free* electron the impact is governed by the usual laws of impact for material bodies. Whence it immediately follows (*a*) that after impact the quantum must have less energy than before and therefore a smaller frequency, and (*b*) that, since the mass of a quantum $\left(\frac{h\nu}{c^2}\right)$ is comparable with that of an electron, it can only give a small fraction of its energy to the electron, so that the complete transfer which does occur in photoelectric effects must represent interaction between light and bound electrons whose mass, from the point of view of the momentum equation, will be that of the atom to which they are attached.

The change in the wave-length of the scattered radiation indicated by (*a*) above was verified by Compton using the characteristic X-rays of molybdenum for the incident radiation and graphite as the scattering substance. His results were verified by Ross using the same substances and by workers in Millikan's own laboratory with aluminium instead of graphite. Prof. Millikan has brought his book right up to date; in fact some of the references are as late as June 1924, while the preface is dated May 18.

Evidence is accumulating that, in certain circumstances, helium may combine with mercury, iodine, and other elements to form stable helides. In a letter to *Nature* (December 13, 1924), J. J. Manley, writing from the Daubeny Laboratory, Oxford, describes the formation of a mercury helide, in the presence of an electric glow discharge, which is stable at temperatures below a bright red heat. In another letter to the same Journal (January 3, 1925), E. H. Boomer, writing from the Cavendish Laboratory, describes results which he has obtained by mixing helium with the vapours of mercury, iodine, sulphur, and phosphorus at liquid air temperatures and exposing the mixture to electron bombardment. Solid substances are formed which decompose at definite temperatures: for mercury and iodine about $-70°$ C. and for sulphur and phosphorus $-125°$ C.

Prof. A. A. Michelson announced the results of his first measurements of

the velocity of light with his new apparatus at one of the Franklin Institute Centenary Celebration meetings. The work has been carried out at Mount Wilson, the distant station being established twenty-two miles away on Mount San Antonio. The chief difference between the present experiments and those made by the same experimenter in 1880 lay in the use of an octagonal mirror which permits the use of higher speeds and makes it possible to reflect the light to the distant station from one face and receive it back again on another, thereby transferring the measurement of the angular deflection of the ray to the mirror whose angles were correct to one part in a million. The mean result came out as 299,820 kilometres per second in vacuo with an uncertainty of one part in ten thousand. It is proposed to repeat the work this year with an improved method for measuring the velocity of the mirror whereby the uncertainty should be reduced to a fourth or fifth of its present magnitude.

The most interesting feature of the Ninth Annual Report of the Committee of the Privy Council for Scientific and Industrial Research (H.M. Stationery Office, Imperial House, Kingsway, W.C.2, price 3*s.* net) is the account which it contains of the progress of the Industrial Research Associations. It will be recalled that these associations were granted certain annual payments from the Council for a period of five years from the date of their formation. During the year covered by the Report nine associations have reached this five-year limit, and only in one case, that of the British Portland Cement Association, do the funds raised by the industry itself suffice for the continuance of effective research; the other associations require further aid from the Million Fund. Accordingly, the Council has caused a thorough and expert examination of the work and administration of these associations to be made by visiting committees. In all but one instance the reports of these committees have been favourable and further grants will be made, *e.g.* the British Scientific Instrument Association is offered £10,000 a year for the next five years subject to the condition that the trade shall raise a total sum of £9,000 during this period.

The exception is provided by the Glass Research Association, concerning which the Report states:

"The visiting committee of the Glass Research Association, though appreciative of the improvement effected by the Association in the general outlook of the industry and in its factory technique, were not so favourably impressed with the scientific results achieved. Moreover, the present financial position of the industry affords no prospect that the firms will be prepared to subscribe funds sufficient to justify the continuance of a research association for this trade. As a result we propose to consider what other steps should be taken to provide for glass research."

It should be pointed out that this association appears to be the only one to have as Director a person without academic or professional qualification.

We have received from Messrs. Ernest Benn Ltd. the two volumes of the magnificent work on *Electrical Measuring Instruments* written by Dr. C. V. Drysdale and Mr. A. C. Jolley, A.M.I.E.E. (price 55*s.* net each volume). The first volume deals with the general principles of design, the properties of electrical materials, and with commercial and indicating instruments; the second with supply metres, induction instruments, and A.C. instruments generally; and, finally, with test-room equipment. The books represent an immense amount of labour and research, and form a most welcome and notable contribution to our very scanty literature of scientific instruments. The diagrams are unusually clear, and the whole production one of which both the authors and publishers are entitled to feel very proud. We hope to review the two volumes at length in the next number of SCIENCE PROGRESS.

The Halley Lecture last year was delivered by Prof. Joly, of Trinity College, Dublin, who chose as his subject *Radioactivity and the Surface History*

of the Earth. It is published by the Clarendon Press, Oxford (price 4*s.* net). After a review of the theory of isostasy, Prof. Joly considers the part radioactivity must have played in the past history of the earth's crust and its effect in future. The heat generated by the radioactive substances in the depths of the crust is not all escaping from the surface, but is being conserved so that the present conditions cannot maintain indefinitely. They must give way to a temporary change which permits of the discharge of the heat. The greatest of such changes which the earth has to our knowledge experienced formed the Archean era when at least two great sedimentary series of pre-Huronian Age were overwhelmed, their total thickness being not less than twenty miles. The solidifying period of the substratum attending the last great terrestrial revolution appears to have persisted for at least five million years since the Pliocene period and may not yet be quite complete. During the preceding stages of this revolution a split across Africa formed the Great Rift Valley, and the great mountain ranges of our present age were formed—the Western Alps, the Himalayas, and the Rockies. A number of other periods of partial continental flooding and limited diastrophism have occurred. Schuchert has, for example, defined eight in the North American Continent since the Archean.

We have received from Mr. A. L. Curtis, of the Westmoor Laboratory, Chatteris, a booklet describing the various varieties of sand he is prepared to supply for industrial purposes partly from his own quarries and partly from imported material. The list comprises no fewer than fifty different kinds and invites inquiries for other special sands not mentioned in the pamphlet, which is both novel and interesting.

Among the recent publications from the Bureau of Standards, Washington, U.S.A., are the following: *Circular No.* 100, Nickel and its Alloys, containing a comprehensive account of these substances in all their aspects and including some 1,300 references; *Circular No.* 101, The Physical Properties of Materials. I. Strengths and Related Properties of Metals and Wood, dealing with the tensile properties, hardness, and thermal properties of these substances under all the conditions of treatment form and temperature likely to arise in practice; *Circular No.* 154, National Standard Petroleum Oil Tables, containing tables for reduction of observed specific gravities, etc., to those at 60° F.; and *Scientific Paper No.* 492, Formulas, Tables, and Curves for Computing the Mutual Inductance of Two Coaxial Circles, a paper whose contents are sufficiently indicated by its title.

Perhaps by way of compensation to officialdom for the loss of its red tape, the Bureau has issued a specification for the Government Red Ink, with instructions for taking samples and comparing them with the standard, which consists of 5·5 gm. of crocein scarlet 3B, Schultz No. 227, dissolved in 1,000 c.c. of distilled water.

The year's record of H.M. Patent Office gives a promising indication of better trade. Patent applications totalled over 30,000 during the past twelve months, and, although this figure is 1,000 less than that for 1923, the latter period showed a decrease of over 3,000 as compared with 1922.

Prof. D'Arcy Thompson, F.R.S., contributed an article to the Royal Society on January 22 regarding the Thirteen Semi-regular Solids of Archimedes, and on their Development by the Transformation of Certain Plane Configurations.

The abstract of the article states that: "The thirteen isogonal non-isohedral solids, whose discovery is attributed to Archimedes, are shown to stand in close relation to the ten regular plane-repeating patterns or 'nets' first described by Kepler, which consist of regular and identical sets of congruent polygons. If, in the table of indices giving the number of triangles, squares, etc., found at each junction (or node), the *order* of an index be successively reduced, we pass accordingly from the plane polygonal assem-

blage to some one, and then to another, and so on, of the indices which characterise the several polyhedra. This transformation may be performed mechanically, by constructing a hinged net, removing (*i.e.* replacing by fenestræ) the polygons of a certain order, and allowing those which remain to slide over and so overlap one another. These latter are thus reduced in number, while the fenestra becomes a polygon of lower order than before; and the indefinitely extended plane sheet folds up into the corresponding polyhedron. This process may be continued; and in this manner the whole thirteen Archimedean bodies (besides three of the Platonic bodies) may be produced from the ten Keplerian repeating patterns."

The Zoological Society of London and the London School of Hygiene and Tropical Medicine have conjointly established a Fellowship of Comparative Pathology and an Aquarium Research Fellowship, both being full-time appointments. Drs. H. H. Scott and F. R. Stowell have been appointed to each post respectively. Much work has already been done on the diseases and parasites of animals which have died in the Zoological Gardens, especially by the late Dr. Plimmer, F.R.S.

The *Oxford Forestry Memoirs*, No. 3, 1924, contains an interesting study of the Water-mark Disease of the cricket-bat willow (*Salix cærulea*) by W. R. Day, B.A., B.Sc., mycologist of the Imperial Forestry Institute, Oxford. The disease, which renders the wood useless, is judged to be due to *Bacillus salicis*, n.sp.; and the author recommends for prevention the selection of less marshy soils, and more widely interspaced planting, with destruction of infected trees.

The Ministry of Agriculture and Fisheries publishes a *Report on the Possibilities of Aerial Spotting of Fish*, by A. C. Hardy, M.A. (*Fishery Investigations*, Series II, Vol. VII, No. 5, 1924). The paper describes the results of five seaplane flights in the Southern North Sea and in the western channel; but these results were not so promising as those of similar flights of French observers in the Bay of Biscay and off St. Malo some years ago. The conditions required for the detection of shoals of surface fish from aeroplanes are (1) a calm surface, (2) clear water, and (3) strong sunlight, and few cloud-shadows—all rather uncommon in British waters.

"The South-Eastern Naturalist" (*Proceedings and Transactions of the South-Eastern Union of Scientific Societies*, London, 1924) contains many interesting articles—Sir Richard Gregory deals well with the subject of Science in Civilisation—we may admit the science, but scarcely the civilisation. Dr. A. F. Tredgold contributes a very complete yet calm discussion of Evolution and Eugenics, and Mr. Ray Palmer, F.E.S., F.Z.S., some singularly interesting "Observations on the Social Life of Humble Bees." It would be somewhat difficult to disprove the thesis that humble bees are to-day little less "civilised" than men are.

Dr. W. E. Haworth, late Acting Director of the Laboratory, Dar-es-Salaam, publishes a very complete study of *Mosquitoes and Coconut Palms* in the Transactions of the Royal Society of Tropical Medicine, Vol. xviii, No. 4, October 1924. Many species of the insects, sometimes including *Anopheles*, were found breeding in the small quantities of water which often exist in the axils between the stems of the leaves and the stem of the palm, and elsewhere. The author doubts whether rainfall or even dew are the only sources of this water; and we may draw attention to similar difficulties occurring in regard to the water found in the node-cups of the common teasel, *Dipsacus sylvestris*, in this country, as discussed by Mr. Robert Paulson, F.L.S., F.R.M.S., in the number of *The Southern Eastern Naturalist* mentioned in the previous note. That mosquitoes often breed in rot-holes in trees and in the axils of wild pine-apples, sugar-canes, and elsewhere has long been known, and palms have long been suspected, though probably never investigated so carefully before. Whether the *Anopheles*-output of such breeding-places is of sufficient

consequence compared with terrestrial breeding-places must depend upon local conditions. The same publication contains an article by I. J. Kligler, Ph.D., B.Sc., on the *Flight of Anopheles Mosquitoes*, in Palestine. By the word *flight* is meant the radius of dispersion from the breeding-centre, and the author does not appear to be well acquainted with the large previous literature of the subject, including mathematical studies of it. He finds "flights" up to 5 kilometres, but thinks that 2·5 kilometres are sufficient for routine control in the areas dealt with by him. Several villages were cleared of malaria by dealing with breeding-pools from 1·5 to 2·1 kilometres distant.

Prof. Sir Robert Philip, University of Edinburgh, is to be congratulated for his fine and complete address on the *Effects of the Anti-Tuberculosis Campaign* at the International Union against Tuberculosis, Lausanne, August 1924 (*Edinburgh Medical Journal*, September 1924). During the last fifty years there has been a steady decline in the tuberculosis-mortality in Scotland from 13,207 deaths in 1870 to 5,786 deaths in 1923, with similar diminution in England and the United States. This has been due to various "co-ordinated activities" against the disease; and, as he remarks, "Reference to the statistics of countries where anti-tuberculosis organisation has been less adequate and less prolonged fails to reveal a similar disparity between the drop in the general death-rate and the drop in mortality from tuberculosis." He disproves several erroneous contentions, such that this drop is due to an advancing natural immunisation against the disease—since it has not occurred in countries where adequate measures have not been taken. Neither is the disease dependent on racial characters—because it is universal among all civilised races: thus Austro-Hungarians have a very large tuberculosis-mortality in Austria and Hungary, but a very low one in the United States. Victories against diseases are probably the greatest victories ever won by the human race; and they have generally been due to medical and other scientific men, with little assistance from the public, from public funds, politicians, universities, or other institutions for the alleged "advancement of science."

Sir Arthur Schuster, F.R.S., is publishing some very interesting reminiscences of eminent men of science—in *Nature* every week from January 10 last.

The cause of "dexterity" is carefully discussed by Beaufort Sims Parson in a well-written little book called *Lefthandedness* (Macmillan, New York, 1924). After examining many theories, the author adopts the homology of "one-eyedness" in sighting. There are forty-eight pages of bibliography—somewhat too much for the subject.

We have received the prospectus of an *International Bibliography of the War* suggested by Mr. Alex. J. Philip, M.B.E., F.L.A., Borough Librarian, Gravesend. It states that "a sinking fund of some £5,000" for the scheme will be required, and asks for promises of subscriptions pending the appointment of Trustees. We are told that the Bibliography is likely to comprise a million cards, which will cost £5 per thousand, and that two hundred cards have already been completed, and material for nearly half a million is in hand.

We have received the first number, dated October 1924, of a new Indian Quarterly called *Yoga Mimansa*, edited by S'rimat Kuvalayananda, address Lonavla, Bombay. Apparently the periodical will be devoted to studying, "according to the modern scientific methods, the great Yogic culture of India in its different aspects." This first number is printed in English, and contains many good photographs of certain curious religious (or physical) exercises.

Sir Archibald Garrod, Regius Professor of Medicine at Oxford, delivered the Harveian Oration at the Royal College of Physicians of London last year on "The Debt of Science to Medicine," and mentioned many points of interest in the history of science and medicine.

ESSAY-REVIEWS

THE PRESENT POSITION OF BIOLOGY AMONG THE SCIENCES. By J. H. Woodger, being a Review of **The Study of Living Things**. By E. S. Russell. [Pp. x + 139.) (London: Methuen & Co. Price 5s. net.)

In the days when science and philosophy were alike young, when the data of the one and the systems of the other had not yet become so numerous as to be beyond the compass of a single mind, it was possible for science to supply data for philosophy and for the latter to guide the footsteps of science—pointing out new paths for it, and checking its conclusions in the light of a broader point of view.

In the present stage of specialism not only has science developed an antagonism towards philosophy, but the several branches of science have themselves become so unwieldy that a stocktaking of the general achievements even of a single branch becomes increasingly difficult, and a detached, unprejudiced view of the mutual relations of several branches, from the standpoint of an exponent of one, becomes almost impossible.

Nevertheless inroads have been made from one science into another, leading to far-reaching conclusions being drawn concerning not only the mutual relations of these sciences, but also about the whole universe of being and the problems of philosophy generally.

At the present day biology is in a unique position. On the one hand its subject-matter has been made the object of physical and chemical inquiry, and on the other the methods of psychology have been applied to living things in the attempt to study animal behaviour as an objective manifestation of mind.

Of these two external influences the first has been much the more far-reaching in its effects—largely because it has been exerted over a very much longer period than the second. For of the natural sciences physics was the first to take definite shape and to gain prestige by its positive achievements, and has, in consequence, served as a pattern for the younger branches.

The influence of the physical sciences on biology throughout its history is easily discernible. The cleavage of biology into its physiological and morphological branches has no doubt largely been responsible for the introduction of physical notions. Physiology has been purely analytical. It has been compelled to consider each organ separately, and has been so engrossed in its task that the organism as a whole has been lost sight of, and, in consequence, the very existence of a relation between the *organism as a whole* and the environment has easily been forgotten. To the physiologist the environment is nothing more than a place between which and the organism exchanges of energy and material take place. This piecemeal treatment of the organism has lent itself very readily to investigation by physical and chemical methods, and to interpretation in terms of physical concepts.

When we turn to the course followed by the morphological branches, we find that the point of view is essentially the same. They have accepted the conclusions of physiology and have treated form statically and geometrically. With few exceptions adaptation has been regarded more as presenting

pitfalls for the unwary morphologist in the search for homologies than as constituting a fundamental biological problem in itself.

The study of the relation between the organism and the environment, and between one generation and the next, which received a special impetus with the coming of the doctrine of evolution, was naturally conducted on the foundation physiology and morphology had laid. Weismann's corpuscular theory of heredity had its birth in consequence, and the selection hypothesis itself accounted for adaptation in a purely mechanical way. And these explanations were accepted.

There have not been wanting, however, those to whom the organism has presented activities and aspects which were not to be explained away along these lines. Present-day biology has witnessed the rise of the experimental study of three problems: those raised by (1) the process of individual morphogenesis (*entwicklungsmechanic*); (2) the phenomena of regeneration; and (3) animal behaviour. These three problems (which are beyond the scope of traditional physiology, and are not even mentioned in physiological books) have all tended to focus attention on aspects of living things which have previously received scant recognition, and have raised difficulties in the way of the generalisations which satisfied the older physiology and morphology. All three deal clearly with the behaviour of the organism as a whole, but the term "behaviour" has not quite the same meaning in each case. In the investigation of animal behaviour we are dealing with the activities of the fully formed organism towards the outside world, but in the first and second cases we have to do with the behaviour of the organism when it is concerned, as it were, with itself, that is to say with the process of reaching the necessary functional equipment to enable it to deal adequately with its surroundings.

Up to the present these investigations have chiefly been conducted by different workers approaching these problems from different angles, in isolation, and without much regard for each other's conclusions, or for their bearings on traditional conceptions. For while experimental embryology has absorbed the attention of a considerable proportion of modern biologists, the study of animal behaviour has been curiously neglected. In the past it has been relegated to those "spider-hunters and hay-naturalists" of whom Huxley spoke so contemptuously. With one or two well-known exceptions its scientific investigation has been left to those psychologists who have been led by the doctrine of evolution to seek in animals some foreshadowing of the human mental processes which had previously been their sole concern. Their approach has, consequently, been from a totally different angle from that of the biologist, and this invasion of psychology has so far had little or no influence on biological thought, nor have these results been correlated with those reached by the students of ontogenetic and regenerative behaviour.

While it is useless to speculate about the probable influence this new incursion into biology will have, it is desirable to consider what it implies and how it conflicts with the fundamental conceptions reached under the influence of the far older and better established physical sciences.

It is the special characteristic of the physical sciences that they need take no account of the fact of the objects of their investigations being *known*; consequently for them the fundamental antithesis between the knowing mind and the object known need never arise. None the less the existence of this antithesis is tacitly acknowledged in the very terms used to name the several branches of physics, light, sound, etc., for these are purely subjective manifestations, and physics is concerned not with them but with the physical motions which are their usual antecedents. Possibly, in the light of the most recent physical conceptions, framed in accordance with the doctrine of relativity, the above statement would have to be still more modified, but for our present purpose no such recent developments need be

considered, for it is not modern physics from which biology has borrowed its methods and fundamental conceptions, but the physics of some forty or fifty years ago.

When we turn to psychology, we see that it is concerned with the other side of this antithesis, with the knowing mind. But it studies the mind not only as *knowing* objects, but also as *feeling* and *striving* in regard to them, and " environment " has, therefore, a meaning for it which is not to be found in traditional biology.

In view of these considerations it seems clear that the influence of psychology on biology will be very different from that exerted by the physical sciences. The consequence of the importation of physical and chemical concepts into biology has been the widespread adoption of the assumption that living organisms are nothing more than vastly complicated colloidal aggregates of carbon-containing molecules, and the hope of being able to describe *all* the phenomena of life in terms of the interchanges taking place between these aggregates and the less complicated ones occurring in the inorganic environment has frequently been expressed by eminent physiologists with all the assurance of unquestioned conviction.

To the physiologist with his analytical procedure no difficulties have arisen to obscure this hope, in spite of the fact that so far no single vital process has been satisfactorily explained in this way. We are told that it is only a matter of time, that this is in consequence of the complexity of the processes and not due to any fundamental difference in their *nature* from physical ones. But to the biologist who is confronted with the refractory phenomena presented by morphogenesis, regeneration, and behaviour in the narrower sense, there are two courses open : either to break away entirely from the mechanistic tradition and its assumptions, or to find some means of avoiding the new difficulties, and of explaining them away on a mechanistic basis. The latter course is followed by the various corpuscular theories of heredity and development, by the neurological attitude towards mental processes, and, among psychologists themselves, by the " behaviourist " school. All these attempts agree in their utter failure not merely to explain, but even to take account of, the very difficulties that modern biology is confronted with, and they tend, therefore, to divert attention from them. All agree, moreover, in resting in the long run on the assumptions of philosophical materialism, and philosophical materialism can offer no explanation of that persistent striving after ends, which especially characterises the behaviour of the organism as a whole.

An attempt has recently been made by Dr. E. S. Russell to show how pyschological concepts can be utilised to form a new starting-point from which to regard vital phenomena. For the study of behaviour especially forces upon our attention those characteristics of life which escape elucidation by the concepts of current physiology and suggests a means whereby psychological concepts can be introduced which may also make it possible to bring into line those difficulties met with in the study of developmental and regenerative processes, for these difficulties seem to arise from the same characteristic vital properties that present themselves in the study of behaviour.

The chief obstacle to this attempt is that arising from the question of terminology. We are in the position of the early comparative anatomists when, for example, they applied the nomenclature of the human brain to that of the fish. This difficulty is particularly great when terms which have long been used in describing attributes of the fully developed human mind are applied to what seem to be their phylogenetic precursors in the lower organisms, for they imply qualities in those precursors which we have no reason to believe them to possess, and a loop-hole is thus offered to the criticisms of those who wish to maintain the mechanistic interpretation of behaviour.

The characteristic of vital activity which most strikingly distinguishes it from the processes of the inorganic world, which is most manifest in be-

haviour, but which can also, Dr. Russell believes, be recognised in developmental processes, is that of persistent striving after ends in accordance with deep-seated impulses. In conscious human life these have long been called conative impulses. For their manifestations in the lower organisms Dr. Russell, following Professor Nunn, uses the term *hormic* impulses. In human mental life, again, these impulses are guided and brought into proper relation with environmental events through perception. In the lower animals, even among the Protozoa, there is reason for believing that the influence of the environment is not normally *direct*, as it must be on the mechanistic view, but indirect. The organism under natural conditions responds not to stimuli but to the whole situation presented to it, so far as it is relevant to the hormic impulse which is dominant at the moment. For this activity which intervenes between the stimulus and response Dr. Russell uses the term *reception* proposed by Professor James Ward.

Of responses themselves Dr. Russell distinguishes those manifested in developmental and regenerative phenomena as *morphoplastic responses* from those seen in behaviour in the narrower sense, which he calls *behaviour responses*. These two principal classes of essentially vital phenomena are thus brought under the general conception of responses.

The same characteristics which are found in responses of the whole are also seen in the activities of the parts, but are, in this case, much more "mechanised" and subordinated to the activities of the whole. For these part responses Dr. Russell reserves the term *functions*.

Finally, the functions are themselves dependent upon the material or "enabling" conditions through which they are manifested. There is thus a "hierarchy of action"—responses, functions, material conditions. The first and second embrace the sphere of biology, the third alone admits of physical and chemical investigation. Dr. Russell contends that responses can never be completely described in terms of functions, nor functions in terms of their chemical and physical enabling conditions, but rather that we should seek to interpret responses in terms of the fundamental hormic impulses of self-maintenance, development, and reproduction, and to show how the functions are co-ordinated to serve these ends. The rôle of physics and chemistry is the useful but limited one of investigating the limiting and enabling conditions of those functions.

Present-day biology is thus offered an alternative to the consequences of philosophical materialism, namely, to approach its problems from a new standpoint, studying vital activity as an activity *sui generis*, without bias concerning its reducibility to physico-chemical processes, but employing provisionally the psychological concepts outlined above, recognising that there are no grounds save purely speculative ones for accepting the "ultimates" of physical science as necessarily its own also.

We might then divide the natural sciences into: (1) physical, and (2) psychical, the latter being further divided into biology and psychology. Biology itself would be further divisible into:

(1) The study of organ and cell responses (Dr. Russell's functions), which would include physiology after bio-physics and bio-chemistry have been removed from it, since these are not biology but branches of physics and chemistry.

(2) The study of responses of the organism as a whole, which would itself have two subdivisions:

(*a*) The study of morphoplastic responses, including the functional study of ontogeny and regeneration, and

(*b*) The study of behaviour responses, which merges into psychology.

Dr. Russell claims that his "functional biology," for which the ultimate concept is "not matter but activity," will avoid the difficulty of the interaction of mind upon matter, but it is difficult to understand his position at

this point. For if we recognise the antithesis between the knowing, perceiving, or "receiving" individual and the objects of its perceptions this difficulty must arise. The question must ultimately be asked: What is the relation between the fundamental hormic impulses or "Trieben" and the "material conditions" through which they are manifested? It seems that we are forced to some such form of dualism if we reject the monistic materialism on the one hand and some form of idealism as that which Dr. Haldane appears to adopt on the other. Dr. Russell definitely rejects the former alternative and makes no explicit pronouncement about idealism. The best answer to the objections brought against "interactionism" appears to be that of Lotze—that we know too little of the action of one thing upon another to feel any surprise that we cannot conceive of the action of mind upon body. This problem, however, is one for philosophy, not for science, although the human mind does not rest easily on a purely agnostic position, and a science without a metaphysical foundation is impossible, as is abundantly illustrated by the existing theories.

Whatever view we finally adopt, we cannot escape the contrast between the thing perceived and the perceiving subject, and the recognition of this fact will perhaps point the way towards a means of escape from the shortcomings and limitations of the purely materialistic point of view. Dr. Russell's book will do a good service in drawing attention to these things, especially as it comes at a time when many are becoming impatient with prevailing views, and are beginning to realise that such views have held undisputed sway too long for the healthy and vigorous growth of biological science.

THE SUN AND THE WEATHER, by E. V. Newnham, B.Sc., Meteorological Office, London, being a review of **World Weather**, by H. H. Clayton. [Pp. 393 + xx, with xv plates and 265 figures.] (New York: The Macmillan Co., 1923. Price 18*s*. net.)

It is generally admitted that both climate and weather are intimately related to effects of solar radiation upon the earth and its enveloping atmosphere, but that the connection is an exceedingly complicated one; the problem presented by the general circulation of terrestrial winds, the distribution of atmospheric pressure, temperature, pressure, etc., over the whole globe, is a problem in applied physics simply, but one of extraordinary complexity.

Until recently, although the supreme importance of the sun has not been overlooked, it has been customary to regard the stream of solar radiation as constant for all practical purposes. The physicists who first attacked the problem of measuring the intensity of this stream accordingly used the expression "solar constant" for the total number of calories of heat received on 1 square centimetre of the earth's surface per minute when such a surface is normal to the sun's rays and when allowance has been made for the amount of heat absorbed or scattered by the atmosphere. Langley, however, with the aid of his "bolometer," showed that the "solar constant" is not really constant, and Abbot and others have developed Langley's methods and obtained a long series of measurements of the irregular fluctuations of the solar radiation. American meteorologists have devoted some attention in recent years to the question of the effect of such fluctuations upon terrestrial weather, and the author of the book under discussion, in particular, has devoted much time to this subject. An all too brief account of his researches appears in the book under review, and under the title of "The Sun and the Weather," constitutes the tenth chapter of *World Weather*. It is the purpose of this essay to test the validity of the conclusions reached after a more careful consideration of the spurious connections that can appear between variables which are subject to irregular periodicity of such a kind

that successive observations of any one of the variables are positively correlated—as is the case generally with meteorological observations and apparently with observations of the solar radiation, when these are made at intervals of less than about a week. The great importance of knowing whether one can draw any conclusions as to future weather from observations of the solar radiation is taken to justify such an inquiry.

Clayton employs the method of correlation, and uses the formulæ that have been worked out by mathematical statisticians for determining the magnitude of the errors that can arise in correlation coefficients, means, etc., from errors of observation and paucity of observations. Unfortunately, as has already been hinted, there is in meteorological work a source of error in the application of these formulæ, which was pointed out by W. H. Dines some years ago. Its exact nature can be illustrated by a practical example of a determination of a mean. Suppose we require to know the mean temperature for a given month at a place where the climate is not changing in a systematic way, in other words, where the mean temperature in that month may be very different in different years but does not tend to increase or decrease on the average with the lapse of centuries. Suppose, further, that in the long run there is no appreciable difference between the mean temperature for the middle day of the month and the mean for the whole month. The mean might be determined by averaging the means for each day of a month for one particular year (being, therefore, the mean of 31 individual means if there are 31 days in the month) or it might be determined by averaging the mean temperatures for the middle day of the month for 31 different years. Now in the latter case experiment would show that the mean so obtained would 'on the average' differ from the true mean by the amount which the statistical formula for the error in a mean due to paucity of observations would indicate. In the case of the 31 successive daily means, however, far larger errors would occur. This is so because the mean temperature of one day is not independent of the mean for the next day and the 31 observations are statistically equivalent to a much smaller number of well-spaced "independent" observations. This principle has been overlooked by Clayton, and in consequence many of the relationships claimed cannot be accepted unless they are confirmed by a far greater number of observations. I will return to this subject after considering the question of the accuracy of the actual values of the solar radiation.

Clayton admits that the process of correcting for absorption and scattering of the radiation by the atmosphere is a difficult and uncertain one. It is no doubt because of the large errors arising in this process that a correlation of only + 0·49 was found for simultaneous observations of the solar radiation made at Mount Wilson (California), and Calama (Chile). It is quite possible that some condition of the upper atmosphere may cause errors in the corrections for scattering, etc., which tend to cause the estimated radiation at the two places to be both above or both below the true value on a particular day, these errors being in addition to the ordinary "casual" errors of correction; but even if this is not the case the coefficient indicates a high degree of uncertainty. The extent of this uncertainty can be gauged from the fact that there is a correlation greater than 0·49 (actually between 0·5 and 0·6), between readings of the barometer made two days apart in England, yet it is considered important that readings of the barometer for the construction of synoptic charts should be made within ten minutes of the correct time. It appears safe to conclude that no close connection can be established between daily estimates of the solar radiation liable to such uncertainty and daily changes in the weather. Similarly with rather longer periods, for even when five-day means of radiation are correlated for the two places the correlation coefficient is only + 0·63.

The connections found between the solar radiation and various

meteorological elements may now be considered. On page 220 a series of curves are given, showing five-day means of the solar radiation and five-day means of temperature in various parts of the world for a period of about ten weeks. Now it is important to notice that when five-day means are employed the number of observations is from the statistical point of view only $\frac{10 \times 7}{5} = 14$, even if these means can be regarded as "independent" in the statistical sense, and it is quite certain that they cannot be regarded as independent, for the actual curve of solar radiation shows that when the radiation is high on one five-day mean it is generally high on the next, and vice versa. It is well known that, unless two variables are very highly correlated, correlation coefficients based on less than 14 observations cannot be accepted, owing to the high "probable error" due to paucity of observations. In this case the actual coefficients are just such as would be expected assuming that there is no genuine connection, *i.e.* they are small, rarely exceeding $\mp 0{\cdot}5$, and they are sometimes + and sometimes −, *i.e.* sometimes "direct" and sometimes "inverted," and sometimes, in the author's words, "partly inverted and partly direct," *i.e.* nearly zero. Similar reasoning can be applied to the other investigations of temperature given in succeeding pages, and the argument gathers force when ten-day means are used instead of five-day means.

It is not suggested that there is in reality no connection between variations of the incoming stream of radiation and variations of temperature from the normal for the season, but it appears that such a connection could only be established with the aid of a much longer series of observations or by determinations of the solar radiation of a much higher degree of accuracy.

Clayton goes on to consider pressure and rainfall, but does not always give sufficient information about the number of observations to make it possible to estimate exactly the reliability of the relationships claimed; it seems clear, however, that in general the paucity of "independent" observations makes it difficult if not impossible to regard the vague and imperfect relationships as being of any significance.

After all that has been said on the subject of the uncertainty of the determination of the solar radiation, it may be of interest to quote one or two of the statements made in a summary of the results claimed to have been obtained from these very inexact determinations. On page 264 it is stated that "every increase of solar radiation lowers the pressure within the tropics and increases the pressure in latitudes 40° to 60°, whether the increase of solar radiation be for a few days, for months, for years, or for long periods as in the sunspot period." On page 267 it is further stated that changes in the solar radiation alter the intensity of the weather anomalies associated with the "centres of action" in the atmosphere and shifts those centres north or south of their mean position and that "there is a lag in the solar effect near the centres of action proportional to the duration of the solar change. If the duration of an increased solar radiation is no greater than a day the lag at the centres of action is only a few hours, if the high or low radiation continues for several days the lag may amount to two or three days, if the period is near the length of a year the lag is nearly a month, while if the period of change is eleven years the lag may amount to many months or to a year," etc., and again "from the centres of action a series of waves move outward, in general passing from higher to lower latitudes, or in other words moving from pole towards equator with a velocity inversely proportional to the interval of time between the maxima at any given place, etc."

Were it possible to substantiate conclusions of this kind a very important advance would be made in meteorology. The attempt might well have been set out in a work devoted exclusively to the task, instead of in one

chapter of a general meteorological work. Judging from the very abbreviated account given it seems evident that most of these statements are little more than guess-work, and one cannot even be sure that relationship of any kind whatever has been established between the intensity of the radiation received from the sun and the weather of any region of the world. The suggestion may be made that concentration upon portions of the subject might give useful results, in particular the determination of the probable error of an estimate of the solar radiation and the computation from this of the maximum correlation that can possibly be expected between apparent variations of the radiation and different meteorological elements. The way might then be opened to definite advances in meteorology.

TO BE, OR NOT TO BE. By R. R., being a review of **Tantalus, or The Future of Man.** By F. C. S. Schiller, M.A., D.Sc. [Pp. 72.] (London: Kegan Paul, Trench, Trübner & Co., 1924. Price 2*s.* 6*d.* net.)

In July last year we reviewed *Dædalus*, or *Science and the Future*, by J. B. Haldane, and *Icarus, or The Future of Science*, by Bertrand Russell. These small books were each worth many modern novels; and the new member of the family is not inferior to them. Let us hope for yet another called, let us say, *Hercules, or The Man of the Future*—or, at least, *Sisyphus* (who chained Death); or *Ixion* (who aspired to the Queen of Heaven); or—since it is time for an optimist to be heard—why not even *Apollo* or *Zeus* himself—all with the same sub-title?

Dædalus was neither optimistic nor pessimistic, but merely presented speculations regarding future applications of biology, such as artificial gestation. They were rather improbable and not very lofty speculations; and his final picture of the "scientific worker of the future" singing his "song of deicides," while—

> "All through his silent veins flow free
> Hunger and thirst and venery,"

struck us as being a very poor type, common enough even to-day, seldom achieving anything, and frequently ending with "general paralysis"—certainly not a type high enough to work for as an ideal. On the other hand, Icarus was definitely, but wittily pessimistic, especially in his concluding chapters and in his dread of war—though war is not nearly so great an evil as others, such as disease or destitution.

The new arrival on Parnassus disclaims pessimism because his book, he says, "makes it very plain that the evils which threaten mankind are in no case unavoidable." But this is cold comfort, because we expect the future not only to banish evils but to augment benefits—exactly in the spirit of his motto "Man never is, but always to be, blest." In his overture he visits the tomb of Tantalus, "which had just been discovered in Phrygia by the archæologists of the British School at Athens," and has an interesting interview with that king—who was engaged in trying to pluck the fruits of a tree that was fenced off from him by an impenetrable hedge of old bones of inferior animals, evidently relics of the past.

The moral is that Tantalus (humanity) cannot reach perfection because of his own evolutionary past. But here the optimist will at once reply that Tantalus has at least progressed very far and may, therefore, expect with some reason to progress still farther. To this the author replies that "Man has ceased to be a progessive species long ago," and that "the Cro-Magnon people of the Aurignacian age . . . seem to have been very definitely the finest race of human beings that has ever existed." Is there any certainty or even likelihood of all this? To obtain statistical certitude of the superiority of any strain in any one particular quality we must measure and compare a large

number of similar samples of that strain and of the other strains. The bones of Zulus, Sikhs, or Scotsmen of to-day would be very much larger than those of Bushmen: what conclusion will the archæologist of a thousand years hence be able to draw from this fact? The small Japanese appear to be as capable as the large Muscovites, if not more so. Probably also the Cro-Magnon relics have been mostly those of chieftains.[1] The author thinks that we may have improved "in such minor matters as resistance to the microbes of the many diseases which flourish among dense populations under slum conditions"; but that "against that probability have to be set such certainties as that our toes and many of our muscles are being atrophied and that we are getting more liable to caries and baldness." *Certainties*, quotha!—I should describe all this as unproven and indeed unlikely conjecture; but Tantalus then proceeds to say that "this remarkable *fact* of the arrest of his biological development is certainly the greatest mystery in the history of Man." The word "fact" has been italicised by me as it is here a striking example of *sit ergo est*. It *may be* that the Cro-Magnon race was superior to ours and it *may be* that our toes, muscles, teeth, and hair are degenerating: therefore, quite *certainly*, man's biological development has been arrested! Two conjectures make one proof. But those who have had much experience of many races will question even the conjectures.

In the place of eugenics as commonly proposed, we might try Frederick the Great's method for the creation of Pomeranian Grenadiers, that is the matching and mating of particularly fine specimens, or wise specimens, or good specimens of humanity. A millionaire might be persuaded to endow a "garden city" *in perpetuo* for this purpose; and we might there try to breed various strains of men as we now breed strains of horses and dogs. One might write a romance on the subject, but I fear that the different breeds would soon take to quarrelling.

Our thanks are due to Dr. Schiller for his wise and witty book. No one will agree, and no one will disagree, with all of it.

[1] Keith says that "the stature of the Cro-Magnon people has been exaggerated" (*The Antiquity of Man*, 1925, p. 97).

REVIEWS

MATHEMATICS

L'Analysis Situs et la Géométrie Algébrique. Par S. LEFSCHETZ. Collection ce monographies sur la théorie des fonctions publiée sous la direction de M. Émile Borel. [Pp. 154.] (Paris: Gauthier-Villars et Cie. 1924. Price 20 fr.]

AMONG the many branches of modern mathematics which owe their origin to Riemann may be reckoned that peculiarly difficult one which is nowadays usually called *Analysis Situs*, but which is sometimes known as *Topology*, a name which Listing preferred. The Riemann surface of an algebraic curve and the relation between its connectivity and the genus of the curve are matters with which every geometer has at least a nodding acquaintance. When we come to the study of algebraic surfaces we are forced to consider four-dimensional manifolds, and naturally enough the complexity of the problem is greatly increased. Fundamental work on the subject of the different orders of connectivity in space of n dimensions was done by Betti (1870) and was carried further by Poincaré, who pointed out the considerable differences which arise when manifolds of greater dimensions than two are considered. The treatise of Picard and Simart, *Théorie des fonctions algébriques de deux variables indépendantes*, gives some account of the theory in its application to the geometry of surfaces, but the treatment is on the whole transcendental (*i.e.* deals with integrals on the surface) rather than topological.

In this new volume of the Borel series of tracts M. Lefschetz lays the foundations of a purely topological study of the geometry of algebraic surfaces and varieties. It is a fact that while the Riemann surface of an algebraic curve is topologically the most general bilateral surface of two dimensions, a similar statement cannot be made for surfaces; the index of connectivity of dimension one for a four-dimensional variety can have any value whatever, but for the variety connected with an algebraic surface is necessarily even. The difference arises from the fact that it contains varieties which are topologically very special, its intersections with other surfaces. Profiting by this fact M. Lefschetz investigates the special properties which result and applies them to a discussion of the curves on the surface, and then to existence theorems for Abelian functions. He is thus led to new results and to new proofs of theorems discovered by Castelnuovo, Enriques, and Severi—notably in connection with the *irregularity* of the surface, a number which plays a part corresponding to some extent to that of the genus of a curve. He also gives a new treatment, remarkable for its simplicity and elegance, of the theorem due to Weierstrass, but first proved by Picard and Poincaré, on the conditions for the existence of $2p$-ply periodic functions belonging to a given matrix Ω, *i.e.* of meromorphic functions of p variables u, invariant when the elements of any row of Ω are added simultaneously to the u.

The subject is difficult, but the book is well written and lucid, and will repay close attention, although the reader should certainly first have mastered thoroughly the more familiar treatment of Picard and Simart.

F. P. W.

Leçons de Mathématiques Générales. Par L. Zoretti. Deuxième édition, revue et augmentée. [Pp. xv + 788, with 235 figures.] (Paris: Gauthier-Villars et Cie, 1925. Price 60 frs.)

This book is intended primarily to give a groundwork of mathematics to those who will need to apply it later on, a miscellaneous class comprising, in the words of the preface, "futurs mathématiciens, futurs physiciens, futurs ingénieurs électriciens, mécaniciens ou chimistes, conducteurs des Ponts et Chaussées et Controleurs des Mines, jeunes filles se destinant à l'Enseignement public." But it also aims at supplying a compendium of mathematics for the use of those who in the course of life come up against some difficulty of a mathematical nature which they are unable to overcome, either because they have forgotten what they were taught in their youth or because they never got so far. It is, therefore, as concrete as possible, it assumes frankly what it would be difficult to prove satisfactorily or what requires too much algebra, and rejects what is of purely mathematical interest. We are familiar enough with the kind of book in England. Analytical geometry and the differential and integral calculus occupy most of the space, and there are chapters on Fourier series, on differential equations, and on graphical statics. A good deal of attention is paid to methods of calculation, graphical and otherwise. A number of examples, in the snappy French style, are appended to each chapter; they are announced to include applications "empruntées à la vie courante," and so we may collect information on cycle-racing (p. 64), on the favourite sewing-machine (p. 156, "machine New Home") of France, and on the badness of French railway tracks (p. 328, calculation of the velocity of a train from the bumps every 12 metres where the rails join). An index is added "suivant l'excellent habitude anglaise qui n'est pas assez suivie chez nous."

There is an introduction by M. Paul Appell which contains some interesting remarks upon education in France and a puff of M. Zoretti; but M. Appell should not have stated that the author has sacrificed the whole of the theory of conics and quadrics when the book devotes forty-six pages to them, and a tantalising reference to a chapter on elliptic functions should have been cut out, with it, in the second edition.

The book is excellent of its kind, but it is not likely to have a wide circulation, except in its own country. F. P. W.

Mélanges de Mathématiques et de Physique. Par Émile Picard. [Pp. 364.] (Paris: Gauthier-Villars et Cie, 1924. Price 25 frs.)

As perpetual secretary of the Académie des Sciences M. Picard has led a busy life. He has had to deliver orations at the jubilees of the Société française de Physique and of the Société mathématique de France, he has had to go down to Annonay in the Cevennes to unveil a statue of Marc Seguin, the engineer, to preside at the Conférence de Poids et Mesures, to take part in the centenary celebrations of Pascal, of Pasteur, and of the famous clock-maker Abraham Breguet, who with Cauchy was in 1816 admitted to the Académie des Sciences by royal command, which ordered that the names of Carnot and Monge should be erased from the list of members. The discourses delivered on these and on other occasions have now been reprinted, together with various obituary notices, of Halphen, Sylvester, Weierstrass, Hermite, Zeuthen, and articles of a philosophical kind. The result is a fascinating volume.

We are given, too, an extremely valuable analysis of the contribution of the Finnish astronomer Sundmann towards the problem of three bodies, an account of the work of Fizeau in optical theory, and an introduction to the history of science contributed to M. Hanotaux's *Histoire de la Nation française*. Two short notes, on a partial differential equation occurring in the theory of electric waves and on the rotation of a deformable system, are more technical

in character and do not harmonise very well with the rest; they might well have been left out.

The book is one to read and to possess; the style is delightful, enlivened with epigrams and anecdotes. One, concerning Sylvester, we may be pardoned for repeating—it is not given in the life prefixed to the fourth volume of the Collected Works. During one of his holidays in Paris, about 1887, Sylvester, who was essentially an algebraicist and knew little of modern work in analysis, asked Picard whether he could learn the theory of elliptic functions in six weeks. Picard said that he could, and recommended a young mathematician who was to give him instruction several days a week. The lessons began, but at the second reciprocants and matrices turned up and the young teacher was soon being instructed in the latest researches of Sylvester. There the matter ended.

A highly interesting book indeed; but we could wish that M. Picard had omitted his discourses to the Congress of Mathematicians at Strasbourg in 1920—it is a sorry business.

F. P. W.

PHYSICS

Ions, Electrons, and Ionising Radiations. By J. A. Crowther, Sc.D., F.Inst.P. (Fourth Edition.) [Pp. xii + 328, with 110 illustrations.] (London: Edward Arnold, 1924. Price 12*s*. 6*d*. net.)

Professor Crowther's book provides an introduction to a large number of important advances and problems of modern physics, and the appearance of a fourth edition is good evidence of its usefulness. It is no easy task to select and to condense into one volume the main contributions to these branches of physics which have grown so rapidly during the last few years, and the author is to be congratulated on the selection he has made. In this edition many extensive alterations have been made, and many new sections dealing with resonance potentials, the theory of X-ray spectra, the collision of α-particles with atoms, X-ray spectra and the structure of the nucleus have been added. Particular attention has been paid to the experimental basis of the Quantum Theory throughout the book, and its value has been increased by the lists of references which have been appended to the various chapters.

L. F. B.

The Earth: Its Origin, History, and Physical Constitution. By Harold Jeffreys, M.A., D.Sc., Fellow and Lecturer of St. John's College, Cambridge. [Pp. xi + 278.] (Cambridge: at the University Press. Price 16*s*. net.)

Dr. Jeffreys' book is more than a mere record of the present state of our geophysical knowledge; it is a definite and important landmark in the history of the subject. In fact it is not too much to say that by the production of this book, Dr. Jeffreys has transformed geophysical study into a clear-cut subject, for the outstanding feature of his work is that he has shown us how the different branches of the science may be united into one harmonious whole, and it can no longer be held that geophysics consists of a collection of more or less unrelated researches. The picture of the whole subject which is now presented to us is admittedly incomplete, but it is a picture, and those of us who have been accustomed to hunt through the scientific journals of the world for accounts of the various pieces belonging to the geophysical puzzle will not be slow in recording their appreciation of Dr. Jeffreys' reconstruction. His book will undoubtedly prove of great value in accelerating progress, insomuch as it consolidates the ground already gained, and suggests the future lines of attack.

It is the purpose of the reviewer to dwell briefly on some of the matter

treated by Dr. Jeffreys, and he hopes in this way to illustrate the essential continuity of the book. But he trusts that the author will not think unkindly of the sins of omission which he is about to perpetrate, as even a bare summary of all the topics discussed in the book would be hopelessly beyond the space here available.

Dr. Jeffreys adopts the tidal theory of the origin of the solar system. According to this theory, the near approach of a star caused the ejection of a filament of gaseous matter from our sun, and this filament split up into "condensations" which were the ancestors of the planets. The theory of the subsequent history of these condensations is given in some detail in chapter ii; the course of evolution of the larger and smaller condensations differ somewhat, but in each case the result is the formation of a *liquid* primitive planet. Chapter iii deals with the formation of the moon from the still liquid earth, but we must pass on to chapter vi. At the beginning of this chapter Dr. Jeffreys explains to us how the liquid earth would cool until we have a primitive solid earth with the temperature steadily increasing as we pass inwards. The theory of the subsequent cooling depends to a large extent on the assumptions made as to the distribution of radio-active matter, but Dr. Jeffreys shows us that the results obtained by adopting two extreme hypotheses are not very different. The actual state of affairs may be considered to lie between these two extremes, and we are thus led to the view that the cooling since solidification is chiefly confined to the crust, and that at a depth of 700 kilometres the temperature is still the initial temperature of solidification, namely, about 1,400° C. At such depths the physical state of the matter must be to some extent that of a liquid; it should be capable of being deformed to any extent by continued shearing stress, it should be devoid of rigidity, and consequently it should be unable to transmit distortional waves.

At the more moderate depths of 200 to 400 kilometres the matter composing the rocks would still be very weak. If, for instance, a mountain range was formed on the upper surface, the matter at these depths would, under certain conditions, flow sideways, and the upper crust would be depressed. The author shows that the tendency would be for a state of affairs to be attained in which the mass per unit cross section of a vertical column is unaffected by surface load, and we thus arrive at a physical basis for isostatic compensation, which is now a firmly established observational fact. The theory of Isostasy is discussed fully in chapter ix, and Dr. Jeffreys shows that we must not expect complete compensation of surface inequalities less than 2,000 kilometres in horizontal extent. At the beginning of this chapter there is a useful and stimulating discussion of the physical states of matter.

Chapter x is devoted to the Thermal Contraction Theory of Mountain Building, for which the ground has been cleared by the discussion in chapter vi on the Thermal History of the Earth. We have seen that at a depth of 700 kilometres there can have been no appreciable cooling. At depths immediately above this, each layer, on cooling, becomes too small to fit the interior, but we are still in the region of nearly zero strength, and the contracting layers adapt themselves by spreading out. Near the surface, there has been no appreciable cooling during geological time, the present surface temperature having been attained within a comparatively short time after solidification, and accordingly the surface layers become too large to fit the interior. They are consequently subject to horizontal stress. There is obviously an intermediate layer where the contraction is just enough to allow it to continue to fit the interior, and this is called "the level of no strain." The horizontal stresses near the surface crumple the layers there, thus giving rise to mountains, and a quantitative discussion shows that the amount of compression available is sufficient to account for the greater part, and possibly the whole, of the existing mountain ranges.

In chapter xi Dr. Jeffreys discusses the theories of other surface features such as oceanic deeps and continents. He incidentally also discusses the formation of certain surface features on the moon, but space insists that the reviewer must pass on, and content himself with the remark that these discussions are based on the picture of the cooling solid earth presented to us in chapter vi. He hopes that by now the reader has begun to appreciate the fundamental importance of this chapter.

Let us return for a moment to chapter vi. From what has been said above, it is obvious that the time which has elapsed between the solidification of the earth and the present day must have entered into the analysis, and in point of fact this time is assumed by Dr. Jeffreys to be $1 \cdot 6 \times 10^9$ years. To see how this figure was reached, we must go back to chapter v, which deals with the age of the earth. In this chapter we are shown how the age of the earliest known minerals, as derived from the uranium-lead ratio, comes out to be about $1 \cdot 3 \times 10^9$ years. Geological evidence indicates that some sedimentary rocks are still older, so that the age of the ocean must exceed this figure. Another line of argument, also based on the uranium-lead ratio, indicates that the time which has elapsed since solidification is less than 8×10^9 years. From the previous argument it must be greater than $1 \cdot 3 \times 10^9$ years. The actual figure assumed, viz. $1 \cdot 6 \times 10^9$ years, is somewhat of the nature of a guess, the author considering that the interval between solidification and the formation of the ocean is a comparatively short one.

The orders of magnitude of these figures receive some additional confirmation in a striking way. We have already referred to the condensations in the original gaseous filament expelled from the sun, and their development into primitive liquid planets. But not all the ejected matter would be included in the planets and their satellites. Some of it would disperse and constitute a resisting medium. Dr. Jeffreys points out that this medium would itself be in a state of rotation round the sun, a consideration which appears to have been overlooked by previous investigators of the effect of a gaseous resisting medium in the solar system. He investigates in detail the subsequent history and gradual disappearance of this medium and its effect on the planets. He shows that it must have reduced the initial orbital eccentricities and from the discussion there emerges an estimate of the time during which the action of the medium was effective on the eccentricity of Mercury. This time comes out to be of the order of 6×10^8 to 7×10^9 years. Dr. Jeffreys is also led to estimate that the total age of the solar system since the ejection of the filament is not more than twice this amount, and he concludes that the tidal theory of the origin of the system suggests that its age is of the order of 10^9 to 10^{10} years.

It might be objected that these earlier chapters dealing with the origin of the solar system belong properly to a treatise on cosmogony, and should not appear in a work devoted mainly to geophysics. The reviewer will have failed in one of his purposes if the reader does not begin to feel by now that geophysics cannot be divorced from cosmogony. The earth as it is is a necessary consequence of the earth as it was. From what has been said above it is surely clear that the picture presented to us by a study of the earth's thermal history is absolutely necessary if we are going to try to understand the properties possessed by the earth to-day. And its early thermal history is a consequence of its formation. Things would have been very different if the earth had not arrived at the state of things outlined at the beginning of chapter vi.

Space compels us to pass over with a bare mention the chapters on the Figures of the Earth and Moon, on Tidal Friction, on the Variation of Latitude, the first chapter of all dealing with the now exploded nebular hypothesis of Laplace, and the five useful appendices. We must even pass over page 258. Chapter xii is a useful chapter devoted to seismology. The theory of

earthquake waves is set forth here clearly and concisely, and the practical seismologist will find much of interest. We shall just mention one point connected with this chapter which once more illustrates the essential continuity of Dr. Jeffreys' book. The previous discussion on the Thermal History of the Earth indicated that the strength of the earth's crust is finite at the surface, increases to a maximum at a depth probably of the order of 100 kilometres, and becomes insignificant at a depth of about 400 kilometres. The greatest earthquakes should occur where the greatest stress is relieved by fracture, some 100 kilometres down. The foci of the greatest earthquakes should thus be at depths of the order of 100 kilometres, although they would have a considerable range about this depth.

The book is in many places necessarily mathematical, but not academically so, the author's aim being an exposition of the physics of the earth. He is undoubtedly speculative at times, and no doubt some of his speculations will prove to be wrong, although to be sure he is accustomed to subject his own speculations to the same searching quantitative examination as other speculative minds have to endure at his hands. The fact remains that this book shows—to quote his own words from the Introduction—how "geophysics is no longer a field for uncontrolled speculation; it is a single science whose data are harmoniously co-ordinated by a definite physical theory."

The book is appropriately dedicated "to the Memory of the late Sir George Howard Darwin, the founder of Modern Cosmogony and Geophysics." One cannot help reflecting on how Darwin would have rejoiced over its perusal.

W. M. H. G.

Crystals and the Fine-Structure of Matter. By DR. FRIEDRICH RINNE, Professor of Mineralogy in the University of Leipzig. Translated by WALTER S. STILES, B.Sc. [Pp. viii + 195, with 12 portraits and 203 figures.] (London: Methuen & Co., 1924. Price 10*s.* 6*d.* net.)

EVEN for those of us who are blessed with abnormally high powers of assimilation with respect to scientific ideas, every day sees it become increasingly difficult to grasp alone more than an insignificant fraction of the intricate but harmonious detail that Nature is gradually yielding to us. We often, rather thoughtlessly, talk of "sister sciences," but, in truth, it is being forced upon us more and more that there is only one Science. No longer can we with safety confine our thoughts closely to our own small sphere of investigation. Whatever the point of view we individually might be acquainted with, there are a hundred and one other ways of looking at our problem that urgently demand recognition if truth is to be attained. Particularly is this to be remembered in the study of crystals, and particularly must one recall the lessons of crystallography in the study of physics, chemistry, and mineralogy. Nearly everything solid is crystalline, and this fact is the expression of all-embracing laws which are the property, not of any one science, but of science in general.

It is clear that Professor Rinne is an ardent believer in the truth of this statement. His book has been written with the definite idea of helping students of other branches of science to realise the all-important part *crystals* play in the scheme of things, how their properties as revealed by fine-structural investigations are telling us the nature of many abstruse chemical and physical processes, and how the last thirteen years have brought such a rich harvest of results that it has become imperative to all to strike up an acquaintance, if only a "nodding" one, with the beautiful facts of crystal structure. A book written with such an object in view, especially from the pen of such a well-known mineralogist as Professor Rinne, is to be commended strongly. His method is to introduce to us "crystals as typical of the general

conception of the constitution of matter. In their macroscopic form and their physico-chemical relations are reflected, not only the fine-structure and the physics and chemistry of their own particular microcosm, but also of matter in general." The result is on the whole very pleasing, but there is a certain disadvantage attached to this form of presentation—a disadvantage which the author himself has perceived. It is that in the present early stages of the subject very many aspects of it, as yet not clearly defined by experimental results, cannot be more than hinted at, with the result that the reader in parts of the book gets a meandering sort of feeling, without feeling quite sure what crystals definitely tell us and what they do not. There is, too, some tendency to wordiness in this book. For instance, such phrases as "The ternary rhythm must appear in a whirl form and dominated by the octant" are not very helpful to students of other branches of science who are trying to acquire a clear notion of the fundamental truths of crystallography. The same students also, it is to be feared, will find it somewhat difficult to decipher certain of the diagrams. These would be improved considerably by a little more explanation and a larger scale. The structure of graphite frequently referred to has recently been shown to be incorrect, but probably this circumstance has little effect on the author's argument. There is a mistranslation of the sentence (referring to Haüy's pioneer work on crystal structure), "Er dachte sich die kristalle aus integrierenden Molekülen . . . zusammengesetzt," which is rendered as "He thought of crystals as 'additive molecules' constructed of . . . ," instead of "He thought of crystals as constructed out of additive molecules ('molécules intégrantes') . . . "

The first part of the book is enlivened by a reproduction of Dürer's engraving, "Melancholy," and a series of admirable portraits of crystallographic celebrities. These are very pleasing, for it always nice to know what the "great ones" look like. We have heard the opinion expressed that this book is well worth buying, if on this account alone.

W. T. A.

Distillation du Bois. Par G. Dupont, Professeur à la Faculté des Sciences de Bordeaux. Encyclopédie Léauté, 2e serie. [Pp. xv + 284, with 52 figures.] (Paris: Gauthier-Villars & Cie, 1924. Price 25 frs.)

This volume is a very welcome addition to the literature on Wood Distillation, which in France, as well as in England, is not well represented by standard works devoted to the subject. Until the present publication the only French work of note on this Industry was Dumesny and Noyer's *Wood Products: Distillates and Extracts*, a work which is to-day somewhat out-of-date. In addition, there recently have appeared in French journals several important contributions, amongst which we may mention C. Mariller's excellent pamphlet in *La Technique Moderne*, "La Carbonisation des Bois en France pendant la Guerre," as being noteworthy.

The present volume is thoroughly up-to-date, and embodies descriptions of the different types of newer plant which have been devised for this industry, particularly by Paris engineering firms—notably Messrs. Barbet, Messrs. Granger-Mariller, and Messrs. Prache et Bouillon (evaporator). In this respect it differs markedly from Klar's work, in which the reader cannot escape from the superiority of the plant of Messrs. Meyer of Hanover.

After dealing with the chemical and physical properties of wood—both hardwood and coniferous woods are surveyed—the author touches on extractive processes, passing on to his main theme, the destructive distillation and the products obtained.

This part of the work is well done, and a very complete account is given of the various processes that are in use throughout the world both in small and large scale operation to produce the usual range of products both from hardwoods and coniferous woods.

In the second part the author shows how these products are advanced into higher stages of manufacture, *e.g.* charcoal briquettes, distillation of the tars, guaiacol, methyl alcohol, the acetic acid industry, the acetates and acetic esters.

Short descriptions are also included of the Acetone and Formaldehyde Processes, whilst the volume closes with analytical details.

In fact, the arrangement and ground covered are very similar to Klar's work already mentioned, but the introductory survey of the chemical and physical properties of the woods is fuller and includes all the recent research which has been carried out in this connection.

On one point we are not in agreement. In dealing with the scrubbing of the retort gases to free them from the last traces of naphtha by the Brégeat cresol system, the author quotes Mariller as claiming for cresol a higher solution efficiency than for water. Excellent as is this system, we cannot agree that water is inferior to cresol as a solvent. In our view the advantage of Brégeat's cresol process lies in its power to absorb the oil fog occurring in the gas.

C. S. G.

CHEMISTRY

The Corrosion of Metals. By U. R. Evans, M.A. [Pp. xii + 212, illustrated. (London: E. Arnold, 1924, price 14*s.* net.)

As the author points out in his preface, there are a very great number of books dealing with metallurgy, or "the Making of Metals," although these appeal only to the limited number of people engaged in the metal industries, whereas the study of corrosion or "the Unmaking of Metals," which is of very direct interest to us all, from bridge-engineer to radio "fan," has only a very limited literature of its own.

Mr. Evans has therefore compiled the present work in order to help three classes of readers, namely, the practical engineer or works chemist, those engaged in research work upon metallic corrosion, and, lastly, students of engineering, metallurgy, and inorganic chemistry who wish to gain a general idea of the subject; there is no doubt that all three types of readers will be grateful to Mr. Evans for his timely book.

The following list of chapter headings will best indicate the scope: Historical Survey; Direct Chemical Combination of Metals with Non-metals; Passage between the Metallic and Ionic Conditions; Anodic Corrosion by means of an externally Applied Current; Corrosion involving the Production of Hydrogen Gas; Corrosion involving the Presence of Dissolved Oxygen; Effect of Oxidising Agents on Metals; Corrosion of Copper and Copper Alloys; Corrosion and Tarnishing in a Moist and Polluted Atmosphere; Factors affecting the Velocity of Corrosion; Prevention of Corrosion; Corrosion-resisting Materials; Appendix: Summary of the Behaviour of Individual Metals to Different Corrosive Agencies.

From this it will be seen that the author has covered the ground very completely, and his book should be welcomed by a wide circle of readers.

F. A. M.

The Principles of Applied Electro-Chemistry. By A. V. Allmand, M.C., D.Sc., F.I.C., and H. V. T. Ellingham, A.R.C.S., B.Sc. [Pp. xii + 727, illustrated.] (Second Edition, London: E. Arnold, 1924. Price 35*s.* net.)

The favourable reception accorded to the first (1912) edition of Prof. Allmand's well-known book would, no doubt, have necessitated a new edition within a short time; but, as in other cases, the war has hitherto prevented

this being done. The author, in collaboration with Mr. Ellingham, has now produced a fully revised (and in part rewritten), edition which will be as welcome as was its predecessor.

Certain elementary portions have been removed and their place taken by a more detailed treatment of irreversible electrode phenomena. In the second part, dealing with special technical operations, a considerable amount of revision has taken place, as, for instance, the discussion of new electrolytic methods for copper or zinc extraction, alkali chlorine cells, and the section on electro-metallurgy. The section on nitrogen fixation also, though short, contains the gist of the matter. It is noteworthy that only four pages are devoted to the production of organic chemicals by electrical means, chiefly, no doubt, owing to the small amount of attention paid to the use of electrical methods in the organic chemical industry; perhaps in the next edition it may be found necessary to expand this section.

Those engaged in dealing with electro-chemistry either on the theoretical or practical sides will be grateful to the authors for the painstaking way in which they have revised the book.

F. A. M.

Outlines of Organic Chemistry. By E. J. Holmyard, M.A., F.I.C. [Pp. xi + 466, with six plates.] (London: Edward Arnold & Co., 1924. Price 7*s*. 6*d*. net.)

New elementary textbooks are, as a rule, dull fare for the reviewer, but Mr. Holmyard's book is a piquant apéritif, which makes one long for the more solid cheer he could undoubtedly supply in a more ambitious work. The arrangement may challenge comment, as the first eight chapters deal with general theoretical considerations; but it has advantages, since only by this means can "the elegance of the theoretical structure" (to quote the preface) of organic chemistry be brought home to the student as a coherent whole. The subject-matter is accurate, judiciously selected and up-to-date, whilst the author is pleasantly frank in dealing with problems where other writers prefer to conceal our ignorance by restating the facts in different words, thinking that they thereby furnish an explanation. On the other hand, the book is very modern, the author does not shirk the introduction of the simpler ideas of Lewis and Langmuir, and it is obvious that he has the justifiable confidence that senior students in schools can appreciate much more than is often supposed to be the case. How often is this supposed lack of ability made the excuse for the science teacher's neglect of his own education. Those who are content to be dogmatic and dislike their pupils to ask awkward questions, will do well to shun this book, as in the hands of an intelligent boy it will promote a healthy scepticism and a flow of posers which will necessitate his teacher being very much alert. One cannot resist one quotation to indicate the mentality of the work. Speaking of the constitution of benzene, the author states: "The problem of the fourth valency of each carbon atom was shelved by Armstrong, who suggested one 'centric' formula in which these valencies were merely directed towards the centre of the benzene ring. One advantage of this formula was its unconventional nature, which provoked much healthy criticism and fruitful research. Essentially it was merely a confession that chemists were entirely ignorant of the actual conditions within the benzene molecule." The frequent historical references, unusual in a book of this type, and the excellent plates add to the human interest, and the reviewer is of opinion that Mr. Holmyard's book is the best elementary textbook of organic chemistry which has appeared for many years in any language.

O. L. B.

A Comprehensive Treatise on Inorganic and Theoretical Chemistry. By J. W. Mellor, D.Sc. Vol. V., B., Al., Ga., In., Tl., Se., Ce, and Rare Earth Metals, C (Part I). [Pp. x + 1004, with 206 diagrams and illustrations.] (London: 1924, Longmans, Green & Co. Price 63s. net.)

At one time the belief was widely held that organic chemists had a monopoly of long names to describe their compounds, but, after reading a few pages of Dr. Mellor's fifth volume, one is inclined to revise that belief; such names as Hydrated Tetrasodium Tetracupric Octodecaborate, Decahydrated Hydrazine Hexahydrododecaborate and Ammonium Isotetrahydroborododecatungstate are printed unblushingly—one dare not think how they would look in German—so that obviously organic chemists must look to their laurels or take to cross-word puzzles.

The "Comprehensive Treatise" has by now attained to the stage reached only by great prima-donnas, authors, artists, and cricketers, of being quoted *tout court* by the surnames alone, so that "Mellor" is now a household word in chemical circles. The present volume must have given the author at least as much trouble, and probably a great deal more, than the previous ones, as the section upon the rare earths alone is a subject of very considerable complexity, and one calling for great care in its arrangement.

The table of "Reported Elements of the Rare Earths," with the frequent comments of "Mixture," "Unverified," and "Unconfirmed," indicates how easily even a seasoned investigator may be led astray, and it is no easy matter to judge between fact and fiction where so much is vague and unconfirmed. Dr. Mellor seems to have accomplished the task with his usual skill, and chemists may well be grateful to him for his clear arrangement of the facts.

For the rest, the book follows the lines of the previous volumes, and shows that the author is slowly and methodically codifying the whole of inorganic chemistry with a patience and pertinacity worthy of emulation. We may be proud that such an inorganic rival to "Beilstein" is being produced in the English language.

F. A. Mason.

Cotton Cellulose, its Chemistry and Technology. By A. J. Hall, B.Sc., F.I.C. [Pp. 228, illustrated.] (London: E. Benn, Ltd., 1924. Price 30s. net.)

It might perhaps be doubted whether there was room for another book upon cellulose, but as most of those already published are somewhat lengthy compilations written by experts for experts, the present volume, written by an expert for those less experienced, will not come amiss.

The importance of cellulose needs no emphasising, whether we consider it from the point of view of one of our staple industries worth some £200,000,000 a year, as an essential clothing and writing material, as an important constituent in all vegetable foods, or of its urgency as a basic material for modern warfare. The author wisely confines himself to essentials, and deals chiefly with the chemical and physical properties of cellulose, together with the preparation and properties of its more important derivatives, such as the artificial silks, nitro-cellulose, and celluloid.

One feels that rather more space might have been devoted to the problem of the chemical structure of the cellulose molecule than eleven pages, but doubtless Mr. Hall decided that where so much is still obscure it would be unwise to spend too much time upon its consideration.

The book is well printed, illustrated, and bound, but the reviewer feels entitled to a protest at the growing habit, of which the present volume is an example, of issuing scientific works printed with wide margins upon large sheets of paper so thick as almost to be indistinguishable from thin paste-

board; the work under review, although of only 228 pages, is nearly one and a half inches thick, the pages themselves being seven inches by nine inches, with margins of one to one and a half inches, the total weight being two pounds.

It is difficult to see what purpose is served by this device—unless, indeed, in the present case it is intended to show the practical uses of cellulose—as, although it may make the book imposing to look at on one's bookshelves, scientific students prefer a more handy size with weight and thickness reduced to a minimum. Despite this drawback, however, Mr. Hall's excellent book should serve a useful purpose in introducing the chemistry of an important substance to a wide circle of readers. F. A. MASON.

The Plant Alkaloids. By T. A. HENRY, D.Sc. Second Edition. [Pp. vi + 456, with 8 plates.] (London: J. & A. Churchill, 1924. Price 28*s.* net.)

DURING the last few years much research has been carried out on the alkaloids, consequently this new edition of Dr. Henry's book has involved a great deal of alteration. One must congratulate the author on the thoroughness with which he has carried out his task, and by judicious selection of material has kept the book within reasonable bounds without sacrificing any really important matter; the temptation clumsily to graft the new work on to the old has been happily resisted. For a volume of this character it is unusually up-to-date, work published in the early part of last year being included. The clear exposition of the first edition is maintained, and it is a matter of satisfaction that what must be regarded as the standard work on the alkaloids has been published in this country. The reviewer has one quarrel with Dr. Henry: the inclusion of the purine group. These compounds cannot be treated satisfactorily apart from the simpler ureides, such as barbituric acid, and it seems desirable for this reason to exclude caffeine, theobromine, etc., from the alkaloids. O. L. B.

The Elements of Colloid Chemistry. By H. FREUNDLICH, Professor, University of Berlin. [Pp. iv + 210, with 29 figures.] (London: Methuen & Co., Ltd. Price 7*s.* 6*d.* net.)

THIS book is an abbreviation of Freundlich's *Colloid and Capillary Chemistry*, of which the English translation is about to appear. In order to make it available to a larger number of students of medicine and technology, the bones, *i.e.* the mathematics, of the larger volume have been removed, and a very readable textbook is the result. Colloidal chemistry without its mathematics is usually presented as a formless mass of facts which makes very dull reading. This difficulty the author has overcome without omitting any essential details.

A wider audience could perhaps have been reached had the terminology of colloid chemistry been explained in greater detail with the help of diagrams; but as there are so many new conceptions to be presented, it is doubtful if this could have been accomplished within the short space of 200 pages.

Much of the modern work is included, although certain important aspects, *e.g.* the Donnan membrane equilibrium and the Langmuir theory of orientation of polar molecules on surfaces and their applications, are dealt with much too briefly. On this account, the book does not survey the whole field of colloid chemistry in as uniform a manner as could have been wished. A few minor points of criticism may also be made: for example, Willard Gibbs is referred to as "the American theorist"; and, on p. 43, "Charcoals . . . bind gases by loose combination" is given without any qualification.

It is, however, the nearest approach to the ideal elementary textbook on colloid chemistry that has yet been attained, and it is strongly recommended to all students of physical chemistry. W. E. G.

Electrode Reaction and Equilibria. A general discussion held by the Faraday Society, November 1923. [Pp. 152, with numerous diagrams.] (The Faraday Society, 10 Essex Street, Strand, W.C.2. Price 10*s*. 6*d*. net.)

Two important functions are served by the general discussions of the Faraday Society. In the first place, they provide a clearing-house for ideas on the knotty problems of physical chemistry, and in the second place they lead to publications which summarise the present position of the subjects within their purview. The present volume on electrode reaction and equilibria contains many interesting contributions to the study of the reversible and irreversible processes of electrochemistry. It includes a number of theoretrical papers on the theory of the mechanism of the reversible electrode in aqueous and non-aqueous solutions, and of polarisation phenomena, which, although they bring out little that is new, cover the whole field from several different points of view. Of special value is the comprehensive summary and bibliography by Allmand and Ellingham on irreversible electrode phenomena, and of wide interest that of Smits on the application of his theory of allotrophy to electromotive force equilibria.

The experimental papers will prove of considerable value, especially those on the Biilmann quinhydrone electrode. Professor Biilmann in his paper gives a full description of the experimental technique of the new electrode, and Pring an example of its application to the measurement of the affinity constants of bases. Other experimental papers by Goard and Rideal on a controlled oxygen electrode and by Glasstone on overvoltage contain many interesting details of technique. A paper by Lattey gives an account of measurements with a string galvanometer of the polarisation voltage of hydrogen and oxygen electrodes, which bring out very clearly the complex character of the factors involved in overvoltage.

It is a valuable addition to the series of publications of a similar character which have emanated from the Faraday Society.

W. E. G.

The Theory and Application of Colloidal Behaviour. Vols. I and II. By R. H. Bogue, Ph.D (editor). [Pp. xlvii + 829, with numerous illustrations and figures.] (New York: McGraw-Hill Book Co. Price 40*s*. net.)

These volumes are in the direct line of descent from Bancroft's *Applied Colloid Chemistry* and Loeb's *Theory of Colloidal Behaviour*, by the same publishers. The original plan of Bancroft was that his theoretical treatise would be followed by a series of monographs on industrial colloid chemistry, but this plan has not been adhered to in the new volumes. Instead, one volume on the theoretical and one on the industrial aspects of the subject are presented, each containing seventeen essays contributed by a number of the foremost colloid chemists of the day. These essays have been skilfully combined into a co-operative treatise which adequately surveys the whole field of colloid chemistry. The result is as homogeneous as was possible, bearing in mind the difficulties inherent in such a scheme. If freshness and originality in presentation were to be retained, a certain amount of overlapping in material and difference of opinion on fundamental questions could hardly be avoided.

Colloid chemistry is at a transition stage where two radically distinct ideas are slowly being merged into a coherent theory. On the older view, it is maintained that absorption and the action of surface forces are the main factors in the world of neglected dimensions! While according to a later development, very largely due to Loeb, the first place must be awarded to the Donnan equilibrium. On the whole, the continental schools, represented by Michaelis and Freundlich, have adopted the former and the American and English the latter point of view.

The Proctor-Wilson theory of colloidal behaviour based on the Donnan equilibrium finds ardent supporters both in the essays on the theoretical and industrial branches of the subject. It is the day of the ubiquitous hydrogen ion, whose goings and comings are faithfully recorded by the colloidal system; indeed, its importance is perhaps exaggerated by Van Slyke in the section on the colloidal behaviour of body fluids when he explains all the physical phenomena in terms of changing pH and entirely ignores hydration and absorption phenomena.

Attention is directed to the part played by the oriented monomolecular film in colloidal behaviour by Harkins and to the properties of the monomolecular layer in contact catalysis by Taylor, and it seems clear that the conceptions advanced by these writers will exert considerable influence on the future development of colloid chemistry.

The subjects of the essays on the application of colloidal behaviour to industry cover the whole range through metallic, vegetable, and animal products, and they all provide interesting reading.

W. E. G.

GEOLOGY

A Textbook of Geology. Part II. Historical Geology. By C. Schuchert. Second, revised Edition. [Pp. viii + 724, with 236 figures.] (New York: John Wiley & Sons; London: Chapman & Hall, 1924. Price 22*s*. 6*d*. net.)

This book is Part II of a second, revised edition of Pirsson and Schuchert's well-known *Textbook of Geology*, which, originally published some nine years ago as a single volume, is now presented in two volumes. The book is increased by about eighty pages; the majority of the old illustrations are retained, but there are a number of new ones, notably some excellent palæogeographical maps drawn by A. K. Lobeck. These portray the distribution of land and sea in North America during the geological periods in a very clear manner, and even attempt a bird's-eye view of mountain ranges.

Schuchert treats historical geology as a biological as well as a geological science. He brings together facts from structural and stratigraphical geology, palæontology, biology, oceanography, and astronomy, and weaves them into a connected history of the earth, and the evolution of its inhabitants. Hence the original character of Schuchert's textbook, in which chapters on Organisms, Evolution, Continents, Oceans and Seas, Evolution of the Stars, Origin of the Solar System, and on the various groups of fossil animals and plants, are intercalated in their logical places within the matter dealing with the stratigraphical sequence. The result is a textbook which reads like a romance, yet maintains a rigidly scientific method in the presentation of the facts.

Exception has been taken to the inclusion of some of the above topics; but the answer (in the reviewer's opinion) is that the subject of the book is not Stratigraphical Geology in itself, but Historical Geology, which embraces a much wider field; and none of the included matter is irrelevant in relation to this more comprehensive view-point.

While the book deals chiefly with North American stratigraphy, there are numerous references to extra-American correlations, but not sufficient to make the book of value as a text for European students. Nevertheless, the reviewer has always recommended Schuchert's textbook to advanced students because of its original method of presentation, and its inculcation of ideas which are fruitful when applied to European stratigraphical problems.

Schuchert uses the term Eozoic for the era of dawning life preceding the Archæozoic and following the Azoic; but, of course, there is no known rock record of either Eozoic or Azoic (pp. 47, 103). Thus, excluding Cosmic or

Astronomic time, the Pre-Cambrian is divided into the Azoic, Eozoic, Archæozoic, and Proterozoic eras by Schuchert. This strikes one as a reasonable employment of these much-debated terms. In the earlier edition of the work Champlainian was the name of the middle division of the Ordovician. Now Schuchert has extended its significance so as to cover the whole of the Ordovician, and indeed to replace that term. We cannot but think this a regrettable step, especially as it is inconsistent with his action in extinguishing two other system names proposed in the former edition. Ozarkian, for instance, is now employed for the closing stage of the Cambrian, and not as the name of a full system bridging the gap between Cambrian and Ordovician. We welcome the abandonment in this edition of the premature apportioning of the Cambrian and Ordovician into divisions which were given the full rank of stratigraphical systems. Similarly the term Comanchean is abandoned as a system name of world-wide application, and restored to its former use as the name for the lower part of the Cretaceous in Western North America.

The book may be accepted as the most authoritative account of the geological history of the North American continent, and especially of the United States. We wish its method could be applied to the stratigraphical geology of the Old World. Eurasia and the Southern Hemisphere as a whole may be suggested as two suitable units for treatment, although smaller units might perhaps be better. But, except for certain small regions, we could not yet look for the precision and detail of Schuchert's work.

G. W. T.

BOTANY

The Transpiration Stream. By Henry H. Dixon, D.Sc., F.R.S. [Pp. 80.] (London: University of London Press, 1924. Price 2*s*. 6*d*. net.)

This little book of three lectures delivered at London University is rather in the nature of a supplement to the author's *Transpiration and the Ascent of Sap*, published ten years ago. To any unfamiliar with the latter the significance of these lectures will be largely lost. This is emphasised by the Bibliography of about sixty papers which is additional to those cited in the above-mentioned work.

The first lecture is concerned with the continuity of the water columns in the stem, the tensile strength of these which would appear to be over 300 atmospheres, and the visual recognition by Bode of the compression due to the negative pressure. The second lecture is mainly a criticism of the views of Bose, in which the author records his inability to obtain the results of that investigator.

The third lecture treats of translocation, and the view is advanced that the bast, medullary rays, and wood parenchyma are not only responsible for the entry and exit of materials to and from the conducting system but that also they are responsible for the prevention of precipitates and gels, et cet.

The text is written in the easy and interesting style that one is accustomed to associate with Prof. Dixon's writings and for the greater part of the way the author will carry most of his readers with him. But far more evidence will be needed before the hypothesis receives any general acceptance that the sieve tubes, with their highly specialised structure, large lumen, and perforated end-walls, play no part in the actual translocation process.

E. J. S.

Researches on Fungi. Vol. III. By A. H. R. Buller, D.Sc. [Pp. xii + 611, with 227 text figures.] (London: Longmans, Green & Co., 1924. Price 30*s*. net.)

The first part of the present volume, like its two predecessors, is chiefly concerned with the minutiæ of hymenial construction and spore dispersal in the Hymenomycetes.

Many of the types here considered possess relatively ephemeral fruit-bodies, as *Psathyrella dissemminata*, *Lepiota cepæstipes*, *Bolbitus flavidus* and the Coprini Types, with prolonged spore discharge, are represented by *Lepiota procera* (8 days), and *Armillaria mellea* (5 days.)

In *P. disseminata* and *L. cepæstipes* development of the hymenial layer is uniform, and there are four distinct generations of basidia, each shorter than its predecessor, whilst the paraphyses form a continuous layer between the basidia. The former species appears to possess a secondary red-coloured reproductive mycelium (ozonium) comparable to that of *Coprinus domesticus*. The author draws attention to the contrast between *Lepiota cepæstipes*, with its ephemeral fruit-body and a spore discharge period of only 15 hours, and *Lepiota procera*, the Parasol Fungus, in which the fruit-body and the period of discharge last for over a week. The basidia in this latter species are all alike, and the hymenium is loose in character, whilst spore discharge is intermittent. The ephemeral *Bolbitus flavidus* agrees with *L. procera* in the single type of basidium and with other ephemeral species referred to in having four generations of these structures.

Armillaria mellea presents a common type to which several familiar fungi conform (e.g. *Amanita rubescens*, *Amanitopsis vaginata*). Here the hymenium is loosely constructed, the basidia are of a uniform type and do not show intermittent development, whilst the paraphyses are small.

The author finds that the Coprini exhibit two, three, or four generations of basidia, which are either di-, tri-, or tetra-morphic. In *C. comatus* the basidia are dimorphic and not monomorphic as Prof. Buller had previously stated. In *C. atramentarius* the gills are separated by elongated cystidia which are attached at both ends.

The author gives an account of luminescence in the higher fungi which is mainly a summary of the results of previous investigators. The work of Molisch and Guéguen has shown that the stag's horn fungus *Xylaria hypoxylon*, is probably represented by two strains, of which the mycelium of one is luminous and the other non-luminous. Atkinson recorded luminescence of the fruit-body of *Panus stypticus* nearly a quarter of a century ago, and Buller, whilst confirming this in the American material examined by him, has failed to observe luminescence in the fruit-bodies of English specimens and concludes that a luminous and non-luminous strain exist. In view of these results it might have shown better judgment if Prof. Buller had contented himself with recording his failure to observe luminescence in *Fomes annosus* and *Polyporus stypticus* instead of expressing scepticism with regard to the observations of W. G. Smith.

The three chapters of Part II treat of the spore dispersal in the Uredineæ and the nature of the spore-walls, the structure of the latter being explained in terms of their function.

The illustrations, with the exception of some shadow photographs, and a few others, are of a high standard of excellence both from the technical and scientific standpoint. The photograph of *Lycoperdon giganteum*, despite its technical defects, will be welcomed by mycologists for the excellent portrait of Mr. Ramsbottom, who holds the specimen.

E. J. Salisbury.

Botryllus. By E. Catherine Herdman, M.Sc. L.M.B.C. Memoir XXVI. [Pp. xi + 40, with 6 plates.] (Liverpool: at the University Press. Price 4*s*. 6*d*. net.)

Botryllus was wisely chosen as a type of a compound ascidian for detailed description in the well-known L.M.B.C. Memoirs, its abundance on our coasts making it a specially suitable subject for the laboratory student. It is intended that this memoir should be studied with the volume previously published in the same series on Ascidia as a type of simple ascidian by the

late Sir William Herdman, and these together form an excellent introduction to the whole group.

Miss Herdman gives a short and very lucid account of Botryllus, whose individuals have, as she says, "to some extent lost their individuality." It is, however, by no means certain in this case which is the individual, although we agree with her in preferring the ascidiozooid to the corm.

The anatomical descriptions are illustrated by simple and effective drawings, those on Plate II showing very clearly the general arrangement of the internal organs, and there is one coloured plate illustrating one of the numerous colour schemes of this extremely variable species; for in this monograph it is concluded that only one species of the genus has as yet been described.

Vexed points are fully discussed, the author's opinion based on the experiments of many workers being that the refringent organ is a digestive gland and the function of the neural gland is sensory. As regards the feeding, she is in full agreement with Orton as to the endostyle secreting the mucous with which the food is mixed, but bearing little or no part in the actual food collection, this being the function of the cilia on the walls of the gill bars of the pharynx, the current directing the entangled food to the dorsal lamina and so to the œsophagus.

This is altogether a most welcome addition to the series, and Miss Herdman is to be congratulated on a good piece of work, clearly expressed and easily followed, which will be a real help in the laboratory.

MARIE V. LEBOUR.

ZOOLOGY

British Waders. By E. C. ARNOLD. [Pp. viii + 102, with li coloured plates.] (Cambridge: at the University Press. Price £3 10s. net. Limited edition of 50 signed and numbered copies on hand-made paper, of which 45 are for sale, £7 7s. net.)

EVEN in this age of beautiful books, Arnold's *British Waders* will attract the attention of book lovers. It is tastefully designed and perfectly produced.

The text is slight but full of interest. It consists almost entirely of original notes and personal observations, the value of which is greatly enhanced by the author's wide experience and exceptional appreciation of movement and colour. Of the fifty-three species dealt with, Mr. Arnold has collected no less than twenty-nine, and has narrowly missed getting several more.

Many questions of particular interest to field ornithologists are raised in these pages. The thorny subject of sight records is frequently alluded to. In commenting on his sight record of a Bonaparte's Sandpiper (p. 24), our author points out the very true but rather unfortunate fact that such records are received with scepticism by the most ardent protectionists as well as scientists. We may infer that the recorder of birds has no choice but to kill if he wants to be taken seriously.

Mr. Arnold's pictures, although several are open to criticism from one point of view or another, are certainly refreshing in their striking originality of conception and execution. Many of them are very effective, and some exceptionally pleasing.

The book is not a scientific production, but an illustrated collection of field notes. A definite statement as to the age, sex, and season of the specimen depicted would have made the pictures more useful, though in some cases these facts can be gleaned from the text. The total length of the bird, as an indication of size, is given in every case, however. The omission of an index is also a matter of regret.

We can heartily commend the volume to field ornithologists.

W. R.

MEDICINE

Textbook of Pathology. By Robert Muir, M.A., M.D., Sc.D., F.R.S. [Pp. viii + 774, with more than 420 original illustrations.] London: Edward Arnold & Co., 1924. Price 35*s*. net.)

Medical students who read well the book which Professor Muir states is written primarily for them will obtain a good knowledge of the actual subject of pathology. But they will also obtain something of even greater value, an understanding of the way in which the problems connected with the etiology and processes of disease can be solved by the application to them of fundamental physiological principles. It is this aspect of the book which in all probability will make it of great value to the general practitioner, who in the study of each case is confronted with a fresh problem.

In writing a textbook on a subject as extensive as pathology there must always be selection if the book is to be kept a reasonable size. Professor Muir, in making his selection, has been guided by two main principles; to quote his own words, he has endeavoured "in the first place to give due weight to the scientific aspect of the general pathological processes, and in the second to describe those pathological changes in the various organs which are of special importance in relation to Clinical Medicine and Surgery." One of the chief features of the book, indeed, is the way in which the author has sought to make it of service to the practitioner by emphasising the clinical application of pathological results.

Certain subjects of a more specialised nature such as Diseases of the Skin, Tropical Diseases and Parasitology have been omitted, but nevertheless an account has been included of the more important parasites in connection with the lesions which they produce.

The book throughout incorporates the results of recent experimental and chemical research, and is of the greater value in that it is so largely based on the author's own observations covering a large field of work and extending over many years. It contains a large number of original illustrations and references are given to works which the author has found of value to his students for the purpose of more detailed study of special points.

The chapters on inflammation, disturbances of the circulation, blood, and on the hæmopoietic system generally are especially good. In the account of the pathology of the heart one would have liked a fuller account of disturbances in the conducting system, with an indication of the author's views on Lewis's explanation of fibrillation and flutter. The chapters on the central nervous system might well have been longer. These criticisms are really a testimony to the excellence of the book, for those parts of the subject that have been dealt with in any detail are so clear and illuminating that it rouses a desire to have the whole subject dealt with in the same manner. But within the limitations imposed by the size of the book it is hard to conceive of a better or more stimulating textbook of pathology than the one the author has given us.

The Leucocyte in Health and Disease. Being an Enquiry into Certain Phases of Leucocytic Activity. By C. J. Bond, C.M.G., F.R.C.S. [Pp. viii + 84, with 48 illustrations on 24 plates.] (London: H. K. Lewis & Co., Ltd., 1924. Price 12*s*. 6*d*. net.)

This is essentially a study of the *living* leucocyte. In carrying out the investigation use has been made of (1) the "Closed Cell" as a means of obtaining living leucocytes free from other cellular elements and in sufficient numbers for experimental purposes, (2) the dark-ground method of illumination for observing the changes in the leucocytes and their active response under varying conditions.

The author shows that if a drop of blood, placed in a closed cell, be incubated for a few minutes, a clot forms which stretches like a membrane across the cell, serum exudes, and leucocytes emigrate on to the slide and adhere to it. If the clot is gently removed, the living leucocytes adhering to the slide can be mounted in normal saline and examined.

Among many interesting observations made on these living cells are (1) the relation between the form of the leucocytes and their activity, (2) an oscillatory movement of the cell granules in the stimulated leucocytes, which is preliminary to (3) the protrusion of pseudopods and very fine branching dendrites, which may be of considerable length, and which serve to entangle foreign bodies, (4) responsiveness of the leucocytes to changes in surface tension and their consequent adaptation to the finest irregularities of any object with which they come in contact.

Again, he shows that if the removed clot, washed in a gentle stream of saline, be replaced and reincubated in a closed cell with a drop of saline, a second crop of leucocytes will emigrate, but this time free from serum. He has thus been able to demonstrate the influence of serum on the emigrating leucocyte in both health and disease, and he points out important differences in different diseases.

Sections are devoted to the interaction between leucocytes, serum, and red corpuscles, both native and foreign, and it is shown how additive toxic or opsonic substances may be formed on the surface of the red cells by serum, and the close relationship between these erythrotoxins and erythro-opsonins. Indeed evidence is advanced which strongly suggests that the latter may be derived from the former. Other sections deal with the part played by leucocytes in carbohydrate metabolism, and the presence, staining reactions, nature, and function of "diffusion" and "iodophil" substances secreted by the leucocytes.

All the phenomena observed are beautifully illustrated by actual microphotographs, and the conclusions arrived at are well summarised in the last section. As an original contribution to the elucidation of the problems of immunity, and in particular the part played by the leucocytes, the book is of considerable importance. A new line of attack has been opened up, and the valuable results so far achieved should stimulate others to follow and still further develop it.

HERBERT C. LUCEY.

The Mongol in our Midst. By F. G. CROOKSHANK, M.D. [Pp. 128, with 28 plates.] (London: Kegan Paul, Trench, Trübner & Co.; New York: E. P. Dutton & Co., 1924.)

THE author describes his work as gleanings from several fields, but he has so arranged his material that whosoever, interested in human evolution and devolution, commences the perusal of this little volume will find a difficulty in laying it aside unfinished. The primary aim of the thesis is to seek an anthropological explanation of certain observations in clinical psychiatry, which leads to a discussion of the bearing of the evidence from atavism on the question of the monophyletic or polyphyletic origin of mankind.

In 1844 Robert Chambers expressed the opinion that the characters of the leading races of mankind might be regarded as representations of particular phases of the development of the highest, or Caucasian, type, which he placed at the top of the scale, the Negro being at the bottom and the Mongolian races occupying an intermediate position. He also made the observation that the offspring of certain Caucasian parents who were nearly related were persons who "in maturity are still a kind of children" and resembled the Mongols. Twenty years later J. Langdon Down suggested the classification of idiots by their "ethnic types," and the one of his categories which has received universal assent as a distinct class, he termed the

Mongol type. The resemblances to their racial prototypes were broad features, obliquely placed eyes, flattened bridge of the nose, rounded pinna, transverse fissuring of the tongue, short stature, and the relative proportions of the limbs. In certain other features there was no resemblance, and probably the association with idiocy discouraged any consideration of the racial aspects of the ætiology of this variety of human deficiency.

Dr. Crookshank has noted certain other features, such as the habitual posture in sitting on the ground, the marking of the hands, and some characteristic movements in which the idiots resemble the racial Mongols. He also considers that the traits of Mongolian idiocy are only fully developed in those cases in which both the parents possess similar characters, though in a lesser degree and without any associated feebleness of mind. He noted that certain traits characterising the Mongolian races were to be found in a considerable proportion of the inhabitants of western Europe. Whence came these traits? Years ago it was thought that there had been no admixture with racial Mongols in western Europe until the invasion of the Huns, but it is now believed that there may have been occupations by peoples with Mongol traits in the Magdalenian and in the Later Neolithic and Early Bronze ages, in which case the necessary elements for an atavistic explanation of the phenomena of Mongolism are present.

Inquiring into the topographical distribution, Dr. Crookshank has been unable to trace the occurrence of Mongolian idiocy other than among the white and the Mongolian races. He has not heard of it amongst pure-bred negroes, native Australians, those races of India that are free from any Mongol strain, or Arabs from the Yemen. He has not seen it among pure Semitic Jews, and though he quotes Feldman as saying that Mongolism is common among Jews, he thinks this may be due to some misapprehension or to the fact that many Jews are descended from the Mongolian tribe of Chazars in southern Russia who were converted to Judaism. This question of origins is of some importance, for the recent paper by Brushfield in the *British Journal of Children's Diseases* reveals a high proportion of Jews among the Mongolian imbeciles admitted to the Fountain Mental Hospital; while the figures regarding cases brought to notice in London under the Mental Deficiency Act show that about 9 per cent. of the instances of Mongolism were in children of Jewish parentage, a proportion greater than that of the Jews in the general population.

Some have regarded the reversion of the Mongolian imbecile, if reversion it be, as something going farther back than a Mongol racial stock, to something prehuman. Dr. Crookshank takes up this point and suggests that in the features to which he draws special attention, to wit, postures, attitudes, and palmar markings, both Mongolian imbeciles and racial Mongols resemble orang-utans. He describes the orang as differing from the chimpanzee and the gorilla in the same kind of manner as the Mongolian races differ from the Semites or the Negro. Both Mongol idiots and orangs are dysthyroidal in their make-up, and both in towns suffer from respiratory disorders and lymphatism. The author also draws attention to certain similarities in the palmar markings and postures of the two groups, the Negro and gorilla on the one hand and the chimpanzee, the "white," the sufferer from dementia præcox, and certain neurotics on the other. He finds the best explanation of these similarities and differences in the hypothesis of multiple lines of human and anthropoid descent enunciated by Klaatsch.

Such an hypothesis, if proven, would indeed help to clear up the problem of the ætiology of mental deficiency, but certain difficulties stand in the way. This hypothesis seems to require a continuous gradation physically and mentally between the lowest Mongolian idiots and the highest types of European with some eastern traits of countenance; but this does not occur. The subject of Mongolian imbecility is abnormal at all ages, his intelligence

develops beyond that of the child, he is never capable of continuous attention, he appears full of promise but never of achievement. The "Mongol" school child described in this volume is a quite different person, sometimes alert, sometimes slow, whose behaviour is never that of a defective. But though between the subject of Mongolism and the normal there is a great gulf fixed, Mongolism shades off on the one hand into cretinism, and on the other into certain ill-defined forms of primary amentia, although the subjects of these conditions would apparently be held by Dr. Crookshank to belong to different "ethnic types." Probably the common factor between these forms of defect is a certain degree of dysthyroidism, though the true Mongol idiot is unimproved by thyroid medication.

The nervous system of the Mongol idiot shows a diminution in size and complexity not so much in the neopallium as in the more archaic structures of the mid and hind brain, a feature which could scarcely be explained by atavism to an anthropoid stage of evolution.

The comparision between the chimpanzee and the sufferer from dementia præcox seems less happily devised; the anthropoid appears a lively and very thorough extravert living in the real present, the psychotic an introvert living in a world of phantasy. It should also be noted that Mongolism and dementia præcox may be common in the same populations, *e.g.* the Jewish.

Although the characters on which stress is placed are by no means constant nor the arguments always convincing, the volume as a whole is thoroughly stimulating and may well have applied to it the saying, "Si non e vero e ben trovato."

F. C. Shrubsall.

Elementary Morphology and Physiology for Medical Students. By J. H. Woodger, B.Sc. [Pp. viii + 528, with 255 text figures.] (London: Humphrey Milford, Oxford University Press, 1924. Price 12*s.* 6*d.* net.)

This book strikes a fresh note, and is a welcome departure from the usual junior textbook. The general aspects of biology are here considered from a wide and enlightened standpoint. Each animal is presented to us as a living being; the structure and function of the organs are treated side by side, an essential but often neglected factor in any introduction to general biology, whilst the needs of the medical student are constantly kept in view.

A short but excellent introduction is followed by a brief survey of animal organisation in general, after which comes a chapter on the elementary histology of the tissues. We are then led on to a consideration of the cell and cell division. The next four chapters deal with Invertebrates, the usual types being described. In the study of the Protozoa, the attention of the reader is directed to the experimental side of biology, and the account of flatworms concludes with a summary of parasitism as a whole.

The Vertebrata necessarily fills a greater proportion of the volume. This part begins with the anatomy and development of Amphioxus, after which, a detailed description of the dogfish gives us an insight into the structure of a typical Vertebrate. This section is excellent, especially with regard to the elucidation of the muscular and nervous systems, and the sense organs. Passing from the frog to the rabbit, the student is gradually initiated into the necessary changes arising in the transition from water to air. An illustration of the vascular system of the rabbit would have been a useful addition. The account of the urinogenital organs is perhaps rather brief, and from what is known of their development it would be more correct not to speak of "the kidneys in the adult mammal" as "of *totally* different origin from those of the frog (italics mine)."

Light is thrown on the evolution of the Mammalia by a concise account of some fossil types of Amphibia and Reptilia. This is a distinct innovation

in a work of this kind. The importance of the developmental history of an organ is well emphasised throughout, and there is a chapter devoted to the development of the chick, followed by that of a mammal.

The illustrations are a special feature. They are clear and well chosen, a large number are from original drawings by the author, and most of the others are new to an elementary textbook.

Mr. Woodger has produced a really good book, well written and carefully thought out. It will not only be valuable to the medical student as a supplement to the ordinary practical textbook, but will interest every teacher of zoology.

E. A. F.

ENGINEERING

Elements of Electrical Design. By Alfred Still, F.A.I.E.E., M.I.E.E. [Pp. 528 + xxi.] (London: McGraw-Hill Book Co., 1924. 25s. net.)

Although limited to a single volume, the treatment of the subject is thorough and comprehensive. Commencing with the design or resistances—electromagnets and windings, insulators, etc., the treatment is progressive and covers generally the whole field of D.C. and A.C. engineering appliances both static and rotary.

However complete a theoretical design may be, its ultimate form is decided by practical experience and conditions. The author is therefore justified in avoiding all purely academic or deeply theoretical considerations and in the free use of empirical formulæ—the limitations of such formulæ being always carefully indicated.

Theoretical treatment is not completely excluded where immediate practical application is available, such cases being: equalisation of voltage gradient in di-electrics, thermal conductivity and temperature rise, leakage reactance of a transformer, etc. Other works are referred to for detailed treatment of theoretical points. Such references are usually to English authors of prominence in the electrical design field and readily accessible to English readers.

In all cases the treatment is clear, well illustrated with line-drawings and vector diagrams where necessary, while each chapter concludes with a series of questions embodying the points raised in it.

Particularly noteworthy are the sections dealing with A.C. generators, and the design of windings and insulators for extra high tension circuits.

The concluding chapter is an all-too-short summary of points to be considered in the mechanical design of rotating machinery.

The result is a work which falls readily into the gap existing between the purely theoretical textbook on electrical design and the brief summary of practical requirements found in the electrical engineering handbooks.

A. N. Jackson.

Applied Elasticity. By John Prescott, M.A., D.Sc. [Pp. viii + 666, with diagrams.] (London: Longmans, Green & Co., 1924. Price 25s. net.)

Under the above title Dr. Prescott has given us a book which should prove of service both to the mathematician who takes up a study of the mathematical theory of elasticity and to the engineer who sets himself to the solution of practical problems in elasticity.

In his preface the author states that he has tried to view the subject from the point of view of the engineer rather than that of the mathematician, and that his main object has been to lead up to problems which had a practical application. This he has done, but the book remains a treatise on the mathematical theory rather than a handbook for the engineer. There are certainly many points of the theory which have not been worked out so fully

as in most textbooks on elasticity, but the fundamental problems have been dealt with very thoroughly. Moreover, the examples given of the application of the theoretical results to the solution of practical problems supply a connecting link—too often missing—between the mathematician and the engineer.

The first four chapters deal with the ground-work of the theory, which is treated very clearly and forms an excellent introduction to the subject. The bending of thin rods and plates and the stability of rods and plates are treated very fully, results being worked out in detail for a number of particular cases, with several practical examples. The elastic deformation of spheres and cylinders, the stretching of thin plates, the vibrations of discs (including rotating discs), and contact stresses are all dealt with more briefly. The main interest of the book consists in the use of the principle of minimum energy to obtain approximate methods of solution, especially useful in the case of stability problems. Dr. Prescott has applied the principle successfully, to many of the simpler problems on thin rods, and has given also illustrations of its general applicability by deducing approximate methods of finding the buckling load for a thick beam and for a thin tube: problems which by other methods present very great difficulty. And while the conscientious mathematician may look with disapproval on the approximate solution, and the engineer still base his calculation on experimental results in such cases as these, the method will have undoubted interest for both.

The text throughout is remarkably clear and explicit, each step in the working being fully explained, so that anyone with a fair knowledge of differential and integral calculus can read the book with ease. Clear diagrams are inserted wherever they are necessary to illustrate the text.

The book certainly justifies its publication, for, while it does not contribute much that is new to the theory of elasticity, the practical view that it takes of the whole subject and the clearness with which it is written make it a treatise on Mathematical Theory which can be read with understanding by the engineering student.

H. T. J.

Alternating Current Rectification: A Mathematical and Practical Treatment from the Engineering Viewpoint. By L. B. W. Jolley, M.A., M.I.E.E., Member of the Research Staff of the General Electric Company. [Pp. xviii + 352, with 244 figures and illustrations.] (London: Chapman & Hall, Ltd., 1924. Price 25*s*. net.)

The rectification of alternating currents to produce unidirectional ones is a branch of electrical engineering which is likely to become increasingly important. Certain phases of it have already received very considerable application, chiefly in connection with radio work, and such application seems likely to meet with further rapid extension. Other phases of the subject, however, are of no less importance, so that the volume under review should fill a much-felt want.

Apart also from the above considerations affecting the usefulness of the book, it is especially welcome to find a treatise which breaks fresh ground and opens up new fields in the much-trodden realm of electrical engineering and does not merely repeat (in more or less new wording) what has already been published on many occasions in standard textbooks.

The field covered by the book is necessarily a wide one, since rectification in one or other of its many forms can be applied to energies ranging from microwatts to many kilowatts, at any voltage from practically zero up to at least 100 kilovolts. In connection with this book the author has used the term "rectification" in its broadest sense to include all methods of producing unidirectional from alternating currents; whether by rotary machinery or by static apparatus. While perhaps from some points of view such a

course may be open to criticism, it nevertheless has much to recommend it, particularly as the specialists in one or other part of the subject which touches or affects their own particular line of electrical or radio work are apt to overlook what has been accomplished in other fields and with other aims. A comprehensive survey of the whole subject such as is here provided cannot but help to stimulate thought, and perhaps lead to important developments or applications of certain forms of apparatus into other spheres of work than those for which they were originally designed.

The book is fully illustrated throughout with good clear diagrams, and also with numerous half-tone plates depicting the various forms of apparatus under discussion. It opens with a general introduction to the problem of rectification which summarises also some of the present applications of A.C. rectification and tabulates the various types of rectifying apparatus applicable to these different requirements. The remaining chapters (to the number of 17) are divided into six groups—Part I dealing with Wave Form, including Harmonic Analysis, calculation of mean and effective values in alternating and in rectified current circuits, and a consideration of the appropriate measuring instruments to be used for various purposes in the rectified current circuits. Part II discusses Mechanical Rectification, and includes brief descriptions of various forms of rotary converters and motor converters, and their application and use. A chapter in this section is devoted entirely to Commutator Rectifiers, both of the smaller types such as are often used for battery charging and similar purposes, and of the larger varieties as typified by the Transverter. Another chapter is devoted to Extra-high-voltage Rectifiers of the types used for electrical precipitation, X-ray work, etc., and to vibrating reed and similar smaller types of apparatus. Part III occupies the largest share of the volume and deals with Gaseous Conduction. It includes chapters on the Conduction of Electricity through Gases; Mercury Vapour Rectifiers (theory and descriptions of both metal and glass bulb rectifiers); Vacuum Tube and Thermionic Rectifiers; Gas Tubes, Neon, Tungar, and Magneton Rectifiers; Point to Plate Discharges, Vibrating Flame, Corona and Photoelectric Cell Rectifiers.

The properties of electrolytic conduction in liquids and the operation and use of electrolytic rectifiers generally are set out in the chapters of Part IV; while the two chapters comprising Part V relate to "Wireless Rectifiers." Crystal detectors and three-electrode valve detectors are dealt with in some detail in these chapters, the crystal detectors more fully than the valves. The title could perhaps more correctly have been given as "Wireless Receiving Rectifiers," as those which are frequently used in radio transmitting stations have already been dealt with in Part III of the book. The treatment of the valve detector is confined to a consideration of its mode of operation as deduced from the characteristic curves of the tube, and in this respect is somewhat brief, but doubtless the author has experienced some difficulty in deciding (both here and elsewhere) as to how much should be put in, and what could conveniently be omitted. To have attempted to include everything relating to the subject would have necessitated a whole series of volumes in place of one only.

The title of the last chapter which comprises Part VI of the book, viz. Alternating Current Measurements and Voltage Regulation, is rather misleading, as one expects from it much more than is to be found in the chapter. What is really discussed is the use of rectifiers in connection with such measurements, and even this is confined to the commutator rectifier. Considerable amplification of this chapter would be a welcome addition to the book, since very much more work has been done on the application of rectifiers to A.C. measurements than the author even hints at, and a proper classification and analysis of such methods is needed.

The publishers are to be congratulated on the preparation of this book,

as the printing and general appearance are excellent, and remarkably few misprints have been noticed. Particular mention may also be made of the setting of the mathematical formulæ, which is both good and clear—a point where so many publishers frequently fail—while a uniform system of notation has been used throughout.

All through the book the treatment of the subject is essentially of a practical nature, with the addition of mathematical theory where requisite for the explanation. In no place is it overloaded with unnecessary descriptive matter, while the usefulness of the book is also increased by the addition at the end of each chapter of a bibliography of references to previously published articles dealing more fully with other aspects of the problems.

PHILIP R. COURSEY.

The Measurement of Fluid Velocity and Pressure. By J. R. PANNELL, F.R.Ae.S., A.M.I.Mech.E. Edited by R. A. FRAZER, B.A., B.Sc., A.F.R.Ae.S., with a Foreword by R. V. Southwell. [Pp. vii + 135, with 49 illustrations.] (London: Edward Arnold & Co., 1924. Price 10*s*. 6*d*. net.)

J. R. PANNELL, the author of this posthumous work, met his death in the disaster which involved the destruction of the airship R.38 on her trial flight. He was especially gifted for work relating to the measurement of fluid motion, and his investigation in conjunction with Stanton of the application of the principle of dynamical similarity to fluid motion, briefly referred to in the present work, is his enduring memorial. Their results, co-ordinated by Lees into a comparatively simple formula, are constantly employed in industrial practice in calculations relating to the pressure distribution in flow systems. The practical determination of the motion of a gas in a pipe or other channel would, however, still appear to be one beset with difficulties. And this despite the fact that the Pitot tube dates from 1732, the Venturi tube from 1791 (and not from 1881 as stated in the text). In industrial installations, the measurement of the velocity of a stream of fluid—most frequently air and consequently of little monetary value—is still generally attempted in a manner which should bring a blush to the cheek of one who has heard of the "harnessing of science to industry." The horse would appear to have bolted with the harness, for of our own certain knowledge we are able to assert that the use of an impossible form of Pitot tube, or of a battered and derelict type of windmill anemometer, has enabled more than one industrial concern to achieve a rocketing reputation for being advanced and scientific!

A book devoted to the subject of the measurement of fluid velocity would, we had hoped, do much to remedy this state of affairs. We are afraid, however, that very little can be hoped for in this direction from the present volume. The work has reference more especially to precision measurements of the motion of fluids in problems relating to aeronautics, hydraulics, and ventilation. Chemical technology will still have to be satisfied with its Fletcher anemometer and gauge, which is the combination it most commonly employs, and which receives no mention in the text. On page 98 we read "inclined tube manometers are in use on the wind tunnels at Göttingen and at Washington." We can add that hundreds, possibly thousands, of such gauges are in daily inaccurate industrial use in this country. Pressure measurements are commonly quite well made in chemical technology. All of the precision instruments customarily employed in the industry are absent from the present work.

Within the serious limitations of the subjects dealt with, to which brief reference has been made, we can heartily commend the present work. Chapters are devoted to anemometers of the pressure tube, moving part and

hot-wire types, ship logs, manometers, and the flow of fluids in circular pipes. The work is authoritative and in general correct. The only adverse criticism we have to advance is that the work rather gives one the impression that the subject has been regarded too much from the point of view of what has been achieved at the National Physical Laboratory. The editor would appear to be afflicted with N.P.L. myopia. Evidence of this is very clearly seen in Chapter III, devoted to hot-wire anemometers. The only form of hot-wire instrument that has been extensively installed on a commercial scale is unmentioned. More than ample justice is done to at least one form of anemometer of the directional type used at the laboratory. The original form of directional instrument introduced elsewhere is dismissed as practically an "also ran," accompanied by an incorrect reference. Additional evidence is afforded by the N.P.L. type of Chattock gauge being referred to as the N.P.L. tilting manometer (pp. 14, 91). This is a minor defect, which it is hoped will disappear when the publication of an enlarged second edition of the work is called for. In the meantime the technologist can avail himself of Litinsky's *Messung grosser Gasmengen*.

J. S. G. T.

MISCELLANEOUS

A Long Life's Work. An Autobiography. By Sir Archibald Geikie, O.M., K.C.B. [Pp. xii + 426, with two plates.] (London: Macmillan & Co., Ltd., 1924. Price 18*s*. net.)

Geologists are a long-lived race; and the late Sir Archibald Geikie, the doyen of British geology, was the last exemplification of this rule. He was born in 1835, and during his long life became acquainted with all the great masters of the science from Hugh Miller onwards. To all foreign workers he stood as the embodiment of British geology. He was successively Secretary and President of the Royal Society, and twice President of the Geological Society of London. He was Director of the Geological Survey for twenty-one years, held many other responsible posts, and was accorded innumerable honours both at home and abroad, culminating with the Order of Merit in 1913. Such, in the baldest outline, were the achievements of a remarkable man of science.

Sir Archibald Geikie's life, apart from his childhood and boyhood, may be divided into five main parts: 1855–1871, his early connection with the Geological Survey, and his Directorship of the Geological Survey of Scotland; 1871–1882, occupation of the Chair of Geology in the University of Edinburgh; 1882–1903, the Director-Generalship of the Geological Survey of Great Britain; 1903–1913, Secretaryship and Presidentship of the Royal Society of London; 1913 to November 10, 1924, the date of his death, retirement at Haslemere, and occupation with literary and classical pursuits.

His autobiography is written in that charming style which distinguished his geological as well as his biographical and other purely literary writings, and made so many converts to the science he served and represented so well. We obtain many anecdotal glimpses of the great figures of Victorian science, as well as vigorous thumb-nail sketches of many of his co-workers and contemporaries.

If, however, we turn to the autobiography to learn something of the progress of British geology from Murchison's time onwards, we are somewhat disappointed. The great geological controversies of the last third of last century, in which Sir Archibald Geikie was a leading gladiator, are presented in a colourless way, conveying nothing of the heat, dust, and noise of the conflict; and with no mention whatever of some of the chief protagonists, notably Charles Lapworth and John W. Judd. The accounts of the unravelling of the structures of the North-west Highlands and the Southern Uplands

of Scotland contain nothing regarding the work of previous investigators, especially that of Lapworth, who had essentially solved both problems before the Survey covered the ground. Nevertheless, by implication, the Geological Survey is credited with the final solutions. Nor is the omission remedied by a footnote (p. 215) indicating that the history of the investigation (of the North-west Highlands), and full references, are to be found in the Geological Survey Memoir. However, the aim of the book may have been to present the facts of Sir Archibald's life from an intimate personal angle, precluding the wider references which would have been appropriate to a more formal history of British geology.

Sir Archibald Geikie had an enormous power of work and great organising ability. He possessed a fine literary sense and considerable artistic gifts. His chief scientific publications were his *Scenery of Scotland*, a work which most conspicuously showed his combination of acute geological observation with power of literary expression and artistic skill; his great *Textbook of Geology*; and his *Ancient Volcanoes of Great Britain*, embodying his best research work.

The reorganisation of the Geological Survey was his biggest administrative task. He laid the foundations of the present-day efficiency, and established the wide outlook and high standard, which now distinguish this organisation. His administrative talent also had full scope during his official connection with the Royal Society.

His literary gifts were best displayed in his biographical works on Murchison, Forbes, and Ramsay, his essays on the founders of geology, and in the writings of the evening of his life, when he returned with zest to the classical studies of his youth, which he had never quite abandoned.

The book has two plate illustrations, both portraits, in one of which Sir Archibald Geikie is shown as fellow-student with his grandson. There is a touching appendix in memory of his son, Roderick Geikie, who died as the result of a railway accident in 1910.

We are grateful indeed to the members of Sir Archibald Geikie's family, who pressed upon him their wish that he should jot down for them the recollections of his life. The great public to which he was so well known will welcome this charming record of his long life's work.

G. W. T.

Patents: Invention and Method. By HAROLD E. POTTS, M.Sc. [Pp. vii + 160.] (London: The Open Court Company, 1924. Price 3*s.* 6*d.* net.)

THIS book is a reprint of six papers, some of which appeared in *Nature*, the *Journal of the Society of Chemical Industries*, and the *Chemical Age*, and some were read before the Chartered Institute of Patent Agents. It is very interesting, both on account of the breadth of reading displayed by the author and of the novel philosophical way in which he treats the subject.

Mr. Potts has not produced a textbook—he did not set out to do so; but he has made a contribution to patent literature which is distinctly original in style and should stimulate thought and study amongst both patent agents and inventors.

In the first two papers, entitled respectively "An Application of Mathematics to Law" and "Prediction and Invention in Chemistry," the author is discussing the vexed question of subject-matter in patents, and it is probably not surprising that he has not succeeded in codifying the conditions under which subject-matter will exist, seeing that distinguished judges and learned patent counsel have generally feared to attempt so difficult a task. The first paper, though amusing, hardly carries one any farther, because it merely states the problem in mathematical language, which is to most people not the best medium for so stating it, though it may enable those queer people

who think in mathematical symbols to understand the nature of the problem better.

The third paper, "Language and Style in Patent Law," is a very interesting one and may well be read by those who have the drafting of patent documents, and indeed by many others who indulge in scientific writing. The idea that literary style is of no importance in scientific writing is a common fallacy, and care in this particular would often greatly improve the scientific value of the work. It is very true, as the author remarks, that "language does more than represent thought: in the very act of expression it moulds the fluid thought."

In the fourth paper methods of definition are very well discussed from a logical standpoint, and, as accurate definition forms so important a part of the patent agent's metier, may well be studied, though whether any draftsman could consciously apply any particular logical method in drawing a claim is another matter. Still, a study of logic tends to clarify the mind.

The last two papers, especially that dealing with the "Influence of Patent Law on the Evolution of Research," are probably the most practical in the book, and should be of interest to works managers and others interested in industrial research and manufacture.

Altogether the book is very interesting and readable.

The Atmosphere and its Story. By E. Frith. [Pp. 204, with illustrations. (London: The Epworth Press, 1924. Price 6s. net.)

This book is made up of a series of short essays on such subjects as rain, snow, dew, lightning, lightning-conductors, meteorites, etc. These essays, although sketchy and popular in form, are pleasant to read and contain remarkably few inaccuracies for a work of this type. Such inaccuracies as have been noted are not of a very serious kind: on page 119, for example, it is stated that "Meteorologists admit as a well-authenticated fact that when the moon is near its full (on either side), clouds have a tendency to break up and disperse as the moon rises in the heavens. What the influence of the moon is that causes this dissolution of cloud remains a problem." The first of these statements might be agreed to by most meteorologists, but they would probably like to add that "a precisely similar tendency exists when the moon is absent from the sky, only the dissipation of the clouds is then not so easy to observe." Sir Napier Shaw has written illuminatingly on this subject in his book *Forecasting Weather*, published nearly twenty years ago.

The illustrations are uniformly good, and the book can be recommended.

E. V. Newnham.

Skill in Work and Play. By T. H. Pear, M.A., B.Sc. [Pp. 107, and 7 illustrations.] (London: Methuen & Co. Price 4s. net.)

This book will be welcomed by teachers as a valuable contribution to the scientific study of the practice of education, especially in that part concerned in the acquisition of skill. Captains of industry, too, and welfare superintendents, seeking the fundamental conditions of efficiency will do well to peruse this thoughtful introduction to a little understood subject.

W. C. B.

A Textbook of General Science. Vol. II. Introduction to Chemistry, Geology, and Biology. By G. H. C. Adlam, M.A., B.Sc., and O. H. Latter, M.A. Science for All Series. [Pp. viii + 243, with 67 illustrations.] (London: John Murray. Price 3s. 6d.)

This book covers a wide range, and for those seeking such a test gives a very fair survey of the ground covered. It is much to be feared that as a schoo l

text it is liable to become a mere cram-book. Scientific study nowadays offers so wide a scope that there is always a danger that the teaching of science will be an introduction to scientific knowledge rather than to scientific methods. The boiling-down that necessarily takes place is rather liable to lead by way of loose logical processes to misleading generalisations. For instance, on page 89 we read: "Fermentation is thus shown to be a chemical and not a 'vital' reaction. It is brought about by a definite substance called zymase. . . . Zymase belongs to a class of non-living albuminous substances *which are the secretions of living organisms.*" (The italics are mine.) Though to the scientific reader the meaning is plain, for the young student more careful wording is necessary. This is not by any means an isolated instance.

W. C. B.

With My Wife Across Africa. By Col. J. C. B. Statham, C.M.G., C.B.S. [Pp. 324, with 55 illustrations and 3 maps.] (London: Simpkin, Marshall, Hamilton, Kent & Co. Price 12*s*. 6*d*. net.)

To anyone who enjoys a thrilling record of travel, this book can be thoroughly recommended.

The route chosen by Col. Statham and his wife led, for the first part of their journey, through one of the least known parts of Africa—Southern Angola. Starting from Mossamedes, they trekked to the Kunene, then on to the Okovango, down which they canoed for five hundred miles, and finally braved a wide stretch of the northern Kalahari before they reached the safety and civilisation of the Zambezi.

The risks and hardships were greater than even an experienced traveller such as Col. Statham had expected, for in addition to "natural" dangers of crocodiles, hippo., lions, the seemingly endless rapids of the river and the putrid water of the few Kalahari water-holes, they were confronted with treacherous carriers while in waterless country, when, to make matters worse, Col. Statham was severely injured in saving his dog's life from a "roan." And, as if this were not enough adventure, danger was threatened from two white men, one an outlaw and the other presumably a madman, who shortly afterwards killed the outlaw and then shot himself.

The book, however, is more than a personal record. The author has an eye for the lie of the land as well as for natural beauty, and a mind interested in the political problems of the region as well as the physical and climatic, although his special interest is in its animal life.

Besides pertinent comment in the narrative, there are appendices dealing with the people, the game animals and birds, the physiography, geology and climate, and the flora. The question of the desiccation of the area is discussed, and although Professor Schwarz's conclusions as to the possibility and value of flooding the Kalahari are perhaps accepted too readily, the first-hand description of the various watercourses is valuable.

The political aspect is ably dealt with, the difficulties of administration in this sparsely peopled and remote frontier zone being insisted upon, and a gracious tribute is paid to the Portuguese, "who are the kindest people I have met in all my world-travels."

Finally, although a big-game hunter, Col. Statham is an ardent advocate of hunting with a camera and of the urgent need for preservation.

M. R. Shackleton.

A Game Ranger's Note-book. By A. Blayney Percival. [Pp. xv + 374, with 32 illustrations from photographs and a map.] (London: Nisbet & Co., 1924. Price 18*s*. net.)

This book will remain the standard work on Big Game Shooting in Kenya Colony for many a long day. It is full of those very details which the sports-

man wants to know and which he so rarely finds in books. Mr. Percival's unique experience among the wild beasts of East Africa is hardly likely to be equalled and certainly not to be surpassed by any single hunter again, even though he were equipped with the author's powers of observation and knowledge of natural history.

All those who have hunted big game in the past, as well as those who contemplate a trip in the near future, will read this book with much pleasure and no little profit. The author, who has a keen sense of humour, tells us what he has to say in a most entertaining manner.

In speaking of the laugh of the hyena, Mr. Percival regards it as a cry of rage. This is partially true, but all the circumstances under which he gives vent to this demoniacal noise seem to vary, and the reviewer would be more inclined to ascribe it to occasions when the beast is excited, disappointed, or annoyed.

The author speaks of three races of leopards, namely: *Felis pardus fortis, F. p. suahelica*, and *F. p. nanopardus*. Personally the reviewer would very chary of speaking of more than two, and these two have quite distinctive anatomical characteristics. They are *Felis pardus* and *F. p. nanopardus.*

The first-mentioned has a very wide range from the coast to the forests of the interior and is in point of fact ubiquitous. Its coloration varies from a very pale creamy buff ground colour in the coastal regions to actual melanism in the Abyssinian and other forests. In the reviewer's collection there are leopard-skins of every gradation of tint, and it would be difficult on colour characteristics alone to divide them into two local races or subspecies as the coloration gradually darkens from the coast species to those found in the mountainous and forest regions of the interior. The world's record leopard was obtained within twenty miles of the coast in British Somaliland, and it is considerably bigger than any leopard obtained in the far interior, although on the whole possibly the forest animals are larger, as they are seldom seen or shot, whereas in the coastal regions, particularly in Somaliland, there is constant war being waged against them by the Somalis. With regard to *Felis pardus nanopardus*, this diminutive, rare, and distinct subspecies is only found in the Horn of Africa and appears to be distributed chiefly in the waterless regions from the coastline of Italian Somaliland through the Nogal Valley to the Ogaden country. Only four specimens have so far been obtained, one of which was lost in transit to England. The remaining three are in the Natural History Museum. The reviewer's specimen was shot within a few miles of Obbia. Thus it will be seen that every variety of leopard is found in the Somali country, from *Felis pardus nanopardus* to the world's record specimen, which was 9 ft. 4 in. (undressed skin). As regards lions, he is a bold mammalogist who attempts to divide them into local races.

Unless the reviewer is very much mistaken this book will pass into more than one edition, and attention may here be drawn to one or two errors which should be corrected in future editions. On page 154 there is a printer's error; *Hyæna striata schillongsi*, should read *Hyæna striata schillingsi*, while on page 176 the fibre plant which the elephants in the north-eastern frontier province so enjoy in the dry weather is *Sansevieria chrenbergii*. Lastly, the practice of calling Somalis Somals should be dropped. No Somali was ever heard to speak of his brethren as Somals.

The book is handsomely produced and the author's and Mr. Martin Johnson's photographs are all good, while the map giving the present Game Reserves in Kenya Colony completes a very good book, which is destined to find a certain place among the equipment of every sportsman or naturalist who visits Kenya.

R. E. Drake-Brockman.

The Social and Political Systems of Central Polynesia. By R. W. WILLIAMSON, M.Sc. In Three Volumes. [Pp. vol. i 438, vol. ii 496, vol. iii 487.] (Cambridge: At the University Press, 1924. Price 75*s*. net.)

THIS book may best be described as "an exhaustive treatise," and will undoubtedly become a classic work on its particular subject. When we consider that the author has laboriously gleaned all his knowledge by sifting the works of other writers (for one gathers that he has never resided in Polynesia, and that his personal knowledge of the Pacific, whether Polynesia or Melanesia, is limited to an expedition in 1910 to Papua) it is amazing how accurate he is and how sound are the conclusions at which he arrives. In fact, the publishers' announcement on the outside cover disarms such criticism from the start, for it is there stated that:

". . . In these three volumes the author has attempted to sift, arrange, and co-ordinate the vast amount of ethnographical material which has been accumulated in the past by travellers, explorers, missionaries, government officials, and others. . . ."

One wonders whether the order in the above sentence is intentional, and if so whether such a low opinion of the value of the work done in ethnology by government officials is really held by the author also. Recent correspondence in *The Times* has shown that the authorities at the Colonial Office and, indeed, government officials in general, are nowadays by no means blind to the value of applied ethnology where administration is concerned.

Anyhow, this book can certainly take a place of honour as a companion to Rivers's great work on the *History of Melanesian Society*, and as one of the valuable flowers of ethnological research that may be said to have blossomed from Frazer's immortal *Golden Bough*. The book is indeed a scholarly work of learning, and is evidently not written for the ordinary reader; but even to the student of Pacific ethnology it is not a book that can be easily assimilated at one perusal, and the style cannot be said to flow in the easy way that made the *Golden Bough* such delightful reading to one and all. As an example may be quoted a passage taken at random from the first volume.

". . . . Krämer says that Lealali politically organised Savai'i. There can, I think, be little doubt that Pili was a Manu'an; and it is noticeable that we have the two statements, one that Pili's son Tolufale founded Savai'i, and the other that Lealali organised it. I find no ground for suggesting that Tolufale and Lealali were the same person; but, according to one of Krämer's genealogies, Lealali was the grandson of a woman, Pili-le-So'opili who married a tuimanu'a; so it is quite possible that Lealali was a member of the Pili family, of whom the god Pili was the ancestor, and he may have been descended from Tolufale. If he was a descendant of Pili, he also was of Manu'an origin. On the other hand, there are the genealogies, given by Stuebel, von Bülow and Krämer, according to which Lealali was the son of Ationgie. . . ."

With regard to the great Polynesian trek into the Pacific the present writer concurs with the author's view that the main, even if later, wave (that of the Tangaroan cult) reached Fiji more or less *en masse*, and after settling there for a number of generations dispersed to colonise other groups, including New Zealand. And it is mostly with the social and political systems of these "Tangaroans" that the author is concerned. If there is any adverse criticism of such a valuable book to be made, it is only to regret that perhaps the author has been apt to devote at times too much attention to one aspect of the subject, and too little to others. For instance, those of us who have lived among and in close contact with these islanders of the Pacific realise that the outstanding feature in the social and political systems is that relating to the tenure of land. Everything hinges on this, for to these

natives—who do not, or at least did not, import their food from the outside world—land meant life. The ownership of land and the right over its produce is and always has been the one vital matter to these people, and the gradual evolution of the land system is, therefore, of the highest importance, yet the author only devotes two chapters out of the forty-four chapters of his book to this, and in his Recapitulation somewhat summarily dismisses the subject by stating: "... The evidence as to the controlling power of the head of the group over the land of the group is too detailed and diverse for more than a very general survey. . . ." But in another place he hints at the possibility of a further book, and one would be glad to think that this important question may be then more fully dealt with. One also is led to hope that the author on some future occasion intends to discuss at length the fascinating problem of the migration of this race, and he may even have some further light to throw on some of the most suggestive similarities between the Polynesians and the ancient inhabitants of Central and South America, and even the Caribs of the West Indies.

One cannot leave this book without reference to the admirable manner in which the subject of Totemism, as it relates to the Pacific, is discussed in volume two. This is one of the most valuable contributions to the subject I have yet seen.

Altogether it is a book that will live.

T. R. St. Johnston.

Everyday Physics. By H. E. Hadley, B.Sc. [Pp. viii + 474, with 425 illustrations.] (London: Macmillan & Co. Price 6*s.* 6*d.*)

The Purpose of Education. By St. George Lane Fox Pitt. [Pp. 91 + xxix.] New cheap edition, revised and enlarged. (Cambridge: at the University Press. Price 4*s.* net.)

The Humanising of Knowledge. By James Harvey Robinson. [Pp. 117.] (London: Hodder & Stoughton. Price 5*s.* net.)

These three books fortuitously coming together to the reviewer's hand have to some extent a common purpose, perhaps best expressed in the title of the second book on the list, namely, the humanising of knowledge.

The publication of a cheap edition of *The Purpose of Education* affords to all scientific teachers and thinkers an opportunity that should not be missed of adding to their library a book that is a milestone in the progress of human thought. Teachers particularly will find it highly stimulating, and, what is more, curative in a real sense of the false ideals of to-day. The physicist and the biologist will both appreciate an angle of view that so well accords with their own line of approach to those problems which form the common basis of all knowledge.

The concluding summary is indicative of the high tone and broad lines of the whole book. Two short extracts will suffice.

"The belief that 'we were meant to be happy,' irrespective of all evidence to the contrary, is a delusion deeply implanted in our natures. . . ."

"... If instead of postulating the incongruity that 'we were meant to be happy,' we were to say that we all should, as in duty bound, learn how to become truly happy, we should be nearer the mark; and, indeed, this amended postulate supplies us in a nutshell with a serviceable formula for the purpose of education."

The Humanising of Knowledge, by an American writer, deals with a similar subject, though from a somewhat different standpoint. It shows how the enormous mass of detail that has accumulated with modern knowledge tends to divorce from it all human interest. The writer makes an appeal for another line of approach to scientific and indeed all knowledge from the

starting-point of man himself, or rather, from his position in the universe. A quotation will make the writer's intention clear.

"The object of this little volume is the attempt to reassess our failures and possibilities in the development of intelligence; especially to make clear why . . . we have failed to make connection between education on the one hand and the obligations, pitfalls, and amenities of life on the other."

The book is worthy of perusal.

As the title suggests, *Everyday Physics* is an attempt to humanise knowledge. It is a hopeful sign that the recent school texts in science are being brought into close touch with daily life, though most of them still err in being too academic. Even though it may not be found possible to use Mr. Hadley's book as a school text, it should at least find its place in the science library.

W. C. B.

College Manual of Optics. By Lloyd William Taylor. [Pp. ix + 236.] (Boston, U.S.A.: Ginn & Company. Price 12*s*. 6*d*. net.)

This is a treatise of a somewhat unusual character, being neither a laboratory manual nor a theoretical textbook, but something between the two. It is intended, in fact, to take the place of both—an ambitious project, which comes as near success as could reasonably be expected in such a small compass. In parts, notably the sections relating to interference, it is extraordinarily good, as of course is but natural for a book issuing from Michelson's laboratory. Throughout it is clear and straightforward, abounding in useful practical hints and interesting and instructive experiments. If one could turn a student loose in an optical laboratory with only this book to help him, he would learn far more optics than from any conventional course.

On the whole there is little to criticise. But we have always regarded Nicol as of Scottish, not German, extraction. At any rate, is there not a tablet to his memory in Edinburgh University? And although it is most necessary to discourage the student from clamping too tightly the end plates of a polarimeter tube, the reason against such procedure is not that it makes them "optically active," but rather that it may introduce elliptical polarisation. Objection might also be taken to one or two other details in the section on polarimetry, as for instance the omission of the Lippisch polariser in favour of the Laurent, now practically obsolete in serious work.

There are a number of appendices dealing with laboratory manipulation which are for the most part of a very useful character (*e.g.* on silvering and half-silvering mirrors). But should it be necessary in a book of this standard to include explanations of the simple vernier and spherometer? If a student is really in need of such instruction, the probable fate of an interferometer entrusted to him does not bear contemplation.

W. E. Curtis.

Optical Measuring Instruments; their Construction, Theory, and Use. By L. C. Martin. [Pp. viii + 270.] (London: Blackie & Son, 1924. Price 17*s* 6*d*. net.)

The application of optical methods to routine and research work in all branches of science, pure and applied, has in recent years been so successful that to ignore its possibilities is to place a serious handicap on the investigator. On the other hand, the use of a strange instrument, the basic principles of which are often but dimly understood, is a most fruitful source of confusion and error. There can be no doubt that the time was ripe for such a book as this, which deals with the construction and use of the more important optical instruments from the point of view of a user who may be neither a physicist nor a technical optician. The ground covered is considerable; range-finders, surveying and astronomical instruments, spectrometers, refractometers, polari-

meters and photometers claim most of the 270 pages, but minor instruments and a chapter dealing with errors and accuracy of observations also find a place. Dr. Martin had succeeded in compressing a remarkable amount of information into this comparatively small compass without the least sacrifice of clarity of treatment or literary form.

At the same time the very comprehensiveness of the book renders it almost impossible that every instrument should receive really adequate treatment, and we must confess that we should have preferred to see an expansion of the material into two volumes, one dealing, say, with range-finding, surveying, and astronomical instruments, and the other with the more purely physical types. It would then have been possible to remedy certain omissions, such as colorimeters and instruments employing interference effects. It is lack of space, no doubt, which is also responsible for the absence of any description of the eye or of the common forms of eyepiece, for both of these would seem naturally to find a place in such a book as this.

The general appearance of the book is very attractive; the diagrams are excellent and misprints exceedingly few. The diagram illustrating the use of a right-angled prism as an index mirror for the sextant is obviously wrong. And on p. 206 we read concerning polarising prisms that "Prisms with end faces which are not normal to the transmitted light produce a certain amount of elliptical polarisation in the transmitted light." This is somewhat misleading, as the elliptical polarisation introduced by such prisms is due either to strain in the balsam film or to partial depolarisation of the plane-polarised beam, owing to cloudiness of the Iceland spar. The effect of reflection or refraction is in general a *rotation* of the plane of polarisation, an effect which may cause serious discrepancies in photometric work.

Not only the instruments, but also the chief errors and their elimination are discussed with conscientious thoroughness, and this constitutes a very valuable feature. But in the discussion of critical angle refractometry a very common and serious error is overlooked, namely, that the film of substance must be of sufficient thickness. On the other hand, Dr. Martin deserves well of the physics student and teacher for his denunciation of the "split-beam" method of determining prism angles, the fundamental unsoundness of which is too little appreciated.

It is certain that the book will appeal to a very wide circle of workers, and might very well find a place in every physics laboratory also.

Practical Microscopical Metallography. By R. H. Greaves, D.Sc., and H. Wrighton, B.Met. [Pp. x + 125, with 184 figures.] (London: Chapman & Hall, Ltd., 1924. Price 16*s.* net.)

In the preface to this volume the writers state that their intention was to provide, within a small compass, a set of typical photomicrographs, suitably annotated and accompanied by an account of such related matters as might profitably occupy the minds of students during the hours devoted to microscopical work. From this point of view it may be said at once that the book accomplishes the aims of its authors.

The preliminary chapters deal in a general way with the technique of microscopical examination. It must be admitted that this part of the work is somewhat slight. In the first place there is no detailed discussion of the optics of the microscope, which is perhaps somewhat unfortunate, for, as a general rule, the average student of metallurgy is insufficiently acquainted with the physics of this instrument. It must be added that, although the optics of the microscope are not considered, some useful hints are given on illumination.

The micrographical part of the book is well arranged. As is to be expected, considerable attention is devoted to iron and carbon steels. There is a

brief discussion of the equilibrium diagram, and the various structures that can be obtained by different heat treatments are illustrated in many excellent photographs. A careful reading should subsequently enable the student to indicate in a general way the type of structure to be expected after any given treatment which is not too complicated. From carbon steels, the consideration of alloy steels naturally develops, and some mention is made of the nickel steels, the manganese steels, the nickel chromium steels, etc. The treatment here is necessarily brief, which is perhaps an advantage, for the alloy steels are so complicated that a simple and brief introduction to them can lay a better foundation than a wider and more comprehensive treatment.

The last four chapters deal with the metallurgy of copper and its alloys together with some alloys of aluminium and zinc, the treatment being similar to that pursued in the case of iron.

One interesting feature of the book is the fact that not only does it consider the various constitutional diagrams from what might be termed an academic standpoint, but it also contains notes dealing with the physical properties and manufacturing applications of many of the alloys considered. The most striking aspect of the book, however, is the very fine series of photographs, to the number of more than a hundred, with which it is illustrated. These photographs set up a standard which even the most careful and assiduous student can hardly hope to reach until after long experience.

A. A. D.

The Science of Metals. By ZAY JEFFRIES, B.S., Met.G., and ROBERT S. ARCHER, M.S. [Pp. xvii + 460.] (London: McGraw-Hill Book Co. Price 25*s.* net.)

THE opinions of the author of every book are generally reflected in its contents. The scientific study of the properties of metals, however, is essentially practical, and the results of experiment should be stated as fairly as possible in order that the reader may draw his own conclusions. In *The Science of Metals* the authors have followed the opposite course. They devote a large part of their book to the discussion of purely theoretical considerations with scraps of experimental evidence—in many cases without references—thrown in here and there to support their views. Further, many statements are made for which, so far as is known, the evidence is even contrary, as for example that on page 164, dealing with the mechanical properties of metal crystals, where they state: "Any deformation short of rupture produced by an external force disappears on removal of the force." It may also be mentioned that their theories of work-hardening and the recrystallisation of metals are not universally accepted by metallographists, which makes an unbiassed account of these phenomena even more desirable.

The book also suffers greatly from repetition, almost the same words appearing again and again. For example, the question of the deformation of metals is dealt with in the chapters on the crystalline structure of metals, on the amorphous theory, on crystal growth and recrystallisation, on the mechanical properties of metals, on the hardness of metals, and on the structures of aggregates (Chapters iii, iv, v, vi, x, and xi). Again, there is no connected account of the metallography and properties of iron and steel, as these are dealt with under several headings involving further unnecessary repetition and confusion. This is due partly to the fact that much of the material in the book formed a series of articles in *Metallurgical and Chemical Engineering*, where a fault of this kind is not so apparent. Many elementary definitions are given, such as the difference between a mixture and a chemical compound (p. 220), which it might be assumed that readers of a book of such an advanced nature would know. If these were excluded and the rest of the

material condensed, this book would be more welcome to scientific workers, as the authors have realised that the study of metals is only a branch of the study of the structure of matter as a whole, and that the problems of the metallographist are only a specialised form of those of the chemist and physicist.

C. F. Elam.

The Lake-Villages of Somerset. By Arthur Bulleid, L.R.C.P., F.S.A. [Pp. 78, with 28 plates and 6 illustrations.] (London: Folk Press, Ltd. Price 2*s*. net.)

In this admirable little book Mr. Arthur Bulleid, the discoverer of the Glastonbury lake-village, describes this site, and another, of a similar nature, at Meare, in the same locality. The volume deals mainly with the famous Glastonbury remains, and these are described clearly and with much detail. The illustrations are numerous and excellent, and it is altogether to the good that this important English settlement of the Age of Iron should have been found and investigated by so careful an archæologist as Mr. Bulleid. The manner in which the hut dwellings of Glastonbury were built, and the methods adopted to raise the whole occupied area above the level of the swamp, are indicated, while the utensils, tools, and weapons used by the inhabitants are described adequately and in an interesting manner. Mr. Bulleid exposes the unfounded nature of the suggestion, by Sir Wm. Boyd Dawkins, that the Glastonbury village was sacked, and the people living in it destroyed, by an enemy attack. The book can be recommended with confidence to all who are interested in the inhabitants of this country just prior to the Roman occupation.

J. Reid Moir.

Principles of Literary Criticism. By I. A. Richards, Lecturer in English, Magdalene College, Cambridge. [Pp. 290.] (London: Kegan Paul, Trench, Trübner & Co., 1925. Price 10*s*. 6*d*. net.)

A tailor who is invited to construct a suit of clothes for a customer begins by showing him numerous samples of various fabrics for selection. In this book the reader is presented with thirty-five similar samples of various literary theories in as many chapters and, after hearing statements regarding their " values," and so on, he is asked to make his own choice. He will find some well-written passages, some clever sentences and observations, and many short quotations from authors whom (possibly) he cannot identify; but (if he is as dull as ourselves) he will probably be still looking for the " principles " promised in the title, even when he closes the work. It is not a short work, but is typically " modern " in its allusiveness, elusiveness, and inconclusiveness; and what light it shows is nebular rather than stellar. The history of literary criticism has generally proved to be a history of defeats, and the academic point of view to be the worst one: but the tactics of this book are so Fabian that they are not likely, we think, to achieve either success or disaster, though they will keep us wandering at leisure in pleasant places.

R. R.

The Ants of Timothy Thümmel. By Arpad Ferenczy. [Pp. 320.] (London: Jonathan Cape, 1924. Price 7*s*. 6*d*. net.)

Some of the most permanent works in literature are those which, in the guise of fantastic or impossible tales, teach us in allegory the truth about mankind or attempt to foretell our future; and *Gulliver's Travels*, *Peter Wilkins*, *The Coming Race*, and *The Time-machine* combine so much philosophy with amusement that we are doubly edified. This book is of the same class, except

that we find in it no parable of adventure, but only a history of certain small tribes of African ants found written by those creatures on leaves by the ingenious Thümmel, who we regret to hear died by his own hand in consequence of the ridicule which his discovery brought upon him—not a rare reward of discovery. Regarding who exactly the compiler of the book may be, we have our own doubts, which we do not propose to discuss; but the end of the work contains some admirable *Notes on the Social Life of the Ants*, made with the assistance of Miss E. L. Cheesman, F.Z.S., F.E.S. (now in the Pacific), who seems to hold the same position in regard to the Zoological Society of London that Thümmel held with regard to the Zoological Society of Budapest. These notes include copious references to the large literature on ants, and much pleasant information regarding those highly civilised Lilliputians of the soil.

Like ourselves, the ants of the Aruwimi Glade commenced æons ago with a Golden Age—when they worshipped the original goddess-ant who dwelt invisible somewhere at the top of a tree in the glade. Then, like us too, they fell and developed kingcraft, priestcraft, warcraft, prophetcraft, schoolcraft, and even authorcraft—to invent some useful words. But these innovations set them all by the antennæ, and their corpulent priests, heavy-jawed warriors, and enterprising prophets exploited the innocent workers in a shameless and often sanguinary manner. How the Aruwimi ants finally escaped from these tyrants and learnt that "the only clear title to life is labour" will be discovered by the persevering reader.

Mark Twain pointed out that an ant will sometimes crawl to the top of a stalk and down again the other side rather than walk, more simply, round it; but so will even philosophers. We must not depreciate their intelligence in consequence; and the lesson of this book is that both have still far to go before they reach the ultimate wisdom.

R. R.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Leçons sur la Composition et les Fonctions Permutables. Par Vito Volterra and Joseph Pérès. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1924. (Pp. 184.) Price 20 frs.

An Introduction to Mathematical Probability. By Julian Lowell Coolidge, Professor of Mathematics in Harvard University. Oxford: at the Clarendon Press, 1925. (Pp. xii + 215.)

Examples in Mechanics. By F. W. Harvey, M.A., B.Sc., Lecturer in Mathematics, Battersea Polytechnic, London. London: Methuen & Co., 36 Essex Street, W.C. (Pp. vi + 82, with 11 diagrams.) Price 2*s.* net.

Introduction to Theoretical Physics. Volume I. By Arthur Haas, Ph.D., Professor of Physics in the University of Vienna. Translated from the third and fourth edition by T. Verschoyle, M.C., B.Sc., A.R.C.S., with a Foreword by Prof. F. G. Donnan, F.R.S. London: Constable & Co., 10 Orange Street W.C. 2., 1924. (Pp. xv + 331.) Price 21*s.* net.

Radioactivity and the Surface History of the Earth, being the Halley Lecture delivered on 28th May, 1924, by John Joly, Sc.D., F.R.S., Fellow of Trinity College, Dublin. Oxford: at the Clarendon Press, 1924. (Pp. 40.) Price 4*s.* net.

Space, Time, Motion. An Historical Introduction to the General Theory of Relativity. By Professor A. V. Vasiliev. Translated from the Russian by H. M. Lucas and C. P. Sanger. With an Introduction by Bertrand Russell, F.R.S. London: Chatto & Windus, 1924. (Pp. xxiii + 232.) Price 7*s.* net.

Relativity and Common Sense. By F. M. Denton, A.C.G.I., M.I.E.E. Associate Head of Department of Electrical Engineering and Applied Physics, Northampton Polytechnic Institute, London. Cambridge: at the University Press, 1924. (Pp. xvii + 479.) Price 10*s.* 6*d.* net.

Physics. A Textbook for Colleges. By Oscar M. Stewart, Professor of Physics, University of Missouri. New York and London: Ginn & Co. (Pp. viii + 723, with 466 figures.) Price 17*s.* 6*d.* net.

Quatre Conférences sur la Théorie de la Relativité. Faites à l'Université de Princeton. Par Albert Einstein. Traduites de l'Allemand par Maurice Solovine. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1925. (Pp. 96.) Price 12 fcs.

Sur L'Électrodynamique des Corps en Mouvement. Par A. Einstein. Traduit par M. Solovine. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins. (Pp. 56.) Price 6 frs.

Statics. Including Hydrostatics and the Elements of the Theory of Elasticity. By Horace Lamb, M.A., LL.D., Sc.D., F.R.S., Honorary Fellow of Trinity College, Cambridge. Second Edition. Cambridge: at the University Press, 1924. (Pp. xii + 357.) Price 12*s.* 6*d.* net.

Vues Générales sur la Théorie le da Relativité Traduction autorisée accompagnée d'une étude sur l'œuvre de Professeur Eddington et de notes par Thomas Greenwood, M.A., Ph.D. Preface de M. Paul Painlevé. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1924. (Pp. xxiv + 101.) Price 10 frs.

Nouvelles Vues Faraday-Maxwelliennes sur la Propagation de la Lumière. Par Charles L. R. E. Menges. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1924. (Pp. 44.) Price 5 frs.

Chemical Dynamics of Life Phenomena. By Prof. Otto Meyerhof. London: J. B. Lippincott Company, 16 John Street, Adelphi. (Pp. 110.) Price 3$.

Cotton-Cellulose. Its Chemistry and Technology. By A. J. Hall, B.Sc., F.I.C., F.C.S. Chief Chemist, The Silver Springs Bleaching and Dyeing Co. London: Ernest Benn, Ltd., 8 Bouverie Street, E.C.4., 1924. (Pp. 228, with 70 illustrations.) Price 30*s.* net.

The Rare Earths. Their Occurrence, Chemistry, and Technology. By S. I. Levy, M.A., Ph.D., F.I.C. Second Edition. London: Edward Arnold & Co., 1924. (Pp. xvi + 362.) Price 18*s.* net.

The Elements of Colloidal Chemistry. By Herbert Freundlich. Translated by George Barger, F.R.S. London: Methuen & Co., 36 Essex Street, W.C. (Pp. vii + 210.) Price 7*s.* 6*d.* net.

Synthetic Organic Compounds. By S. P. Schotz, D.Sc., A.R.T.C., F.I.C. Consulting Research Chemist and Chemical Engineer. London: Ernest Benn, Ltd., 8 Bouverie Street, E.C.4., 1925. (Pp. 412.) Price 45*s.* net.

Les Colloïdes. Par J. Duclaux. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1924. (Pp. 290.) Price 15 frs. net.

Chemistry in the Service of Man. By Alexander Findlay, M.A., D.Sc., F.I.C., Professor of Chemistry, University of Aberdeen. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1925. (Pp. xiv + 300, with 4 plates and 26 diagrams.) Price 6*s.* net.

Chemicals. By A. W. Ashe and H. G. T. Boorman, A.I.C., with a Foreword by H.R.H. the Prince of Wales and general Introductions by the Rt. Hon. Sir Eric Geddes, G.C.B., Sir Max Muspratt, Bart., and R. G. Perry, C.B.E., being vol. vii of "The Resources of the Empire Series." London: Ernest Benn, 1924. (Pp. xxvi + 207.) Price 21*s.* net.

This volume contains statistics regarding the pre-war and post-war commerce in chemical substances, and should be of great value in deciding upon future lines of development and so indirectly upon the orientation of chemical research in this country.

Callinicus. A Defence of Chemical Warfare. By J. B. S. Haldane. Sir William Dunn Reader in Biochemistry, Cambridge Univeaity. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C. 1925. (Pp. viii + 84.) Price 2*s.* 6*d.* net.

An Introduction to the Literature of Chemistry. For Senior Students and Research Chemists. By F. A. Mason, M.A., Ph.D., F.I.C. Oxford: at the Clarendon Press, 1925. (Pp. 41.) Price 2*s.* net.

A Treatise on Physical Chemistry. A co-operative effort by a group of Physical Chemists. Edited by Hugh S. Taylor, D.Sc., Professor of Physical Chemistry, Princeton University, Princeton, N.J. In two volumes. London: Macmillan & Co., St. Martin's Street, 1924. (Pp. 1359.) Price 50*s.* net.

Bituminous Substances. Scientific Progress of Practical Importance during the last Fifteen Years. By Percy Edwin Spielmann, Ph.D., B.Sc., F.I.C., A.Inst.P., A.R.C., Sc. With a foreword by J. Kewley. London: Ernest Benn, 8 Bouverie Street, E.C.4, 1924. (Pp. xv + 206, with 44 illustrations.) Price 15*s.* net.

Practical Chemistry by Micro-methods. By Egerton Charles Grey, D.Sc., F.I.C., M.R.C.S., Professor of Chemistry, Government Medical School, Cairo. Cambridge: W. Heffer & Sons, 1925. (Pp. ix + 124.) Price 4*s.* 6*d.* net.

Chemical Symbols Formulæ and Calculations. By A. W. Wellings, B.Sc., Chemistry Master at Courtenay Lodge. London: Methuen & Co., 36 Essex Street, W.C. (Pp. viii + 96.) Price 3*s.* net.

The Face of the Earth. By Eduard Suess. Translated by Hertha B. C. Sollas, under the direction of W. J. Sollas. Vol. v. Indexes and maps. Oxford: at the Clarendon Press, 1924. (Pp. xvi + 170.) Price 15*s.* net. Complete set 105*s.* net.

The Origin of Continents and Oceans. By Alfred Wegener, Professor of Geophysics and Meteorology at the University of Gratz., Austria, Translated from the third German Edition by J. G. A. Skerl, M.Sc., with an Introduction by John W. Evans, C.B.E., F.R.S., President of the Geological Society. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xx + 213, with 44 illustrations.) Price 10*s.* 6*d.* net.

Crop Production in India. A Critical Survey of its Problems. By Albert Howard, C.I.E., M.A. Director of the Industry of the Plant Industry, Indore, and Agricultural Adviser to States in Central India. Oxford: at the University Press, 1924. (Pp. 200.) Price 10*s.* 6*d.* net.

Plant Forms and their Evolution in South Africa. By J. W. Bews, M.A., D.Sc. Professor of Botany in the Natal University College, . London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1925. (Pp. viii + 199, with 31 figures.) Price 12*s.* 6*d.* net.

Organic Adaptation to Environment. By George Elwood Nichols and seven others. Edited by Malcolm Rutherford Thorpe. Newhaven: Yale University Press. London: Oxford University Press, 1924. (Pp. xviii & 312, with 34 figures.) Price 18*s.* net.

Outlines of Economic Zoology. By Albert M. Reese, Ph.D. Professor of Zoology in West Virginia University. Second Edition. Philadelphia: P. Blakiston's Son & Co., 1012 Walnut Street. (Pp. xxx + 318, with 194 illustrations.) Price $2.50.

A Textbook of Biology. For Students in General, Medical and Technical Courses. By William Martin Smallwood, Ph.D. Fifth Edition, thoroughly Revised. Philadelphia and New York: Lea & Febiger, 1924. (Pp. xv + 393, with 249 engravings and 3 plates in colours.) Price $3.75 net.

Living Organisms. An Account of their Origin and Evolution. By Edwin S. Goodrich, F.R.S., Linacre Professor of Zoology and Comparative Anatomy in the University of Oxford. Oxford: at the Clarendon Press, 1924. (Pp. 200, with 59 figures.) Price 6*s.* net.

Bibliographia Genetica onder Redactie van Dr. J. P. Lotsy en Dr. H. N. Koviman. Deel. I. 's-Gravenhage. Martinus Nijhoff, 1925. (Pp. iv + 461.) Price fl. 25.

The Mentality of Apes. By Wolfgang Kohler, Professor of Philosophy in the University of Berlin. Translated from the Second Revised Edition by Ella Winter, B.Sc. London: Kegan Paul, Trench, Trübner & Co., 68 Carter Lane, E.C. 1925. (Pp. viii + 342, with 9 plates and 19 figures.) Price 16*s*. net.

The Races of Man and their Distribution. By A. C. Haddon, Sc.D., F.R.S., Reader in Ethnology in the University of Cambridge. Cambridge: at the University Press, 1924. (Pp. viii + 184, with 10 plates.) Price 6*s*. net.

Fossil Man in Spain. By Hugo Obermaier, Professor of Prehistoric Archæology at the University of Madrid. With an Introduction by Henry Fairfield Osborn, Vice-President of the Hispanic Society of America. New Haven: Yale University Press. London: Oxford University Press, 1924. (Pp. xxviii + 405, with 158 illustrations.) Price 28*s*. net.

The Antiquity of Man. By Sir Arthur Keith, M.D., D.Sc., F.R.C.S., LL.D., F.R.S. New and Enlarged Edition, completely Revised and Reset. In two Volumes. London: Williams & Norgate, 14 Henrietta Street, W.C.2, 1925. (Pp. xxii + 753, with 266 illustrations.) Price 25*s*. net.

Lord Lister. By Sir Rickman John Godlee, K.C.V.O., M.S., F.R.C.S. Third Edition, Revised. Oxford: at the Clarendon Press, 1924. (Pp. xvi + 686.) Price 42*s*. net.

The Debt of Science to Medicine, being the Harveian Oration delivered before the Royal College of Physicians of London on St. Luke's Day, 1924. By Archibald E. Garrod, K.C.M.G., D.M., LL.D., F.R.S., Regius Professor of Medicine in the University of Oxford. Oxford: at the Clarendon Press, 1924. (Pp. 30.) Price 2*s*. net.

Basal Metabolism in Health and Disease. By Eugene F. Dubois, M.D. Philadelphia and New York: Lea & Febiger, 1924. (Pp. viii + 372, with 79 engravings.) Price 4.75$

Organic Medicaments and their Preparation. By Ernest Fourneau. Authorised Translation by W. A. Silvester, M.Sc. With Prefaces to the French Edition by Emile Roux, and to the English Translation by George Barger. London: J. & A. Churchill, 7 Great Marlborough Street, 1925. (Pp. x + 262, with 22 illustrations.) Price 15*s*. net.

The Medical Sciences in the German Universities. A Study in the History of Civilisation. Translated from the German of Theodor Cillroth, with an Introduction by William H. Welch. New York: The Macmillan Co., 1924. (Pp. xiii + 292.)

The Action and Uses in Medicine of Digitalis and its Allies. By Arthur R. Cushny, M.A., M.D., LL.D., F.R.S., Professor of Pharmacology in the University of Edinburgh. London: Longmans, Green & Co., 39 Paternoster Row, E.C.4, 1925. (Pp. xi + 303, with 77 figures.) Price 18*s*. net.

Louis Pasteur. By E. J. Holmes, Ph.D., Professor of Zoology in the University of California. London: Chapman & Hall, 11 Henrietta Street, W.C.2. (Pp. xi + 246, with 14 illustrations.) Price 7*s*. 6*d*. net.

The Chemical Aspects of Immunity. By H. Gideon Wells, Ph.D., M.D., Professor of Pathology, University of Chicago. New York: The Chemical Catalog Co., 1925. (Pp. 254.) Price 4$

Conferénces Scientifiques. Par Albert Turpain, Professeur à la Faculté des Sciences de L'Université de Poitiers. Premier Fascicule. Le Nouveau Domaine du L'Electricité. (Pp. x + 60.) Deuxième Fascicule. L'Éclairage et le Chauffage électrique. (Pp. x + 64.) Troisième Fascicule. L'Air Liquide. (Pp. x + 60.) Quatrième Fascicule. De la presse à bras à la linotype et à l'electrotypographe. (Pp. x + 98.) Cinquième Fascicule. Le Cinématographe. (Pp. xii + 84.) Price 5 frs. each.

Electrical Measuring Instruments. Part II. Induction Instruments, Supply Meters and Auxiliary Apparatus. By C. V. Drysdale, O.B.E., D.Sc., M.I.E.E., and A. C. Jolley, A.M.I.E.E. London: Ernest Benn, 8 Bouverie Street, E.C.4, 1924. (Pp. 475, with 437 illustrations.) Price 55*s*. net.

Crushing and Grinding Machinery. By Harland Seymour, Consulting Chemical Engineer. London: Ernest Benn, 8 Bouverie Street, E.C.4, 1924. (Pp. ix + 142, with 21 figures.) Price 6*s*. net.

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The Screening and Grading of Materials. By J. E. Lister, A.M.I.Mech.E. London: Ernest Benn, 8 Bouverie Street, E.C.4, 1924. (Pp. xii + 141.) Price 6*s*. net.

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